SCIENTIFIC MEMOIRS,

SELECTED FROM

THE TRANSACTIONS OF

FOREIGN ACADEMIES OF SCIENCE
AND LEARNED SOCIETIES,

AND FROM

FOREIGN JOURNALS.

EDITED BY

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VOL. I.

LONDON:

PRINTED BY RICHARD AND JOHN E. TAYLOR,
RED LION COURT, FLEET STREET.

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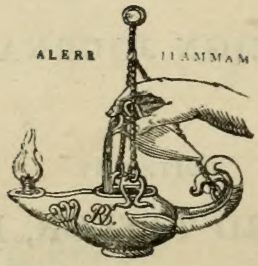
1837.


ALERE FLAMMAM

PREFACE.

IN the publication of the four parts which complete the present volume of the Scientific Memoirs, I have ventured to make the experiment how far I might be able to succeed in supplying an auxiliary which, as was stated in the Advertisement prefixt to the First Part, appeared to be much needed for the progress and advancement of science in this country.

My own conviction of the utility of such a work has been much strengthened since I have been engaged in it, both by the importance of the materials that present themselves, and by the expressed opinions of persons most competent to judge. How great indeed the disadvantage must be, under which those are placed who are engaged in any branch of scientific inquiry, from being uninformed as to what is doing, or has been done, by our active and laborious neighbours on the Continent, must be obvious to every one: and the cases are numerous to which the remark of Lenz, p. 312, speaking of Ohm's theory of the galvanic battery, will apply,—that, although given to the world several years ago, yet, "being only published in the *German* language, it is unknown both in France and in England."

With regard to the execution of the work, I must submit it to the candid judgement of those who are aware of the difficulty of the task,—having availed myself of the valuable suggestions with which I have been favoured in the selection of memoirs, and of the best assistance within my reach for their translation. I shall be satisfied if what has been done should render the present volume useful to science, and if what there is still to do should induce the public to enable me to continue the work. Hitherto, as I can hardly yet boast of the sale of 250 copies, I am very far from having been repaid the cost of publication, to say nothing of the care and labour which have been required: nor could I be expected, having now finished a volume, which, from the nature of its materials, may be considered a complete work in itself, to proceed further unaided, until I have ascertained whether I may calculate upon adequate support. This, however, I cannot yet think it improbable that I shall obtain, when the plan and contents of the present volume shall have become better known ; and with this view, I shall gladly receive the names of those who may be disposed to uphold the work by purchasing what has been published, and forming the list of my future subscribers.

I should hardly have been disposed to persevere further, had it not been for repeated expressions of strong interest in the success of the work, which have reached me from persons of the highest scientific eminence; among whom I may perhaps without impropriety mention the names of Ivory, of Babbage, Powell, Forbes, Lloyd, Challis, Owen, Wheatstone, Phillips, Talbot, Hamilton, Faraday, and others in this country, and of Hare, Henry, and Bache in the United States. To several of them I have been indebted for very important suggestions; and to Professor Wheatstone especially, for his valuable contributions.

From the kind assistance of men of science, and from an increased acquaintance with the sources whence the best materials are to be derived*, I think I may fairly hold out to the public the prospect of some considerable improvements in the work: and I shall be thankful for any suggestion for this purpose. I may perhaps give the titles or early notices of such foreign scientific papers as shall not be adopted for immediate translation: and as our first volume may be said to have cleared off some arrears, we may now come nearer to the present time, and endeavour to supply what is of the latest date and of intrinsic value.

However, this, as I have already stated, must depend upon my prospect of future support, and the success of the present volume ;—and, glad to have finished my humble but laborious task in completing it, I shall be able at my leisure to decide as to the future.

RICHARD TAYLOR.

Red Lion Court,
June 29, 1837.

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[*] Arrangements have been made for obtaining such as may appear in the Swedish, Dutch, and Italian languages.

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ERRATA.

Page 156, line 4, *for* 8·35. *read* 5·35
— 158, line 37, *insert a comma after* Calcium
— 160, line 41, *for* 0·350 carbon *read* 0·350 carbonic acid
— —, line 43, *for* 0·450 carbon *read* 0·450 carbonic acid
— —, line 43, *for* 0·0759 *read* 0·075 water
— 162, line 3, *for* sesquichromate *read* sesquichloride
— 164, line 6, *for* them *read* bases
— —, line 22, *for* ·754 *read* 0·754
— —, line 34, *after* stated that *insert* from
— —, line 34, *omit* contained
— —, line 35, *after* chloride of spiroil *insert* more obtained
— —, line 37, *for* 11·56 *read* 111·56
— 165, line 18, *for* 0·510 water *read* 0·119 water
— 470, line 5, *for* vol. xvi. *read* vol. lxvi
— —, four lines from bottom, *for* (Reaum.) *read* (Centigr.)
— 471, last line, *for* precipitated *read* before-mentioned
— 472, line 11, *for* in the same manner, *read* of the same kind
— —, line 33, *for* discoloured *read* decoloured
— —, line 41, *for* — 15° *read* 15°
— 473, two lines from bottom, *for* give them *read* give it
— 474, three lines from bottom, *for* of *read* and
— 475, line 28, *for* Ka^2 *read* Ka_1
— 476, line 6, *for* sulphur *read* sulphite

SCIENTIFIC MEMOIRS.

VOL. I.—PART I.

ARTICLE I.

Memoir on the Free Transmission of Radiant Heat through different Solid and Liquid Bodies; presented to the Royal Academy of Sciences of Paris, on the 4th of February, 1833, *by* M. MELLONI.

From the *Annales de Chimie et de Physique,* t. LIII. p. 1.

MARIOTTE was the first, so far as I am aware, who attempted to appretiate the action of diaphanous substances in transmitting or intercepting the calorific rays which emanate from terrestrial sources. After having observed that solar heat concentrated at the focus of a metallic mirror, suffered no sensible diminution of intensity by being made to pass through a glass plate, he took and placed his apparatus before the fire of a stove, and found, that at the distance of five or six feet the temperature of the reflected image at the focus, when the rays were allowed to meet there without impediment, was such as the hand could not bear; but that when the plate of glass was interposed there was no longer any sensible heat, although the image had lost none of its brilliancy. Whence he concluded that none [*], or *certainly but a very small portion,* of the heat of terrestrial fire passes through glass.

About a century after Mariotte's time, the same experiment was repeated by Scheele, who, instead of imitating the cautious reserve of his predecessor, asserted that from the moment when the glass was interposed there was no longer any heat whatever at the focus of the mirror [†].

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[*] Mariotte, *Traité de la Nature des Couleurs;* Paris, 1686, part 2, at the end of the Introduction.

[†] Scheele, *Traité de l'Air et du Feu ;* Paris, 1781, § 56.—The original work of Scheele was published in 1777. Mariotte died in 1684.

Pictet, however, corrected the mistake by means of the apparatus known by the name of conjugate mirrors. A very transparent square of glass was placed between a thermometer and the heat of a lighted candle concentrated by the apparatus; the mercury in some moments rose several degrees; there was a perceptible elevation of temperature also when the candle was removed and a small jar filled with boiling water put in its place *.

Some years later Herschel undertook a very extensive series of experiments on the same subject. They are described in the volume of the Philosophical Transactions for 1800. The author employs no artifice to increase the action of the rays of heat, and contents himself with the direct measurement of their effect by placing the thermometer at a very short distance from the diaphanous body.

But doubts were started as to the conclusions drawn from these different results. It was objected that part of the radiant heat was first stopped at the nearer surface of the glass, that it was gradually accumulated there and afterwards propagated from layer to layer, until it reached the further surface whence it began again to radiate on the thermometer. It was maintained even that nearly the whole of the effect was produced by this propagation. In short, some went so far as to deny altogether that the heat emitted by terrestrial bodies can be freely transmitted through any other diaphanous substance than atmospheric air.

M. Prevost, by means of a very ingenious contrivance, demonstrated the erroneousness of this opinion. Having attached to the pipe of a fountain a spout consisting of two parallel plates, he obtained a strip of water about a quarter of a line in thickness. On one side of this he placed an air thermometer and on the other a lighted candle or a hot iron. The thermometer rose, almost always, some fraction of a degree †. Now it is quite evident that, in this case, a successive propagation through the several layers of the screen, which was in a state of perpetual change, could not take place. It was admitted, therefore, that other diaphanous media besides atmospheric air *sometimes* transmit the rays of heat as instantaneously as they *always* transmit those of light.

M. Prevost's process could not however be applied to solid bodies. It was therefore impossible to determine, by means of it, whether caloric was immediately transmitted through screens of glass. Delaroche completely solved this problem by employing a method invented by Maycock ‡.

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* Pictet, *Essai sur le Feu*, § 52 et seq.

† *Journal de Physique, de Chimie, d'Histoire Naturelle et des Arts*, par M. Delametherie, 1811.—P. Prevost, *Mémoire sur la Transmission du Calorique à travers l'Eau et d'autres Substances*, § 42 et 43.

‡ Nicholson, A Journal of Natural Philosophy, Chemistry and the Arts, vol. xxvi. May and June 1810.—J. D. Maycock, Remarks on Professor Leslie's Doctrine of Radiant Heat.

The method consists in observing the thermometer as in the preceding cases; that is, when the caloric rays fall upon it after having passed through the plate of glass. We thus obtain a complex measure of the effects produced by immediate transmission and by that conducting power of the layers to which we have given the name of *successive propagation.* If we know the value of either of these, we have that of the other. Now it is easy to determine the influence of the conducting power by repeating the experiment after having blackened with Indian ink that surface of the plate which is turned towards the calorific source. In this case, the immediate radiation being intercepted, it is clear that the elevation of the temperature at the other side must be attributed only to the conducting power of the layers. Should the elevation be now found less than it was at first, it will be a decisive proof of immediate transmission. And such was the fact in almost all the experiments of Delaroche; I say *almost all,* because it was found that the quantity of heat freely transmitted varied with the temperatures of the source. For temperatures lower than that of boiling water it was nothing, and when an Argand lamp* was employed, it was found to be more than half of the whole quantity.

No doubt can be raised as to the truth of this beautiful discovery of Delaroche; and yet the method which he has employed to measure the quantities of heat freely transmitted is by no means exact, especially in respect to high temperatures. In order to understand this seeming paradox two things are to be observed; 1st, the difference produced by change of surface between the two quantities of heat which penetrate the glass by reason of its conducting power; 2nd, the difference produced between those two quantities by the total or partial interception of the calorific rays.

It is fully proved by the experiments of Leslie and others, that glass, when blackened with Indian ink, absorbs all the rays of heat, though, in its natural state, it reflects a certain number of them. The quantity of heat which penetrates the screen will therefore be greater in the former than in the latter case. However, as polished glass reflects but a very small portion of caloric rays, the error arising from a difference in the state of the surface will be reduced to a very inconsiderable quantity and may be safely disregarded. But the case is different when we examine the error produced by the total or partial interception of the caloric radiation. In some of the experiments of Delaroche one half, at least, of the incident rays immediately passed through the screen. Thus it was evident that it was the other only which was stopped at the first surface of the glass. The effect of conduction must therefore be limited to this latter half. But as the screen, when blackened, stops the whole radiation,

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* *Journal de Physique, &c.,* par Delametherie, 1812.—Delaroche, *Observations sur le Calorique rayonnant.*

it is then exposed to a heat twice as strong, and therefore exhibits a far greater effect of conduction. Hence it follows that when we deduct from the observation furnished by the transparent glass the observation furnished by the glass blackened, the result obtained will be lower than the true temperature of the rays transmitted freely. But the error will not be the same in all cases. Being of no account when boiling water is employed, it will increase in proportion as the temperature of the source is raised. The measures of the free radiations which suffer the greatest diminution will be those furnished by the highest temperatures. Hence it is evident that this latter cause of error in the measure of the immediate irradiation, instead of invalidating the law of Delaroche, serves only to give it greater certainty. We are therefore justified in saying, as we have said, that the want of exactness in the method has no influence whatsoever on the truth of the law which it has served to establish.

To Delaroche we are also indebted for a discovery, no less important than the foregoing, relative to the amount of loss sustained by the same rays of heat in passing successively through two squares of glass. But I abstain, for the present, from entering into any detail on this subject, as I shall have occasion to speak of it hereafter *.

None of those whose labours we have been thus briefly noticing has thought of making an exact comparison between the transmissions of caloric rays through screens of different kinds; and, if we except the experiments of M. Prevost and those of Herschel, from which no consequence can be deduced, all the others were confined to the single purpose of ascertaining the law of transmission through glass only. Neither has sufficient attention been given to the influence of the state of the

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* I must not omit to mention that, notwithstanding the results obtained by Delaroche, some most eminent philosophers (and of these it will be sufficient to name Laplace and Brewster) continued to deny the immediate transmission of heat through transparent solid bodies. Their principal objection was founded on an experiment of that author, from which it was inferred that a thick glass intercepted a greater quantity of radiant heat than a thin glass, though the former was much more transparent. It was insisted that this circumstance proved the presence and action of heat successively propagated from one surface to the other, and every elevation of temperature observed on the other side of the screen was assigned to the conductible caloric. This opinion can no longer be maintained in defiance of the results furnished by the application of the thermomultiplier to this species of phænomena. It will be seen, further, that the calorific action through a transparent layer is instantaneous, and that the time necessary for the instrument to mark its total effect is the same, whatever be the quality or thickness of the screens. Let the direct rays from an unvarying source of heat be received on the thermoelectric pile; let them be first made to pass through any diaphanous screen of one hundred millimetres in thickness: the index of the galvanometer sets itself in motion *from the instant when the communications are established*, and stops after having described an arc of greater or less extent in *an unvarying interval*, which, with my apparatus, I find to be ninety seconds.

surface, or that of the thickness of the layers and their internal structure on the quantities of heat which freely pass through them. I have endeavoured to supply these different omissions, but the undertaking has proved too vast for me, and several parts of it are therefore incomplete. I hope however that I shall be able hereafter to return to these, and to treat them in a manner more satisfactory.

In the mean time I present to the Academy the results of my first researches disposed in two memoirs. That which I offer at present contains an account of the method pursued in the measurement of calorific transmission and the application of the method in the case of an unvarying source acting on bodies of different kinds. In the second I shall explain the facts connected with the succession of the screens and the variation of the sources.

General Considerations on the Free Transmission of Caloric through Bodies, and the Manner of Measuring it by means of the Thermo-multiplier.

We have already observed that a diaphanous screen placed at a certain distance from a calorific source stops a portion of the rays which strike its first surface, while the rest pass freely through. We have remarked besides that after a certain time the heat stopped at the anterior surface, and accumulated there by successive radiations, passes on from layer to layer till it reaches the other surface, whence it begins to radiate anew; and that this radiation mingling with the heat which passes through the screen by immediate transmission, prevents its being measured exactly.

When the screens are liquid, the influence of the conducting power of the layers may always be destroyed if we incessantly renew the matter of the screen by means analogous to the strip of water employed by M. Prevost. But it would be always very difficult, and often impossible, to apply this artifice to solid bodies and even to such liquids as can be obtained only in small quantities. In order therefore to attain the same end in a general manner, and to render the experiments in some degree independent of conduction, other means must be employed.

If we consider with due attention the manner in which the second surface of the interposed plate is heated, and the radiation which results from it, we shall see that the latter possesses properties very different from those that belong to the caloric which is freely transmitted. In order to be satisfied of this, we have only to observe that its action changes with the change of distance between the screen and the source; a thing which does not happen, even in the slightest degree, to those rays that are transmitted freely. In fact, it is with the caloric transmitted *immediately*, as it is with light.

If between the flame of a candle and the eye we interpose a plate of glass or any other substance more or less transparent, we find the diminution of the intensity of the light always the same, however the distance between the plate and the candle may vary. The effect produced by distance on the freely transmitted caloric is exactly similar; and if at a certain distance from the active source there be a thermoscopic apparatus sensible to this portion of heat, the apparatus will always give the same indication, whether the screen be laid close to the source or to the thermoscope.

But it is clear that it must happen quite otherwise to the conductible caloric; for this portion of the heat, when it has reached the further surface of the screen, leaves it in the form of diverging rays which become weaker in proportion to the distance. In other words, the further surface of the screen being heated becomes a new calorific source whose intensity of radiation must decrease as the distance increases.

We possess, therefore, a very simple contrivance for destroying the influence of conduction, if we keep the action of the free radiation intact. This contrivance consists in removing the screen so far from the thermoscope that the radiation of its own heat may, on account of its extreme feebleness, be totally disregarded.

There are, however, some precautions to be taken; for in proportion as the distance between the screen and the thermoscope is increased, the distance between the source and the screen is diminished. The latter is therefore more heated, and radiates with greater force upon the instrument. It is easy to show by calculation that we always gain; that is, that we always weaken the conductible caloric more and more by removing the screen from the thermoscope, until we have placed it midway between the thermoscope and the source*. Let us, therefore, put the screen in this position (which is the most favourable of all), and we shall see that its heat has then no appretiable influence on the re-

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* Let a be the distance from the source to the thermoscope, x the distance from the thermoscope to the screen, i the calorific intensity of the source, we shall have $\frac{i}{(a-x)^2}$ as the expression for the radiation which strikes the anterior surface of the screen. This quantity will become $\frac{c\,i}{(a-x)^2}$ at the further surface, c being a constant quantity depending on the conducting power of the matter of the screen. In fine, the radiation of the further surface on the thermoscope will be expressed by $\frac{c\,i}{x^2(a-x)^2}$: its minimum (y) is to be determined. Now, by differentiating we obtain $\frac{dy}{dx}=\frac{2ci(2x-a)}{x^3(a-x)^3}$; the equation which gives the quantity will then be $2x-a=o$, whence $x=\frac{a}{2}$.

sults obtained by means of the thermomultiplier*, and a source whose radiation is much weakened by distance.

The apparatus is disposed in the following manner. A thermoelectric pile of thirty pairs is closed at one end and enveloped, at the other, in a small tube blackened inside to prevent reflection. At a certain distance there is placed a large metallic diaphragm, with an aperture at the centre equal to the section of the pile. On the other side, in the same line, there is a lighted lamp, which is brought more or less close, until the needle which serves as the index of the galvanometer, marks an elevation of 30°. The radiation is afterwards intercepted by a screen of polished metal placed between the lamp and the diaphragm, and the needle returns to zero. Then there is placed on the other side of the diaphragm a stand, with a plate of glass fixed on it, and the whole apparatus is moved gently until it is brought midway between the pile and the calorific source.

This being done, the opake screen is removed; the rays passing through the glass fall on the pile, and immediately cause the galvanometer to move. In 5^s or 6^s it is driven through an arc of nearly 21°·5, but it afterwards returns nearly to zero, oscillates in an arc of greater or less extent, and at last settles definitively at 21°. This last deviation decidedly marks the whole effect; for it is useless to continue the experiment for 15^s or 20^s. There is no longer any perceptible movement.

The time which the needle takes to attain its position of steady equilibrium is a minute and a half†. When the experiment is repeated

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* For the description of this instrument see the number of the *Annales de Chimie* for October 1831.

† Although the velocity with which radiant heat is propagated is unknown, we are nevertheless pretty certain, since the experiments of Saussure and Pictet, that this agent traverses spaces of from fifty to sixty feet in a time altogether inappretiable. It might be asked, therefore, why does not our apparatus instantaneously indicate the presence and the intensity of the rays emitted by the source? To this I answer, 1st, that the index of the galvanometer deviates at the very instant when the calorific communications are established, and we have just seen that in five or six seconds it describes almost the whole arc of deviation. If a few seconds more are required to mark the entire action steadily, it is because the great conducting power of the bismuth and the antimony, and the great powers of absorption and emission belonging to their blackened surfaces, render the lapse of a certain interval necessary, in order that a balance may take place between the rays which enter the pile and those which leave it or are extinguished within its interior. But the time required for the definitive equilibrium is much greater when common thermometers are used. If, for instance, one of Rumford's most delicate thermoscopes, having the ball blackened, and a metallic cover perforated on the side towards the source of heat, be submitted to the action of calorific radiation, it will be found that the time requisite to mark the whole effect is four or five times more than that required by the thermomultiplier. This delay is the consequence of the obstructions encountered by the conductible heat in its passage through the glass, and in its

with other plates of glass, or of any transparent substance whatsoever, possessing different degrees of thickness, from the hundredth part of a line to five or six inches, the galvanometer exhibits deviations greater or less than 21°; but the time requisite to attain the equilibrium is in all cases the same. In short, if we mark the time which the needle takes to arrive at 30°, we shall find it to be one minute and a half.

The invariability of this time, in such a variety of circumstances, affords the most decisive evidence that the deviations of the galvanometer are exclusively due to that portion of heat which reaches the pile by immediate transmission. Whence it follows, that in the arrangement we have adopted, the heat of the transparent body has no appretiable influence on the instrument.

But a direct proof of this proposition may be obtained by operating on opake screens.

I take a plate of glass a millimetre in thickness. I blacken it on one

uniform distribution over all the points of the mass of air within,—a distribution which will necessarily take place, because of the fluidity of the thermoscopic body.

Another inconvenience produced by the interposition of the glass, and from which the thermomultiplier is free, is the lapse of a perceptible interval between the commencement of the action and its manifestation on the instrument; for there is always some time required, in order that the heat may pass from one surface to the other. I speak not here of the caloric which might pass to the air by free transmission through the diaphanous sides of the cover; for when we have to estimate the intensities of caloric rays by means of thermoscopes, we cannot dispense with the blackening of the glass. So necessary indeed is this, that in order to make sure of the opacity of the glass, it must be overlaid with several coats of colouring matter. Otherwise, a portion of the rays would freely pass through the mass of air contained in the ball without dilating it.

Now, in the common thermoscopes, we *always* measure the radiation through an opake plate of glass. This plate, however thin, must offer a considerable resistance to the propagation of heat, because of the feebleness of its conducting power, and will therefore, as we have already observed, render the apparatus *insensible* during the first moments of action. Let it be observed, moreover, that the more we endeavour to increase the sensibility of the thermoscope by enlarging the dimensions of the balls, the more we diminish the promptitude of its indications; for the increase of volume is proportionally greater than that of the part of the surface turned towards the source, and the mass of air within is increased in a proportionally greater degree than those points of the glass which can communicate to it the heat they have acquired. Hence arises a greater difficulty in attaining the moment of equal temperature in all the points of the fluid mass, and, of course, the necessity of a longer time to mark the entire effect.

In fine, the thermoscopes are utterly useless when it is required to measure caloric rays that are very feeble, and distributed according to given lines, or forming sheaves of small dimension. In fact, it would be necessary in this case to preserve the whole sensibility of the instrument by considerably reducing the size of the balls. But this is impossible.

Whoever takes the trouble to weigh these considerations duly, will not, I think, hesitate for a moment to prefer the thermomultiplier to every other thermoscopic apparatus in studying the subject of caloric radiation.

side, and put it in the place of the transparent plate, taking care to turn its blackened surface to the lamp. The needle remains stationary, although the caloric rays continually fall on the anterior surface. It will be found immoveable also, if we employ a plate of copper coated on both sides with black colouring matter, or a thin flake of wood, or even a sheet of paper. Thus, though we should suppose the screen to be diaphanous, exceedingly thin, an excellent conductor of caloric, and possessing great powers of absorption and emission, the utmost elevation of temperature that can be acquired during the experiment would not furnish rays sufficiently strong to move the index of the galvanometer.

One is surprised at first to see caloric rays capable of giving a deviation of 30° fail to produce any effect when they are absorbed by the screen, which must necessarily send its acquired heat upon the apparatus. But our surprise ceases when we reflect that this heat is sent equally in all directions by every point of the heated screen, and therefore that the portion of total radiation which reaches the apparatus is but a very small fraction.

We shall see hereafter, that the anterior surface of the pile does not measure six square centimetres. With these data, if we suppose even that the thirty degrees of heat are completely absorbed by the screen, and afterwards dispersed through space, we find that the quantity of the rays which reach the thermoscopic body does not amount to the six-hundredth part of the whole. But the galvanometer that I use is capable, at the most, of marking only the 150th part of the force which moves the needle to 30°. Thus, even though the instrument were capable of discovering the presence of a heat four times as feeble, there would be no perceptible action.

The experiments which I have been describing seem to me to leave no doubt whatsoever as to the truth of the proposition just now enunciated; namely, that in my mode of operating the deviation of the galvanometer proceeds entirely from the heat *instantaneously* transmitted through the screen. These proofs, though so conclusive to my mind, seem however not to have been equally convincing to others; for I have heard some persons say, "We grant that the deviation of 21° obtained through the screen does not arise from the caloric propagated by conduction from the anterior to the other surface, but it may be maintained that it is caused by a heat instantaneously *diffused*, in the same manner as light, over all the points of the glass." Before we admit such a mode of transmission, it seems to me that we ought to demonstrate its existence by some decisive experiment. But supposing it true, then we must also suppose one of these two things,—either that the molecules of the glass acquire from the action of the source such modifications that they themselves become so many calorific centres, and return to their natural state when the radiation is stopped; or that the heat, which is supposed to be diffused through the material points of the screen, is but common caloric obeying the known laws of equilibrium. In the first case we should be only attempting to explain the very cause of the transmission, and the hypothesis, true or false, does not at all invalidate the fact which we are desirous to establish. In the second case, this heat, when it has reached the interior of the body, must take some time to issue from it; besides, this time must vary with the thickness of the screen, and its powers of conduction and emission. But let us intercept the calorific communication in our apparatus; let us remove the diaphanous screen from its stand, and expose it for some moments to the free radiation of the lamp on the other side of the diaphragm: if the supposition be true, the internal molecules of the glass will instantaneously acquire some heat. In order to see whether this heat really exists, let us replace the screen on its stand before the pile, still leaving the calorific communication with the lamp intercepted. The further surface of the plate of glass will, according to the hypothesis, immediately begin to emit towards the pile that caloric which reaches it successively from within, and the index of the galvanometer must lose its equilibrium. But whatever be the nature or the thickness of the screen with which this experiment is performed, we never obtain the slightest indication of a movement in the magnetic needle. It is therefore completely demonstrated that the deviations of the galvanometer exhibited in the experiments made with the diaphanous screens are not to be attributed, in the least degree, either to the external or the internal heat of the screen itself, but solely and exclusively to free transmission. Thus, whenever, in consequence of the radiant heat of the source being made to fall on a screen, a deviation of the galvanometer is perceptible, we may rest assured that the *whole* of the effect produced is to be ascribed to the rays of heat immediately transmitted through it, in the same manner as luminous rays.

Before I conclude these preliminary considerations, it is necessary to remark, 1st, that galvanometers of very great sensibility, such as must be used for the thermomultiplier, do not directly indicate quantities less than half-degrees; 2ndly, that the ratios of the degrees of the galvanometer and the forces of deviation are unknown. But it is often useful to have the fractions below the half-degree, and in certain circumstances it is absolutely indispensable to know the ratios of the several degrees of calorific action which move the magnetic needles to different distances from their primitive position.

To find the fractions sought, we have only to take the means of a certain number of observations. As to the ratio of the deviations and the forces, it is difficult and, in the present state of the science, perhaps impossible to determine it generally. But electric piles, such as those employed in the construction of the thermomultiplier, furnish sufficiently simple means of solving the question in each particular case. Indeed there is nothing easier than to keep the index of the galvanometer at any degree of deviation. All that is required for this purpose is to place a lighted lamp at proper distance from either side of the thermoelectrical pile. To prevent the possibility of mistake on this point, let us suppose the axis of the pile to be perpendicular to the magnetic meridian, and the communications so fully established that, when the left or the right side of the pile is heated, a corresponding deviation will be exhibited by the galvanometer. Let there be now produced a sufficiently marked deviation by placing a lamp near enough at the same side. Let this deviation be 44°. After having brought the needle back to 0° by interposing a metallic screen, let us make it move to the 42nd degree of deviation on the left, by means of a second lamp placed on the other side. To bring the needle back again to the zero point of the scale, we have only to stop the radiation by means of a metallic screen, as before.

It is natural to ask what will be the effect now produced by the heat of both lamps being brought to bear simultaneously upon the opposite sides of the pile. The calorific effects will be partially destroyed, and the instrument will mark but their difference. If the same force were always required to make the needles describe arcs containing the same number of degrees, the index would stop at the second degree of deviation to the right; but we know that these effects continually increase to the right and to the left of zero. The difference of two degrees just now observed between the partial deviations of 44° and 42° was owing to the application of a force greater than what is required to make the index traverse the first two degrees of the scale. The position marked 2° will therefore be exceeded, and the more so in proportion as the first force is greater than the second, and the arc described will, when compared with the difference of the two deviations, immediately give the measure of the corresponding force. If, for instance, the needle stops at 8°, it will be inferred that the force required to make the needle pass from 42° to 44° is four times greater than that required to make it pass from zero to 2°. This effect would be five times greater if the needle stopped at 10°, and so of the rest.

I shall not attempt to conceal the fact, that in this process the proportionality of the forces to the degrees in the arc employed as a comparative measure is tacitly assumed. But the assumption is fully justified by experience; for we find that in galvanometers whose astatic system has been brought to a high degree of perfection, the magnetic needles, through the whole extent of the arc comprised between zero and the twentieth degree nearly, describe arcs proportional to the action of the electric current to which they are subjected. To be convinced of this, it is by no means necessary to review in succession all the degrees that contribute to the formation of this arc. The application of our method to the angles of 20° and 10° will be quite sufficient. This being done, we shall find an equal quantity between their difference and the effect produced by the simultaneous action of the moving forces. In other words, let us produce a deviation of 20° to the right and one of 10° to the left: let us then simultaneously expose the two opposite faces of the pile to the two radiations which produce these galvanometric indications: the index will move to the right, and stop precisely at 10°. Hence we infer that the force necessary to make the needle describe the arc comprised between 10° and 20° is equal to the force required to make it pass over the first ten degrees of the scale. Thus the proportion of the degrees to the forces is perceptible as far as the 20th degree on each side of zero.

This fact seems opposed to the inference which might have been made in examining the nature of the galvanometric action; for, in the successive rotation of the astatic system, the poles of the magnetic needles depart from the mean line of the electric currents. The intensity of the repulsive forces, therefore, decreases in proportion as the angle of deviation increases. Whence we should conclude that the effort necessary to make the needles exceed a given arc should change as soon as the first degrees of the scale are passed. This would undoubtedly take place if all the electric currents lay in a vertical plane passing through the line marked 0°; but the circumvolutions of the metallic wire which is wound on the frame placed under the graduated circle are distributed to a certain extent on each side of this plain. In the galvanometer which I have employed in my experiments, they cover the two opposite arcs of 76°, the chords of which are perpendicular to the line marked 0°. Thus so long as the oscillations take place within certain limits there will always be electric currents situated on each side of the needles. Now when the intensity of these currents is extremely feeble, their sensible effect on the needles must cease at a very short distance. Let us suppose this distance to be 18° of the division of the galvanometer intended to show the degrees of electric action which cause the deviations to the right and left for the first 20° of the scale. These degrees of action must be extremely feeble in a very delicate galvanometer. If, during these oscillations, the system of the needles is confined within the two initial arcs of 20°, it is clear that it will always be subject to the same action, whatever may be the position in which it is placed; for there will always be near its plane a series of currents extending to 18° on each side, even when the system will occupy the extreme limits. The influence of the currents that are further distant will, according to our hypothesis, be nothing. As the moving force will therefore have a constant value, we shall have to consider only the modifications which the active part of this force is made to undergo by the different inclination of the needles to the direction of the currents; and these modifications are quite analogous to those which take place in the portion of gravity that acts on the pendulum in different arcs of oscillation.

Now the force necessary to make the pendulum vibrate from one inclination to the other, is proportional to the difference of the cosines of the angles which the two directions form with the vertical. Whence it is clear that it remains sensibly constant in the arcs that are not far removed from the line of rest. The same effect must therefore be produced in the galvanometer also; or, in other words, the force required in this apparatus to increase the deviation of the index by a degree will be constant near the line of zero, as is shown by experiment.

From what we have just said it will be easy to see that the relation between the degrees of the galvanometer and the forces which cause the deviations of the needles, must depend on the sensibility of the astatic system and the distribution of the wire on the frame*. It will vary, therefore, according to the construction of the instrument, but may be always determined by the method we have mentioned.

Experiment having shown that in my galvanometer the proportion of

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* In order to understand this clearly, it is sufficient to suppose a galvanometer in which the circumvolutions of the wire are more numerous towards the extremities than towards the central part. It is evident that under the action of such a system the forces which produce the deviations, instead of increasing or being merely proportional in the arcs near zero, must decrease as we approach the extremities of the frame, in order to increase afterwards when the index has passed these positions.

As to the influence of the sensibility of the astatic system, we shall be able to form a tolerably exact idea of it, if we imagine a galvanometer with the two needles possessing very different degrees of magnetism. Then the terrestrial globe will very powerfully affect both combined; and, in order to produce the least deviations, electric currents must be employed possessing much greater force than those required to produce small deviations in a more perfect astatic system. In the positions near zero, the electro-magnetic action produced by the most distant currents, that is, the action of the currents situated at the extremities of the frame, will possess an energy sufficient at least to overcome the resistance arising from the twisting of the suspension thread and the inertia of the astatic system. It will therefore always contribute to move the oscillating mass. Hence it is evident that if the needles are displaced in the slightest degree, the consequence will be a loss in the moving force; for if the system approaches a certain arc at a certain extremity, it recedes at the same time double the distance from the opposite extremity. Now we have already seen that, in delicate galvanometers, the moving force is constant when the angles are small; and we have assigned the cause of this fact upon the incontestible principle that, in small deviations of the instrument, the action of the currents situated towards the extremities of the frame must be disregarded, not indeed because they have no value, but because it becomes, in consequence of its distance, extremely feeble, and incapable of surmounting the obstacles opposed to it by the torsion of the silk thread and the inertia of the needles.

the degrees to the forces was perceptible as far as the twentieth degree of the scale, I have attentively observed the passage of the index through every 4°, by commencing with this position and continuing my observations as far as the forty-fourth degree. There I stopped; for my experiments on calorific transmission were to be confined to radiations considerably weakened by distance.

The arcs passed once in virtue of the forces acting on the system of the needles at different points of their course are in the following ratios to one another:

The arc comprised between

20° and 24°	is equivalent to	5°·12, commencing at zero.
24 — 28	———	6·44
28 — 32	———	8·00
32 — 36	———	9·92
36 — 40	———	12·44
40 — 44	———	19·04

Each number in the third column represents the mean of eight observations, which agreed with one another as exactly as could be expected from the nature of the instrument. Often equal, sometimes differing by 0°·5, their greatest disagreement never exceeded 1°. A better proof cannot be given of the exactness of the method.

The linear construction of these results, which gives a very regular curve convex towards the axis of the *x*es, has enabled me to obtain the values of the intermediate forces, degree by degree, from 20° to 45°. By connecting them with the fundamental observations, I have formed the following table of the intensities:

Degrees.	Forces.	Degrees.	Forces.	Degrees.	Forces.
20°	20·0	29°	33·4	38°	55·4
21	21·1	30	35·3	39	58·5
22	22·3	31	37·4	40	61·9
23	23·7	32	39·6	41	65·5
24	25·1	33	41·8	42	69·3
25	26·6	34	44·1	43	73·4
26	28·2	35	46·7	44	78·0
27	29·9	36	49·5	45	83·2
28	31·6	37	52·4		

The use of a table requires no explanations. All the forces are referred to that which makes the index describe the first degree of the scale. The values corresponding with the first twenty degrees are not exhibited in it; for through the whole extent of this arc the number representing the force is equal to the number of degrees contained in the arc passed over by the index. Thus, for instance, when we look for the forces which produce the deviations 35° and 16°, the first (46·7) will be found in the table, but the second, being under 20°, will have the same value as the arc; that is to say, 16. When we want to find the forces which correspond to fractions of a degree, we have only to ascertain the proportional part of the degree in question; for, in the interval between one degree and another, the curve visibly coincides with the tangent. If, for example, we wish to know the force that corresponds to the deviation 31°·7, it will be sufficient to take at first the difference between 37·4 and 39·6 (the intensities of the forces belonging to 31° and 32°); this difference being 2·2, we shall find the value (x) of the force corresponding to seven tenths of the degree contained between 30° and 32° by this proportion,

$$1^\circ : 0^\circ{\cdot}7 :: 2{\cdot}2 : x = 1{\cdot}5.$$

Adding this to the number 37·4, which represents the force corresponding to 31°, we shall have 38·9 as the value sought.

Of the Polish, the Thickness, and the Nature of the Screens.

The suggestions which we have offered as to the manner of measuring the quantity of caloric instantaneously transmitted by diaphanous bodies, and as to the precautions to be taken during the experiments, leave us scarcely anything more to say on this subject. Nevertheless it may not be amiss to mention some particulars relative to the construction of the apparatus before we proceed to the exposition of the results.

The pile employed in these researches is of the form of a quadrangular prism; its two ends are plane surfaces, each measuring 4·24 centimetres; it consists of 27 pairs and a half, or 5 elements of bismuth and antimony, 32 millimetres long, 2·5 broad, and 1 in thickness. It was not without considerable difficulty that we succeeded in combining and soldering together these minute bars. The facility with which liquid antimony oxidizes, the difference between its fusibility and that of the bismuth, and the extreme fragility of the two metals, presented so many obstacles, that it cost many an effort to overcome them. But a pile of very small dimensions was indispensable in the investigation of the laws of immediate transmission through rare liquids and crystallized solids. This was, therefore, to be obtained, or the experiments to be abandoned. By this conviction we have been induced to persevere in spite of repeated disappointments, and by redoubling our patience have at last succeeded.

The electric pile is passed into a ring formed of a thin square flake of copper internally lined with pasteboard and having a screw which serves to fix it on the stand, so that the axis naturally takes that horizontal position which it is to keep during the greatest part of the experiments. To each side of the ring there is fitted a tube of six centimetres in length, blackened on the inside; and at a certain distance from the mouths of these tubes are placed the stands destined to receive the screens. In strictness, a single tube and a single stand would be sufficient, and one of the sides of the pile might be closed by means of a small metallic cover; but, when we have to operate on bodies differing in quality and thickness, it often happens that they differ in temperature not only from one another but from the pile also. Then if we place but one screen before the apparatus, the calorific actions at the two sides are unequal, the index of the galvanometer moves away from zero, and we must wait for some time until the equilibrium of the temperature is established and the index returns to its original position.

Now this inconvenience cannot occur when the pile is furnished with two tubes and two stands; for, by placing before each side of it a plate of the same quality and thickness, it is clear that, if care be previously taken to place the two in the same circumstances, they will have the same temperature, and will consequently emit the same quantity of heat on the two sides of the pile. The index of the galvanometer will remain stationary, whatever may be the difference of temperature between the plates and the thermoscopic body, and we may therefore immediately proceed with the experiments. Hence, if we would save time, we should always have a pair of screens of each sort; and, as we have just observed, put both sides of the pile in the same state.

In order to ascertain the influence exercised on free transmission, by the different circumstances relating to the surface, the volume, and the composition of the screens, we must procure a constant source of heat. For this purpose, there is nothing better than a good lamp with a double current of air and a constant level. When this apparatus is well prepared and filled with oil freed from mucilage, by means of sulphuric acid, we obtain a flame which maintains an invariable temperature for more than two hours. Of this I have been able to satisfy myself by means of the thermomultiplier. But in order to have things in this preparatory state, we must wait some moments until the pipe, the oil, and the glass funnel of the lamp shall have attained a maximum of temperature. This time, which varies a little with the construction of the lamp, is about ten or fifteen minutes.

There may be some objections raised against the employment of an Argand lamp as a calorific source. It will be said, perhaps, that in this lamp the heat acts only through the glass funnel; that the funnel itself becomes heated, and mixes its rays of nonluminous heat with the luminous caloric of the flame; and lastly, that such a source of heat is neither uniform nor separated from the agent which usually accompanies it in high temperatures.

But I wish it to be particularly observed, that the only thing about which we are interested at present is, to know whether the state of the surface, the colour, and the internal structure of a body, as well as its chemical composition, have any influence whatever in the quantity of heat which it transmits immediately; and that, in this point of view, the origin and the qualities of the caloric rays become objects of perfect indifference; for it is enough for our purpose that the rays be invariable and identical in all the circumstances in which they are employed. Now this actually is the case with the rays issuing from the well supported flame of a Quinquet lamp placed at a fixed distance.

When we shall have found the ratios of the quantities of heat transmitted by screens of different kinds under the influence of a constant source, then, agreeably to what we have stated in the introduction, we shall examine the changes which those ratios undergo in consequence of the variation of the sources.

All our experiments of comparison have been made with the same calorific radiation. Previously to the commencement of each series the rays were allowed to fall directly on the pile, and the distance of the lamp was modified until the needle of the galvanometer fixed itself at 30° of the scale.

We have remarked in the preliminary considerations, that all the external parts of the thermoscope are sheltered from the caloric rays by means of a large screen of polished metal, having in its central part a hole to correspond with the opening of the pile turned towards the lamp.

In order to establish or to intercept the communication between this aperture and the source of heat securely and commodiously, we make use of a moveable copper screen, consisting of two or three parallel plates fixed on the same support. The side of the pile opposite to the lamp may also be closed and opened by means of a screen altogether similar, and for the following purpose:

When, after having observed the effect of any radiation whatsoever, we intercept the action of the source, we must wait until that face of the pile on which the rays of heat are darted has been restored to its natural state before we make a second observation. Now it appears that the heat emitted by the flame penetrates the apparatus with greater ease than it issues from it, because of its natural tendency to an equilibrium. At least the experiment shows that the time requisite to produce the deviation is to that in which the needle recovers its original position nearly as one to five; for the latter is from 7^s to 8^s, and we have seen that the whole deviation is produced in a minute and a half. Whatever be the cause of this difference between the time required for heating and that required for cooling, we must always allow 8^s to elapse after one experiment before we proceed to another, if we confine ourselves to the placing of the first moveable screen before the radiating source. But let the opposite side of the pile be opened and a lighted candle brought close to the corresponding face: it is evident that if the candle be held for some minutes at a suitable distance, and the communication then intercepted, the needle will be forced back to zero in an interval of time less than 8^{s}. These operations would be impossible if the side of the pile opposite to the lamp were hermetically closed. The second moveable screen serves then to abridge the duration of the experiments. It is particularly useful when the calorific action has been very powerful or considerably prolonged, which sometimes happens in the first attempts at adjustment. During these, the portions of heat penetrate the pile to a great depth, and cannot return until a considerable time has elapsed. Before these simple means of correction had occurred to me, the difficulty of restoring the equilibrium of the two extremes of the pile, as well as that which I experienced in respect to 'he different temperatures of the screens and the apparatus, often obliged me to stand still for fifteen or twenty minutes between two consecutive experiments.

When any object of research requires numerous experiments, we should endeavour from the very outset to avail ourselves of all that contributes to make them more expeditious; for the least delay arising from imperfectness of method will, by gradually accumulating, ultimately render the labour of whole days utterly fruitless. Yet, the attention being absorbed by the main object, these little defects are at first unnoticed. At length, however, we become sensible of them, and endeavour to apply a remedy when it is almost too late. But the result of the experiment is not without its use, since it may be more or less serviceable in analogous circumstances. This consideration must be my apology for the minuteness of detail into which I have entered.

The first problem that presents itself, in the series of questions relative to the passage of radiant heat through solid bodies, is to determine the influence which the degree of their polish has, and the quantity of rays transmitted. In order to solve this, we have but to apply our thermometrical method to several screens perfectly similar in all respects, except as to the state of the surface.

Out of the glass of a mirror which was very pure, and nine millimetres in thickness, I cut eight pieces sufficiently large to cover the central aperture of the screen when they were placed on the stand; and, after having removed the quicksilver, I wore them down with sand, emery, and other such substances, so as to form by their succession a complete series of plane surfaces more or less finely wrought, from the first and coarsest to the highest and most perfect polish. These different pieces reduced to one common thickness of 8^{mm}·371 * and ex-

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* All the measures of small degrees of thickness contained in this Memoir have been taken with a pair of *calipers with pivots*, a species of double compasses, with a spring and with legs of unequal lengths, much used in the manufacture of clockwork. This instrument measures directly, and with great nicety, even the fortieth part of a line.

posed to a radiation of 30° of the thermomultiplier, have furnished the following results:

Order of the screens.	Deviations of the galvanometer.
1. Translucid	5°·38
2. ———	6·50
3. ———	8·66
4. Dull	12·58
5. ——	14·79
6. Slightly dull	17·42
7. Transparent	18·79
8. ———	19·15

These transmissions present nothing extraordinary: the quantity of heat which passes through the medium is greater in proportion as the surface is more finely polished, as it happens in respect to light. The only thing to be remarked is, that in the high degrees of polish a slight difference produces a very slight effect. This is evident from the observations made on Nos. 7 and 8.

Similar processes enable us to determine the influence of thickness, which is one of the elements most necessary to be known in the theory of transmission.

Four pieces cut out of a fine mirror were reduced with great nicety to different degrees of thickness in the ratio of 1, 2, 3, 4: particular care was taken to give to their principal surfaces a perfect parallelism, and the highest polish possible. The following are the deviations which they successively produced in the index of the galvanometer under the action of the same radiation, namely 30°:

Thickness of the screens in millimetres.	Deviations of the galvanometer.	Corresponding forces.
2·068	21°·625	21·850
4·136	20·312	20·343
6·202	19·687	19·687
8·272	19·375	19·375

Each number in the second column is deduced from fifteen observations: the quantities registered under the denomination of forces, representing in this particular case the respective temperatures or quantities of rays transmitted, have been calculated according to the principles with the exposition of which we concluded our general observations. The force or temperature answering to 30°, as given by the table of intensities, is 35·3; now, by dividing each number of the third column by 35·3, we shall obtain the ratios of the transmitted rays to the incident rays. The difference between each of these quotients and unity will give the corresponding loss; that is, the proportional part of the rays that are stopped. By performing these operations, and representing the whole radiation by 1000, we obtain

Table A.

Order of the screens.	Transmitted rays.	Rays stopped.
1.	619	381
2.	576	424
3.	558	442
4.	549	451

Let us imagine the thickest of the screens split into four equal layers; the quantities of heat falling upon each will be

1000, 619, 576, 558,

and the quantities lost in successively traversing the four intervals

381, 424—381, 442—424, 451—442;

that is to say,

381, 43, 18, 9.

We shall then have for the ratios of the respective losses to the incident quantities,

$$\frac{381}{1000}, \quad \frac{43}{619}, \quad \frac{18}{576}, \quad \frac{9}{558},$$

or

0·381, 0·071, 0·031, 0·016.

Thus the losses continue to decrease with great rapidity as the thickness increases by a constant quantity.

We have seen that the action of the radiation on the thermomultiplier commences at the instant when the communications are established, produces the greatest part of its effect in the first five or six seconds, and ceases entirely after a minute and a half. These facts, which are equally true of the direct rays and of those which reach the pile after having passed through screens of any thickness whatsoever, constitute the best proof that caloric is transmitted by radiation through the interior of the diaphanous bodies. If, nevertheless, a new confirmation of this truth were desired, it would be found in the successive diminution of the losses which the rays undergo in crossing the different layers of a transparent medium. Were the heat, which is the subject of our immediate inquiries, the effect of a species of conducting power, the losses would continually increase from layer to layer, or would remain constant, from the moment when the rays penetrated the medium, and could never follow the opposite law of decrease.

The progressive diminution of the losses is, moreover, entirely peculiar to the calorific radiation, whose properties in this and in many other respects are altogether different from those of the luminous rays. In fact, everything leads us to believe that the equal layers which succeed one another in a diaphanous medium act in the same manner on the rays of light which come in succession to pass through them, and that they consequently absorb or reflect a quantity of light proportional to the intensity of the incident rays; that is to say, that the loss sustained by the luminous radiation at every layer of equal thickness is constant. In the case under consideration, the invariable decrement of the light at each of the layers into which we suppose the screen divided is found to be none at all, or extremely feeble, because of the perfect transparency of the glass; and yet the caloric rays undergo in their successive passages an absorption, the sum of which is equal to about the half of their whole value; and the losses at each layer, instead of being constant, as happens to those sustained by the luminous rays, are found to differ enormously from one another, being in the proportion of the numbers 381, 71, 31, and 16.

The resistance of diaphanous media to the immediate transmission of the rays of heat is therefore of a nature altogether different from that which is presented by the same media to the propagation of light.

Whatever be the cause of this singular difference, it is highly important to determine with certainty whether it takes place at great distances from the surface at which the rays enter; and this may be done by repeating the experiments on layers of glass much thicker than those which we have been using.

With this view I took several pieces of the glass of Saint-Gobain, and caused them to be recast. This operation was not completely successful. The matter either formed itself into layers that were too thin, or was slightly striated. From among the thick pieces I selected that which was the purest. It was six inches in length. I divided it into three parts, of one, two, and three inches in thickness. The defects being uniformly distributed over all the points of the mass might probably enough alter the quantity of the caloric rays that would have passed through a perfectly pure mass of the same matter and thickness; but it is clear that they could have no influence on the nature of the progression of the losses which these rays might undergo in passing from one layer to another.

The following are the results obtained by exposing these screens to the ordinary radiation of 30°:

Thickness of the screens in millimetres.	Galvanometric deviations.
	°
27	17·105
54	13·458
81	10·702

By a calculation exactly similar to that already made we find that, of every thousand rays emanating from the source, each screen transmits or stops the following quantities:

Order.	Rays transmitted.	Rays stopped.
1.	484	516
2.	380	620
3.	303	697

By means of these data we obtain as the values of the calorific losses, considered with reference to the quantities of rays which present themselves successively to pass through the three equal layers into which we may suppose the last screen divided,

0·516 0·215 0·203.

These losses are still greater than those preceding, because of the badness of the material and the greater thickness of the layers, but they are still in a decreasing progression. Thus the diminution continues beyond 54 millimetres.

To compare this diminution with that which took place in the last screen in the preceding experiments we must multiply 0·012 (the difference between 0·215 and 0·203) by 2·068, and divide the product by 27. In this way we obtain the mean diminution for a thickness of 2^{mm}·068 in passing from 54 to 81 millimetres, which is nearly 0·001; in the preceding experiment it was fifteen times as much while the rays passed through the same layer of 2^{mm}·068 placed at a distance of 6 millimetres. The difference would be still greater if we had used very transparent layers of glass, such as flakes of the glass of a mirror attenuated.

Nevertheless I had some doubts as to the homogeneity of the glass: I was afraid that the striæ might not be equally distributed over all the points of the mass. But not being able to procure large pieces of this material entirely free from defects, I thought that analogous experiments performed with liquids might answer quite as well. In employing these instead of glass there was, in case of success, the additional advantage of extending the law of calorific transmission by making it independent of the physical constitution of the medium.

I procured therefore several copper troughs, of the same breadth but of different lengths, bounded at each end by a glass plate. These I placed successively between the perforated screen and the pile in such a manner that the anterior glass plate was quite near the screen, the distance of which remained constantly the same. The common section of the troughs was much larger than the central aperture of the screen; the reflexions on the lateral faces could not take place, and the only rays that entered a little out of the perpendicular direction reached the anterior surface of the pile. The lamp was moved up so near that the needle of the galvanometer exhibited a deviation of 30° through the two glass plates of each trough. The radiation was then intercepted, the trough filled with oil of colza*, and after having waited until the needle recovered its original position we reestablished the calorific communication.

The deviations obtained through the different thicknesses of the liquid are exhibited in the following table.

Degrees of thickness of the liquid layer.	Deviations of the galvanometer.
mm	o
6·767	15·642
13·535	12·831
27·069	10·389
54·139	9·540
81·209	8·988
108·279	8·512

The free radiation being always represented by 1000, the respective quantities of the rays transmitted and those stopped are found to be :

TABLE B.

Degrees of thickness of the liquid layer.	Rays transmitted.	Rays stopped.
mm		
6·767	443	557
13·535	363	637
27·069	294	706
54·139	270	730
71·209	255	745
108·279	244	756

If we suppose the last layer (of 108$^{\text{mm}}$·274) subdivided into six parallel slices of the following degrees of thickness : 6$^{\text{mm}}$·767, 6·767, 13·535, 27·069, 27·069, and 27·069, we shall be able to determine, by means of the numbers contained in the two last columns, the quantity of heat incident to the first surface of each of these slices and the quantity lost in the passage. Dividing the second by the first we shall ascertain the loss. It is unnecessary to exhibit the operations in detail, as they are in all respects similar to those which have been performed in reference to the screens of glass. Here are the final results :

Degrees of thickness of the six successive slices into which we suppose the layer of 108$^{\text{mm}}$·274 to be divided.	Losses in the respective transmissions referred to the quantities of rays which arrive at the surface of each slice.
mm	
6·767	0·557
6·767	0·180
13·535	0·190
27·069	0·082
27·069	0·056
27·069	0·040

[* It may be proper to inform the English reader that "oil of colza" is an oil expressed from the seeds of the *Chou Colza* of the French, *Brassica arvensis*, Linn. It must not be confounded with the *rape oil* of England, obtained from the Rape, *Brassica Napus*.—EDIT.]

Whence it is concluded that the losses still decrease at a distance of about 100 millimetres.

To comprehend at a single glance the law of the propagation of caloric radiating through diaphanous media we have only to reduce the results contained in the first two columns of the Tables A. and B. to a linear construction.

The mere inspection of the curves thus constructed shows that the rays lose very considerably when they are entering the first layers of the medium. But in proportion to their distance from the surface we see that the loss decreases and that at a certain distance it is almost imperceptible, and the rays seem to continue their progress, retaining all their intensity; so that in glass and in oil of colza, and probably in all other diaphanous media, the portion of heat which has forced its passage through the first layers must penetrate to very great depths.

Delaroche had found that the heat which has passed through one plate of glass becomes less subject to absorption when it is passing through a second. The identity of this fact with the law of resistance in continuous media shows that the solution of the continuity and the interposition of the atmosphere between the two screens do not alter the nature of the modifications which the rays undergo in the first plate of glass. It is therefore exceedingly probable that the proposition of Delaroche is true with respect to a very numerous series of thin screens; for we have just seen that in the same medium the losses still diminish to the depth of 80 or 100 millimetres. In reference to this point, the following is the result of the experiments I have made with four plates of the same glass that had been employed in the first attempts to investigate the law of propagation through continuous media The common thickness of these plates was 2^{mm}·068.

Numbers of the screens.	Deviations of the galvanometer.
	°
1.	21·62
2.	18·75
3.	17·10
4.	15·90

It is scarcely necessary to observe that the common radiation to which the screens had been exposed was always 30 degrees, answering to a force or temperature of 35·3. If we represent this radiation by 1000, as we have done in all the foregoing cases, we have:

Numbers of the screens.	Rays transmitted.	Rays stopped.
1.	619	381
2.	531	469
3.	484	515
4.	450	540

Whence we have

0·381, 0·134, 0·087, 0·058,

as the losses suffered by the rays in successively passing through the four plates of glass; it being carefully kept in mind that these values are not referred to the initial quantity, but to the number of rays which arrive at the surface of each screen.

Thus the proposition of Delaroche is true as far as the third and the fourth screens; for in the transition from one loss to another a diminution of each loss is observable.

It will have been observed that the losses were not so great in respect to the four equal layers of the screen of a fourfold thickness; and that this should happen will be easily conceived if we consider that in the latter case there is a solution of continuity which causes a greater dispersion of the heat by reflexion. But we see that in both cases the difference between two successive losses becomes less in proportion as the distance from the surface, at which the rays entered, is greater.

Let us now proceed to consider the influence exercised on calorific transmission by the composition of the substance of the screen.

M. Prevost had concluded from the experiments described in a memoir already quoted, that water and glass ought to transmit rays of heat in different quantities; for by causing the sheet of water to fall between the lighted candle and a very delicate air-thermometer, he obtained no indication of heat being transmitted unless when he had blackened the ball of the thermometer, and even then the increase of temperature was extremely small; whilst a plate of glass substituted for the sheet of water produced effects sufficiently manifest[*]. But it was objected to him that the difference between the action of the water and that of the glass was owing to the conductible caloric which was perceptible in the latter case only. Delaroche subsequently observed that a square of greenish glass transmitted more heat than a plate of another species of glass perfectly pure. However, as the first flake was much thinner than the second, it was insisted that the difference in the effects was owing to the difference of thickness. At length, some time after the invention of the thermomultiplier, M. Nobili and myself made some experiments on olive oil, alcohol, water, and nitric acid; whence we inferred that water opposed a greater resistance than any of the three other liquids did to the passage of rays of heat emanating from a hot iron[†]. But these experiments are to be regarded only as mere trials, tending to show the facility with which the thermomultiplier may be employed in all sorts of inquiries relative to calorific radiation; for we did not take sufficient precautions to prevent the heat from passing by

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[*] His own words are: "It appears, therefore, that water does not allow so much caloric to pass *immediately* as glass does. At least it affords a passage *of that kind* only to a quantity of caloric more minute than that which passes through the glass." (Mem. already quoted, § 48.)

[†] See the note in page 4.

means of conduction, and to be sure that the temperature was the same throughout. Thus it was still believed that the portion of heat transmitted through solid or liquid substances was governed by the same laws as the transmission of light, and that, *cæteris paribus*, the most diaphanous bodies transmitted the greatest quantity of caloric rays.

The results which I am about to state seem to me to establish beyond the possibility of doubt a fundamental proposition in the theory of radiant heat, namely, that *the power of transmitting caloric rays is by no means proportioned to the transparency of the media;* it is subject to a different law, which, in bodies without regular crystallization, appears to have many affinities to refrangibility. In crystals the phænomena are still more interesting, since in them we find that bodies possessing a high degree of transparency intercept nearly the whole of the caloric rays, while some others act in a manner directly contrary. These properties are invariably manifested whatever be the temperature of the source, and become yet more singular at low temperatures; for in the latter case we find that the ordinary heat of the hand passes immediately through a solid body of several inches in thickness. Let us not, however, anticipate as to the facts, but first of all examine the methods pursued in this third series of experiments.

In the first place it is unnecessary to dwell on the manner in which the solid screens have been exposed to the radiation and the indications of the thermomultiplier, for in this respect everything was the same as in the previous experiments. As to the liquids, these bodies are less permeable to radiant heat than solid bodies are. They must therefore be brought nearer to the thermoscope in order to obtain a well-marked transmission; but then the proper heat of the molecules themselves might be able to act on the instrument, and this the more certainly as the motions always developed in liquids unequally heated easily transfer the particles of the anterior to the further surface of the layers exposed to the source of heat. This effect of conductibility cannot be neutralized in a general manner by continually renewing the interposed layer, as in the experiments of M. Prevost; for some of the liquids can be procured only in small quantities; others, as soon as they are exposed to atmospheric air, undergo considerable alterations and evaporations which produce corresponding elevations or depressions of temperature that prove very annoying in experiments of this kind. The contrivance which I have employed for the purpose of avoiding these inconveniences is very simple. It consists in putting the liquids into very flat glass recipients, whose two large lateral surfaces are perfectly parallel, and the height four or five times that of the surface of the thermoelectrical pile. The lower part of these vessels is applied to the mouth of the tube that envelops the face of the apparatus turned towards the source. The heat stopped by the anterior face of the vessel penetrates the first infinitely thin layer of the liquid; but this layer, while it is becoming hot, undergoes a certain dilatation, becomes lighter than the rest of the fluid mass, and ascends immediately to the upper part of the vessel, whence it can have no longer any influence on the pile. It is replaced by a second layer, which undergoes a similar process, and this again by others; so that by these partial renovations of the liquid screen, the hinder part of the glass applied to the aperture of the tube is not in contact with heated molecules, and retains the same temperature for a long time.

It was extremely difficult to make flat glass vessels with very regular surfaces of the same thickness throughout, and with the opposite sides exactly parallel. Metallic frames and glasses joined with gum could not be employed because of the corrosive action of the several liquids. After many a fruitless effort to surmount this difficulty, I thought at last that the process by which the index of refraction of liquids is measured in optics might be available in this case also. With this view I had quadrangular pieces of two centimetres in breadth and nine centimetres in length cut out of several pieces of the same mirror unsilvered and sufficiently thick. I laid close to the two faces of each of the pieces from which the excision had been made two flakes made out of another and a much thinner glass. It is known that the mere adhesion of two plates of polished glass is sufficient to prevent the passage of liquids. However, in order to be more secure, I introduced each recipient between two metallic frames, which held the thin glasses in their places by means of four screws placed at the angles. The liquid was poured into these vessels at a small aperture made at the top, and furnished with a glass stopper. In such a system there could be no doubt of the parallelism of the faces and the equal thickness of the layers.

The results furnished by the several bodies, both solid and liquid, I have disposed in several tables, each of them exhibiting at the top the common thickness of the screens employed and, beside the substance, the indications of the thermomultiplier and the quantity of rays transmitted as compared with the whole radiation. This distribution, while it allows the use of plates of different thicknesses, has the additional advantage of presenting distinct groups of each class of bodies. The free radiation in each case was 30°. In order to link the results of these tables together, I have commenced the second and the third with the numbers given by a *flake of glass* placed in the same circumstances as the plates which constitute each group: thus the glass set down in the table of liquids was contained between the two thin plates of the recipients, and made of the thick looking-glass employed in their construction. It was therefore exactly of the same thickness as the liquid layers, and, like them, came into contact with the thin plates which formed the faces of the recipients. But as those faces themselves intercepted a portion of the heat, the lamp was brought nearer and nearer until we obtained, through the combination of the three plates, the same indication of 19° that was furnished by the thick glass when exposed singly to the radiation of 30°.

TABLE I.—*Glass (uncoloured).* Common thickness 1^{mm}·88.

	Deviations of the galvanometer.	Rays transmitted.
No screen	30°·00	100
Flint-glass (of Guinand)	22·90	67
Flint-glass (English)	22·43	65
Flint-glass (French)	22·36	64
Another kind	22·19	64
Mirror-glass	21·89	62
Another kind	21·10	60
Another kind	20·78	59
Crown-glass (French)	20·58	58
Window-glass (common)	19·25	54
Another kind	18·56	52
Another kind	17·83	50
Crown-glass (English)	17·22	49

TABLE II.—*Liquids.* Common thickness 9^{mm}·21.

	Deviations of the galvanometer.	Rays transmitted.
Mirror-glass	19°·10	53
Carburet of sulphur (colourless)	21·96	63
Chloride of sulphur (of a strong brownish red colour)	21·83	63
Protochloride of phosphorus (colourless)	21·80	62
Hydrocarburet of chlorine (colourless)	13·27	37
Nut-oil (yellow)	11·10	31
Essence of turpentine (colourless)	10·83	31
Essence of rosemary (colourless)	10·46	30
Oil of colza (yellow)	10·38	30
Oil of olives (greenish yellow)	10·35	30
Naphtha (natural—a light brown yellow)	9·77	28
Balsam of copaiba (a sufficiently decided brown yellow)	9·39	26
Essence of lavender (colourless)	9·28	26
Oil of pink [*huile d'œillet*] (very slightly yellowish)	9·26	26
Naphtha (rectified, colourless)	9·10	26
Sulphuric æther (colourless)	7·59	21
Pure sulphuric acid (colourless)	6·15	17
Sulphuric acid (of Nordhausen, of a sufficiently decided brown)	6·09	17
Hydrate of ammonia (colourless)	5·47	15

TABLE II.—continued.

	Deviations of the galvanometer.	Rays transmitted.
Nitric acid (pure and colourless)	5°·36	15
Alcohol (absolute and colourless)	5·30	15
Hydrate of potassium (colourless)	4·63	13
Acetic acid (rectified, colourless)	4·25	12
Pyroligneous acid (of a slightly brownish colour)	4·28	12
Sugared water [*eau sucrée*] (colourless)	4·20	12
Alum water (colourless)	4·16	12
Salt water (colourless)[*]	4·15	12
White of eggs (slightly yellowish)	4·00	11
Distilled water	3·80	11

TABLE III.—*Crystallized bodies.* Common thickness 2$^{\text{mm}}$·62.

	Deviations of the galvanometer.	Rays transmitted.
Mirror-glass	21°·60	62
Rock salt (diaphanous)	28·46	92
Iceland spar (diaphanous)	21·80	62
Another species (diaphanous)	21·30	61
Rock crystal, colourless (diaphanous)	21·64	62
Rock crystal, smoky (diaphanous and very decidedly brown)	20·25	57
Brazil topaz, colourless (diaphanous)	19·18	54
Carbonate of lead (diaphanous)	18·35	52
White agate (translucid)	12·48	35
Sulphate of barytes (veined, dully diaphanous)	11·72	33
Emerald (diaphanous, of a light blue)	10·16	29
Yellow agate (translucid, yellow)	10·10	29
Borate of soda (translucid)	9·87	28
Green tourmaline (diaphanous, green)	9·54	27
Adularia (diaphanous, dull, veined)	8·30	24
Sulphate of lime (diaphanous)	7·15	20
Fluate of lime (diaphanous, dull, veined)	5·40	15
Citric acid (diaphanous)	5·15	15
Sardoine (translucid)	4·98	14
Carbonate of ammonia (diaphanous, dull, striated)	4·50	13
Tartrate of potash and soda (diaphanous)	4·40	12
Alum, crystal (diaphanous)	4·36	12
Sulphate of copper (strongly diaphanous, blue)	0·00	0

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[*] In this solution we used very diaphanous pieces of rock salt; the same may be said of the solution immediately preceding—the water was completely saturated with the alum.

TABLE IV.—*Glass (coloured).* Common thickness $1^{mm}\cdot85$.

	Deviations of the galvanometer.	Rays transmitted.
	°	
Deep violet	18·62	53
Yellowish red (flaked)	18·58	53
Purple red (flaked)	18·10	51
Vivid red	16·54	47
Pale violet	16·08	45
Orange red	15·49	44
Clear blue	15·00	42
Deep yellow	14·12	40
Bright yellow	12·08	34
Golden yellow	11·75	33
Deep blue	11·60	33
Apple green	9·15	26
Mineral green	8·20	23
Very deep blue	6·88	19

It is sufficient to cast the eye rapidly over the second and third tables to be fully sensible of the truth of the proposition, that "*the capacity which bodies possess of transmitting radiant heat is totally independent of their degree of transparency.*"

In fact, the liquid chloride of sulphur of a tolerably deep red brown transmits a considerably greater number of caloric rays than the fat oils of nut, the olive, and colza having a clearer tint; while these oils, although of a very decided yellow colour, are more permeable to radiant heat than several other liquids which are perfectly limpid, such as concentrated sulphuric and nitric acid, æther, alcohol, and water. The case is the same with solid bodies, among which we see sulphate of lime, citric acid, and other very diaphanous substances allow a much smaller quantity of heat to pass than some other bodies coloured or translucid, such as emerald, agate, tourmaline, borax, adularia, and sulphate of barytes.

But nothing is better calculated to demonstrate that transparency has little or no effect in the transmission of heat than a comparison of the effects obtained by the crystal of alum with those obtained by means of the smoky rock crystal. The table shows that, in respect to these substances as well as the others which we have just mentioned, the capacity to transmit radiant heat is inversely as the capacity of transmitting the rays of light. I was anxious to try how far this inverse ratio of the calorific to the luminous transmissions might extend, by varying the degrees of thickness so as to give to the light all the advantage and the whole of the loss to the caloric. We submitted to the test a plate of well-polished and perfectly transparent alum only one millimetre and a half in thickness, and a smoky rock crystal the thickness of which in the direction of its polished faces was 86 millimetres.

The brown colour of the crystal was so decided that when it was laid on a printed page in which the letters were very large, and placed in the fullest light, even the traces of the letters could not be distinguished. The paper and the printed characters became confounded together and presented the same dark hue. This crystal, however, transmitted 19°, while the thin plate of alum transmitted only 6°.

A body may then be very opake and afford a very easy passage to the rays of heat; or very transparent and intercept the greatest part of them. It is therefore necessary to distinguish those bodies which possess a capacity for calorific transmission from those which possess a capacity for luminous transmission, by giving them different denominations. The terms *transcaloric* and *diathermanous** (*transcaloriques ou diathermanes*) seem to me to be best suited to this purpose, as being most analogous in form to the epithets *transparent* and *diaphanous*, applied to bodies endowed with the property of transmitting light.

After the statement made in respect to the smoky rock crystal, one might be tempted to ask whether there are any transcaloric substances totally opake. To that question no answer can be given until the effect of calorific radiation upon all known bodies has been tried, and this I am far from having done. I can only go so far as to say that pyroligneous acid in the rough state, and Peruvian balsam, though almost completely opake, afford perceptible transmissions of radiant heat. But all the diathermanous substances that I have subjected to experiment are comprised within that class of bodies which possess some degree of transparency. Those kinds of metal, wood, and marble which totally obstruct the passage of light obstruct that of heat also. Some other bodies, such as carburet of sulphur, rock salt, and Iceland spar, allow both kinds of rays to pass at the same time. It is therefore *probable* that calorific transmission cannot take place without a certain degree of transparency†; but it cannot take place *abundantly* without the cooperation of another quality, which varies as the bodies happen to be crystallized or without crystallization. We find, in fact, that in the different sorts of glass and liquids it follows the order of the different degrees of refrangibility; for flint-glass possessing a greater refracting power than crown-glass affords an easier passage to the caloric radiation. Carburet of sulphur is at the same time more refracting and

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* The first of these terms requires no explanation. The second is derived from διὰ, *through*, and θερμαίνω, *to heat*, as the word *diaphanous* is derived from διὰ, *through*, and φαίνω, *to show*.

† I have since found that the perfectly opake glass employed in the construction of mirrors designed to show the polarization of light transmits a considerable quantity of caloric rays. These *obscure* rays emerging from the dark glass may be employed in some curious experiments which we shall mention in the second Memoir.

more diathermanous than the essence of turpentine; the same may be said of turpentine as compared with olive oil, and so on until we come to pure water; a liquid which, as it possesses the least power of refraction, possesses also the least power of transmitting heat. It is very true that, in the tables, glass appears almost as diathermanous as carburet of sulphur, although its refracting power is considerably less; but this equality is but in appearance; and to be convinced that it is so, we have only to recollect the manner in which the liquids have been subjected to the experiments. Before they can reach the liquid layer, the rays must have passed through the anterior face of the vessel containing it, and the glass gives but a transmission of from 21 to 22 for 35·3. Thus the radiation that will penetrate to the interior of the vessel will be of no greater force than this; so that even if the liquid transmitted all the rays that reached it, the quantity issuing from the recipient cannot exceed 22. This explanation is confirmed in a very striking manner by the transmissions of the chloride of sulphur and the protochloride of phosphorus. The indices of refraction of these two liquids, though not well known, are certainly higher than that of glass, and have different values; a fact from which it may be inferred with great probability that the quantities of transmitted heat are also different, though in the tables both these quantities appear equal to the transmission assigned to the carburet of sulphur.

There are, it is true, some real anomalies in the transmissions through balsam of copaiba and sulphuric æther. But the differences are very small, and may probably be referred to some slight error in the measure of the transmission or the refraction. The proportionality of these two elements is obvious, and so fully established in such a variety of cases, that it may hold as a general law for liquids, for the several kinds of glass, and probably for all those bodies which are without regular crystallization.

But this law totally fails with respect to crystallized bodies. We see, in fact, that carbonate of lead, a highly refractive and colourless substance, transmits less heat than Iceland spar and rock crystal, which are much inferior to it in refracting power; while rock salt, possessing the same transparency and the same index of refraction as citric acid and alum, gives six times their amount of calorific transmission.

The transparent and colourless bodies contained in the third table are nine in number, namely, rock salt, Iceland spar, rock crystal, topaz, carbonate of lead, sulphate of lime, citric acid, tartrate of potash and soda, and alum. These crystals transmit the following quantities of heat respectively:

92, 62, 54, 52, 20, 15, 12.*

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* Such as have not a thermoscopic apparatus similar to that which we have employed may easily satisfy themselves that rock salt transmits almost all the

Differences so striking in bodies of the same aspect seem to arise rather from the particular structure of each crystal than from the chemical composition of the molecules; for a block of common sea salt being divided into flakes instantly arrests calorific radiation ; and we perceive besides, by means of the second and third tables, that the transmissive power of pure water is increased nearly in the same degree whether we dissolve in it alum or rock salt, two substances which, in their solid state, transmit very different quantities of heat. But we perceive no relation between the power of transmitting heat and the primitive or the secondary form of crystallization.

M. Mitscherlich has found that the dilatation of crystals, when they are submitted to the action of heat, is not equal on all sides. Although such an effect may not proceed from the radiant heat, yet it might be thought that a difference in the direction in which the plates are cut out of the crystal would produce a difference of transmission. I have had plates of equal thickness cut out of rock crystal in all the principal directions relatively to its axes. The transmission varied in no case. I obtained the same results from Iceland spar.

Radiant heat is capable of passing through crystallized bodies of very considerable thickness. It may be affirmed, also, that the rays do not lose so much in the interior of these bodies as they do in the masses of glasses and of liquids. For we have seen that the deviation changed only from 21°·6 to 19°, though the smoky rock crystal first employed was replaced by one of fifty-seven or fifty-eight times its thickness.

I have exposed to the action of radiant heat a piece of Iceland spar 92 millimetres* in length. The deviation, which was 21°·8 through a flake of the same substance 2mm·6 in length, fell no lower than 18°·5; a circum-

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radiant heat that falls on its surface, by fixing vertically, on the same stand, a plate of this substance and a plate of glass or alum of the same dimensions, and by bringing the stand quite close to the fire of a stove. If it is allowed to remain in this state for five or six minutes, the glass becomes burning hot, while the rock salt, if applied to the most tender part of the hand, will produce no sensation of heat. These differences of temperature exist not merely in appearance, but are as palpable as those that are felt when we touch wood and marble that have been exposed to the sun. To prove this, we need only lay some pieces of wax or suet on the two bodies. Those laid on the glass will melt rapidly, but those laid on the rock salt will continue in their solid state. We may also demonstrate in a direct manner, and without the aid of a thermomultiplier, the great transmissiveness of rock salt as compared with other diaphanous substances. Let the two plates be brought close together in the same plane, and behind them let two metallic tubes be placed, with the blackened balls of two common thermometers of equal sensibility fixed at their further extremities. If we now place a red-hot bullet at a certain distance from the plates, the thermometer that is to indicate the transmissive power of the alum will ascend but 1°, while the other will ascend 8° or 10°.

* [A *millimètre*, it will be remembered, is equal to ·03937 of an English inch. —Edit.]

stance which shows that the diminution of effect was only about one seventh for an increase of thickness equal to thirty-five times that of the first piece. The experiment was still more interesting when I employed rock salt, in which I was unable to discover that thickness had any influence whatever on the amount of the transmission: for pieces of 2^{mm} gave the same galvanometric deviation as pieces of 30^{mm} and 40^{mm}.

From these observations it follows that the numbers in the second column of the table of crystals, though they express the ratios of the calorific transmissions of those bodies reduced to the common thickness of $2^{mm}\cdot6$, may be employed also to represent approximately the ratios of the transmissions, even when the common thickness is greater. I say *approximately*, because, in order to determine the true specific transmissions, it would be necessary to know the exact law of the loss at the several points of the media. If the losses, as compared with the quantities of heat which arrive at each of the thin laminæ into which we may imagine the medium to be divided, were constant, the intensity of the rays would decrease in a geometrical, while the layers increased in an arithmetical ratio; and in order to know how much one substance is more diathermanous than another, we should vary the relative degrees of thickness of the plates until we obtained the same transmission in the two cases. The ratio sought would be inversely as the degrees of thickness which produced an equality of action *. Now we have seen that this constancy in the loss does not exist. But in the particular case of crystallized bodies, the differences are so very small when the thickness is increased beyond 3^{mm}, that the ratios obtained by operating on thicker screens would not differ materially from those which we have found.

But even if we had succeeded in ascertaining the specific transmissive powers of the different substances, the question would not yet be solved in a general manner; for we shall see in the second Memoir, that if, while we vary the temperature of the calorific source, we do not change the order of the transmissions also, the relations of these quantities are no longer the same. To perceive this we have only to recollect what has been already stated as to the action of rays emitted from a source of low temperature on certain substances; that is, that the heat of the human body instantly passes through a certain crystal, and that crystal is rock salt.

It is known that the caloric rays of the hand are completely stopped by glass. Hence, although the ratio of transmission between glass and rock salt, when the source is an Argand lamp, be 62 : 92, it becomes

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* For the demonstration of this proposition, see Bouguer, *Traité d'Optique sur la Gradation de la Lumière*, Paris, 1760, liv. III. sect. 1re, art. 1, 2, 3, 4.

1 to infinity when we consider the effects produced by sources of a low temperature.

Hitherto we have made no account of the colours [of the diathermanous bodies], or, rather, have considered them only in relation to the diminution of transparency, or to the greater or less opacity which they always cause in diaphanous substances*.

We must now examine them more particularly, and determine their influence on transmission. Such is the object of the fourth table. The tints of those kinds of glass marked with an asterisk are the purest, and approach nearest to those prismatic colours that bear the same names. Of this I have satisfied myself by the following experiments. Having by means of a heliostat introduced a horizontal sheaf of solar rays into a dark chamber, I divided it into two by causing it to pass through two apertures made in an opake screen. I contrived to make one of the sheaves fall on a vertical prism, and the other on a coloured glass which I wished to try. Thus the solar spectrum was seen cast on one side, and a coloured spot in the line of the direct rays. To bring this spot into contiguity with the corresponding colour of the spectrum, I placed behind the glass a second vertical prism which turned about until the desired effect was obtained. The two analogous tints are always easily compared when they are near each other, and at the same time we are able to judge whether the colour of the glass be more or less pure by the new tints which are always developed in the passage of the coloured rays of the glass through the prism. Of fourteen colours selected from several species of glass, I have found but five making any near approach to the prismatic colours and producing very feeble secondary tints. These tints were absolutely imperceptible only in the case of red glass.

There is another mode (and it has not been overlooked) of appretia-

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* I was lately told by an eminent philosopher, that to think of comparing the intensities of different colours would be as absurd as it would be to institute a comparison between heterogeneous elements. Waiving all inquiry as to the correctness of such an assertion, I beg leave to remark that in certain cases it is unanimously agreed that a tint is more or less clear than another tint of a different kind, without giving rise to any metaphysical ideas opposed to the general opinion. Let us take, for instance, the solar spectrum. Has it not been always held that the maximum of brightness is to be found in the yellow, and that on each side of it luminous intensity decreases? The principle put forward by me seems equally plain. When I assert that colours always introduce some opacity into diaphanous bodies, no one is at a loss for my meaning. Put some pure water between two parallel plates of colourless glass: let an observer be placed at one side, and at the other a piece of writing, which is to be moved just so far from its first position as to become illegible. Now, for the water substitute wine or oil or any other diaphanous liquid *more or less coloured*; the distance at which the writing may be read will become less in proportion to the greater *depth* of the colour independently of its kind. Thus when the writing will be legible at the same distance through a yellow and a red liquid, these two media will, in respect to us, be equally transparent.

ting the influence of colour in diaphanous media. It consists in causing corresponding rays of the spectrum to pass through the glasses. The passage is attended only with a very inconsiderable loss when the tints are very pure. Now by fixing one side of my five plates of glass at proper distances on the margin of a sheet of pasteboard exposed to the coloured sheaf of the prism, I found that each prismatic ray traversed glass of the same colour without suffering any loss of intensity. At least, the alteration produced by these glasses on the corresponding solar rays was nearly the same in all cases. This fact is inferred from a comparison of the prismatic rays which fall on the wall directly and those which reach it after having passed through the coloured pieces of glass. The shadows brought by the latter rays are so very light as to be almost imperceptible. In every other case they are very strongly marked. If for instance we substitute the violet for the red, the spot on the wall is almost dark; if the violet be not perfectly pure, it will not at least transmit a quantity of red rays less than that which passes through the red glass.

It is known that in the solar spectrum produced by a prism of common glass, the greatest heat is found in the red, and that the intermediate temperatures continually decrease until we come to the violet. Does this calorific distribution in the coloured rays, separated by the refracting power of the prism, exist also when they are separated by the absorptive power of the colouring matter?

In order to ascertain this we have only to compare, at the different temperatures of the spectrum, the numbers which represent the calorific transmissions of our five coloured glasses; they are as follows:

violet	red	yellow	blue	green
53,	47,	34,	33,	26.

The order of the colours considered relatively to their degrees of heat and the numerical relations of those degrees are so altered that the violet light, which in the spectrum possesses a temperature twenty-five or thirty times lower than that of the red light, appears here of a higher temperature. Such a difference is not to be explained by supposing that, in the transmission of the violet glass, there passes a great quantity of red rays; for it should, on this hypothesis, be found to transmit them in a greater proportion than they are transmitted by the red glass; which, according to the preceding experiments, is impossible.

These facts seem to be opposed to the opinion of those philosophers who hold that in luminous heat the same rays simultaneously excite the two sensations of light and heat, but would be easily comprehended if we supposed caloric and light to be two distinct agents. In the latter case we should say that in the prism the refractive force acts unequally on the different caloric rays, as it does, in a greater or less degree, on the different luminous rays, and thus throws certain quantities of heat on the very spaces occupied by the different colours of the spectrum; but that in the coloured glasses and, generally, in bodies more or less diathermanous, the absorbent force does not act in the same manner as the force of refraction, which sometimes extinguishes more heat than light and at others more light than heat.

But those who maintain the identity of the two agents will reply, that the differences observed in the calorific and luminous transmissions of the diaphanous or coloured media are produced by rays of obscure heat which mix in great quantities with the rays of light emitted by the flame.

In order to decide how far it is allowable to maintain the one or the other hypothesis, we should have data which, at present, are not within our reach. We shall resume this subject at the end of the next Memoir, and conclude the present one with an account of a very remarkable application of the numerical results contained in the foregoing tables.

It had been established by the beautiful experiments of Seebeck that the place of the maximum of temperature in the solar spectrum varies with the chemical composition of the substance of which the prism is made. This eminent philosopher observed that the highest degree of heat which, in the spectrum furnished by a prism of crown glass, was in the red, passed to the orange when the prism employed was a hollow glass one filled with sulphuric acid, and was found in the yellow when the same prism was filled with pure water*.

I discovered some months since that the caloric rays scattered on the colours given by a common prism do not undergo the same alteration in passing through a layer of water; the loss varies inversely as the refrangibility, so that the most refrangible rays pass undiminished and the least refrangible are entirely stopped by the liquid†.

This experiment led to a very simple explanation of the results obtained by Seebeck.

The solar heat which presents itself to the anterior face of the prism of water contains rays of every degree of refrangibility. Now the ray which has the same index of refraction as the red light, suffers in passing through the prism a loss porportionally greater than the ray which possesses the refrangibility of orange light, and less is lost by the latter in the passage than by the heat of the yellow ray. These increasing ratios in the losses of heat sustained by the less refrangible rays have an evident tendency to transfer the maximum to the violet. It may therefore be stopt at the yellow.

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* Schweigger's *Jahrbuch der Chemie und Physik*, vol. x. [A translation of the memoir of Seebeck here referred to will be found in the Philosophical Magazine, first series, vol. lxvi. p. 330, *et seq.*—Edit.]

† *Annales de Chimie et de Physique*, Décembre 1831.

If we suppose the action of sulphuric acid analogous to that of water; but not so energetic, we shall see the reason why, with the prism of acid, the maximum takes place in the orange. In short, the very glass of which the common prisms are made must operate in a similar manner, and cause in each ray a loss inversely proportioned to its refrangibility. Therefore, if we employed in the construction of the common prism a substance *less active* than common glass, the losses sustained by the less refrangible rays would be diminished in a greater ratio ; so that they would gain on the more refrangible rays, and the maximum would pass in a direction opposite to the preceding, that is, from the violet to the red.

This is exactly the result obtained by Herschel, Englefield, and Seebeck by operating on prisms of flint glass; for the maximum was transferred to the obscure space quite close to the last red stripe of the spectrum.

Let us compare these effects with the numbers which represent the calorific transmissions. We shall find that the maximum of heat, in passing from the yellow, where it is found when we use a prism of water, departs from it *always in the same direction* in proportion as the substances of the prisms substituted for the water are more diathermanous. It passes a little out of the spectrum when, instead of crown, we employ flint glass. Admitting then the correctness of such a theory, the line of greatest heat must pass quite beyond the colours into a space far distant from the red limit if we employ rock salt, a substance possessing a far greater diathermancy as compared with flint glass than flint glass does as compared with crown. I tried the experiment; it was completely successful. I found that the maximum of temperature in the spectrum derived from the prism of salt was thrown into the dark space as far at least from the last band as the blue is (in an opposite direction) from the red. At the moment I cannot assign more exact measures; for in the first place I operated with very small prisms, and when I subsequently obtained larger pieces the season did not allow me to reconsider and study the result more nicely. But the effect has been so marked in the experiment which I made, and so invariable in several successive repetitions, that I look upon it as decisive, and have not the least doubt as to the removal of the maximum of temperature to the last band of the red rays in the spectrum produced with rock salt *.

The distribution of the degrees of temperature in the solar spectrum

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* I have since obtained the same results with five prisms of rock salt whose angles of refraction vary between 30° and 70°. These prisms have been made out of several pieces taken from the mines of Cordona, Wieliecza, and Vicq: they have been cut in different directions relatively to the axis of crystallization. I shall give the numerical data in a work in which it is intended to treat specially of the analysis of the caloric solar rays.

is therefore a phænomenon entirely depending on the order which we have found to exist in respect to the calorific transmissions of diaphanous bodies.

This phænomenon now constitutes a striking relation between the properties of the caloric rays of the sun and those of the radiant heat of terrestrial bodies; but we shall see relations yet more intimate appear between these two species of rays when we examine the alterations produced in calorific transmissions by changing the temperature of the radiating source.

ARTICLE II.

New Researches relative to the Immediate Transmission of Radiant Heat through different Solid and Liquid Bodies; presented to the Academy of Sciences on the 21st of April, 1834, and intended as a Supplement to the Memoir on the same subject presented to the Academy on the 4th of February, 1833; by M. MELLONI.

From the *Annales de Chimie et de Physique*, t. LV. p. 337.

Of the modifications which Calorific Transmissions undergo in consequence of the Radiating Source being changed.

THE experiments described in the former Memoir have shown that diaphanous bodies do not act in the same manner on the rays of heat and the rays of light simultaneously emanating from the most brilliant flame. We have seen, in fact, that thin flakes of alum and of citric acid, because of their transparency, perceptibly transmit all the luminous rays of an Argand lamp, and stop from eight to nine tenths of the caloric; while, on the other hand, thick pieces of smoky rock crystal intercept nearly the whole of the light and allow the radiant heat to pass freely. Do the different properties thus exhibited by each body, relatively to the two agents, and the relations of the calorific transmissions of the one screen to those of the other, remain constant, whatever be the source (luminous or obscure) whence the rays emanate? Such are the first questions that I have undertaken to solve in this second series of researches.

That the comparison between the quantities transmitted in each particular case might be fairly made, it was necessary to operate upon rays emitted by a source having a constant temperature. This condition could be complied with by means only of certain flames and boiling liquids. I was therefore unable to vary the experiments so much as I should have desired. The sources however which I have employed present the most remarkable phases of the heating and combustion of bodies. They are four in number; namely, the flame of oil without the interposition of glass, incandescent platina, copper heated to 390°, and boiling water. Thus I had two *luminous* and two *non-luminous* sources. The first is furnished by a Locatelli lamp*; the second is a spiral of platina wire kept in a state of incandescence by means of a lamp fed with spirit of wine; the third is obtained by covering a flame of alcohol with a plate of copper, which soon acquires a fixed temperature whose mean value, as found by the method of immersion, is 390° Cent. (732° Fahr.); and the last source is merely a vessel of thin copper, blackened on the outside and filled with boiling water.

The intensities of the radiations have been always ascertained by the thermomultiplier. The means necessary to be adopted in order to obtain with this instrument the measure of the immediate transmission having been stated in the Memoir already quoted, I think it needless to enter here into further detail as to the arrangement of the apparatus and the nature of the galvanometric indications. I shall only remind the reader that as this method requires that the operation should be performed under the influence of a radiation equivalent to 30° of my thermomultiplier, the diaphanous substances, if placed at a suitable distance between the thermoelectric pile and the source of heat, cannot acquire a temperature sufficient to produce in the instrument any perceptible action. This is proved in three ways: first, by placing the screens on their stand after having exposed them to a calorific radiation of the same intensity as that to which they are exposed during the experiment; secondly, by substituting for the diaphanous body plates of blackened glass or metal, flakes of wood or stone, or sheets of paper; thirdly, by varying the nature and thickness of the medium (more or less transparent) through which the rays are to pass, from the thinnest plate of mica to pieces of rock crystal, glass, or Iceland spar several inches in thickness. In the first case the index of the galvanometer remains unmoved, notwithstand-

* The Locatelli lamp is merely a common lamp with one current of air and fed with oil. It has a wick of the shape of a quadrangular prism, which exactly fills the beak, but has no funnel. It gives a fine flame of constant temperature. The Argand lamp produces a flame of much greater intensity.

In the first series of experiments the main object was to determine the difference between the calorific and the luminous transparency. We therefore preferred the source which was least favourable to the establishment of the principal fact which it was then our purpose to verify. In the present experiments we proposed more particularly to examine the calorific transparency by itself. It was therefore necessary to operate upon rays that had not been forced to undergo a transmission previously to their being employed in the experiments.

ing the heat acquired by the screens ; in the second case also it remains unmoved, although in this case the plates (blackened or opake) are submitted to the actual radiation of the source itself. In the third case the index of the galvanometer leaves its position of equilibrium and describes arcs of greater or less extent according to the quality and thickness of the screen. But the time which it takes to reach the extremity of these arcs is invariable, and equal to that which it takes to describe a deviation of 30° when there is no screen interposed.

This third proof, though indirect, is nevertheless the most convincing, and possesses the additional advantage of showing, as it were, palpably, that the manner in which radiant heat is transmitted in the interior of diathermanous substances is altogether analogous to that in which light is propagated through transparent media whether solid or fluid. For in respect to the latter we perceive no appretiable difference between the times which the luminous rays take to pass through layers of any quality and thickness whatsoever.

The analogy between the transmission of light and that of radiant heat is rendered still more striking if, by shaking or otherwise, a motion is produced in the mass of the screen submitted to the experiment. I have passed the different parts of a large square of glass rapidly before the narrow aperture of the metallic plate through which the calorific rays that strike the surface of the pile are transmitted. By means of a bow I made it vibrate ; it emitted sounds more or less acute : the index of the galvanometer pointed invariably to the same degree of its scale. I found the deviation of the magnetic needle equally invariable when I measured the intensity of the calorific radiation through a layer of acidulated water, at first still, but afterwards set in motion by agitators or traversed by a strong electric current.

Here then, though under different forms, the fact observed in the experiments of Pictet and Saussure when we agitate the mass of air interposed betwen the reflectors is reproduced; namely, the impossibility of altering by these means the direction or the intensity of the luminous or the calorific rays passing through atmospheric air or any other diaphanous medium.

These different considerations seem to me well calculated to dispel every shade of doubt that may yet be entertained as to the immediate transmission of radiant heat by diathermanous bodies, whether solid or liquid. But (to return to the four sources) we have already observed that in our method of proceeding it is necessary to operate uniformly under the influence of a radiation equal to 30° of my thermomultiplier. Now to effect this with sources of various temperatures they must be brought more or less close to the thermoelectric pile until we have obtained the galvanometric indication required, and such is the way in which we have proceeded in all our experiments of transmission. The same screen being, in those different circumstances, submitted to the same quantity of radiant heat, the different degrees of diminution suffered by this heat in passing through it must evidently be attributed only to the peculiar quality of each radiation. This reflection will give still greater force to the truth of the consequences which we are about to deduce from the results of our experiments.

Seven plates of glass of different degrees of thickness submitted to the action of the four sorts of calorific rays in succession have given the following transmissions:

Thickness of the plates.	Transmissions of the glass out of 100 rays of heat issuing from			
	a Locatelli lamp.	incandescent platina.	blackened copper heated to 390° (732° Fahr.)	blackened copper heated to 100° (212° Fahr.)
$0^{\text{mm}}{\cdot}07$	77	57	34	12
0 ·5	54	37	12	1
1	46	31	9	0
2	41	25	7	0
4	37	20	5	0
6	35	18	4	0
8	33·5	17	3·4	0

Although we do not exactly know the degree of heat given by the flame of oil or by platina kept in a state of incandescence by an alcohol lamp, we are nevertheless quite certain that the first of these possesses a higher temperature than the second, and that this again exceeds the 390° of the first plate of copper. Now a glance at the table is sufficient to show that the number of rays transmitted by the same plate decreases with the temperature of the calorific source, a fact which confirms the well-known law of Delaroche. But the decrease is more or less rapid in proportion to the greater or less thickness of the plate.

Let OM, ON, (Plate I. Fig. 1.) be two rectangular axes of the same length; let the first represent the thickness of the screen of 8^{mm} and the second the total quantity of incident heat. Let us divide OM into six parts, Oa, Ob, Oc, Od, Oe, Of, respectively equal to $\frac{0\cdot07}{8}$ OM, $\frac{0\cdot5}{8}$ OM, $\frac{1}{8}$ OM, $\frac{1}{4}$ OM, $\frac{1}{2}$ OM, and $\frac{3}{4}$ OM; and through the points of division let us draw the perpendiculars $aa' = \frac{77}{100}$ ON, $bb' = \frac{54}{100}$ ON, $cc' = \frac{46}{100}$ ON, $dd' = \frac{41}{100}$ ON, $ee' = \frac{37}{100}$ ON, $ff' = \frac{35}{100}$ ON, $Mg' = \frac{33\cdot5}{100}$ ON. The curve $(a'\,b'\,c'\,d'\,e'\,f'\,g')$ passing through the extremities of these perpendiculars will represent the decreasing intensity of the Locatelli lamp at each point of the screen of 8^{mm} in thickness.

A similar construction will give the curves $a''b''c''d''e''f''g''$, $a'''b'''c'''d'''e'''f'''g'''$, $a^{IV}b^{IV}$, representing the decreasing intensities of the three other radiations.

Let us now suppose the screen cut by any plane (PP′) parallel to ON; the emergent rays of the detached plate will be determined by the points at which the plane intersects the curves; so that PP′, PP″, PP,‴ will represent the quantities of heat that issue from the plate OP when exposed to the first three sources; for the rays of the fourth are completely extinguished at the distance of one millimetre. We now see that the ratios of the distance from those points of intersection to the axis OM decrease in proportion as the thickness of the interposed layer is less. The distances from those points to the axis are pretty nearly equal when the section coincides with the ordinate aa' at which the observations commence; they will become yet more so in the interior of the first layer Oa, so that within a limit very close to the surface at which the rays enter the differences will almost vanish*.

The first infinitely thin plate will therefore transmit sensibly equal quantities of radiant heat from the four sources. The diminutions however which the rays from each source will suffer in the interior of this elementary plate, though so exceedingly small that they may be disregarded in reference to the quantities transmitted, must nevertheless bear very different ratios to one another; for it is to such diminutions, several times repeated by the action of the successive layers, that we are to attribute the remarkable differences in the quantities of heat transmitted from each source by a screen of a given thickness.

The law of Delaroche did not show whether the variable interception

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* I have been unable to procure plates of glass thinner than $\frac{7}{100}$ of a millimetre. But we shall see presently that all other diaphanous substances, whether natural or artificial, are in their effects more or less analogous to glass. Now there are several crystals which spontaneously separate into plates of great tenuity, and are, consequently, well calculated to show that the ratios of the quantities of heat transmitted by a screen exposed to the radiations of the four sources approximate to equality in proportion as the thickness of the screen is reduced. Thus a plate of sulphate of lime $2^{mm}\cdot6$ in thickness gave for the four transmissions respectively,

14, 5, 0, 0.

These transmissions became

38, 18, 7, 0

when the thickness was reduced to $0^{mm}\cdot4$; and

64, 51, 32, 21

when the thickness was reduced $0^{mm}\cdot01$.

A plate of mica, $0^{mm}\cdot02$ thick, gave for the four transmissions

80, 76, 39, 26.

An extremely thin flake was taken from this plate (which was however not coloured): the four transmissions through this flake were,

86, 85, 61, 46.

of the same flake arose from an internal or external action of the screen. Nay, more; the ordinary properties of the caloric seemed to lead to the far more probable consequence that the interception was entirely superficial; or in other words, that as the same plate of glass successively exposed to the radiations from several sources gave different calorific transmissions, it was natural to suppose that the heat was first stopped at the external surface in a proportion varying with the temperature of the source, and subsequently propagated inwards according to the known laws of conductibility. But the experiments which I have just mentioned seem to me to demonstrate clearly that *the calorific rays from different sources are more or less quickly extinguished in the very interior of the mass.*

Thus the molecules of glass act upon radiant heat with a real *absorptive force*, the activity of which is greater in proportion as the temperature of the source is lower. It will perhaps be now asked whether this kind of action be common to all diaphanous substances or peculiar to glass only.

To determine this, it is not necessary to repeat on all the bodies those experiments which we have made on different thicknesses of glass; for, the law of Delaroche being once established, it will follow that the substance of which the flake is composed operates on the rays of heat with an absorptive force inversely as the temperature of the source: and as this force acts from all points of the mass, it is clear that the difference between one transmission and another must decrease with the thickness of the screen. The question is therefore reduced to this; whether all bodies more or less transparent act upon heat radiating from different sources in a manner analogous to that which we have observed in one only of our flakes of glass.

I have registered in the following table the quantities of heat immediately transmitted from each of the four sources through plates of different kinds reduced to the common thickness of $2^{mm}\cdot6$. The transmissions are expressed in hundredth parts of the incident quantity. They are uniformly measured, like the preceding, under the action of a radiation of the same force derived from each source of heat.

Names of the interposed substances (common thickness, $2^{mm}\cdot 6$.)	Transmissions from 100 rays of heat issuing from			
	a Locatelli lamp.	incandescent platina.	blackened copper heated to 390°(732°)	blackened copper heated to 100°(212°)
Rock salt (diaphanous, colourless) ...	92	92	92	92
Fluate of lime (diaphanous, colourless)	78	69	42	33
Rock salt (diaphanous, dull)............	65	65	65	65
Beryl (diaphanous, greenish yellow)...	54	23	13	0
Fluate of lime (diaphanous, greenish)	46	38	24	20
Iceland spar (diaphanous, colourless)...	39	28	6	0
Another species (diaph., colourless)...	38	28	5	0
Mirror glass (diaphanous, colourless)...	39	24	6	0
Another kind (diaph., colourless)......	38	26	5	0
Rock crystal (diaphanous, colourless)	38	28	6	0
Rock crystal, smoky (diaph., brownish)	37	28	6	0
Acid chromate of potash (a vivid orange)	34	28	15	0
White topaz (diaphanous, colourless)	33	24	4	0
Carbonate of lead (diaph., colourless)...	32	23	4	0
Sulphate of barytes, pure, (diaphanous, rather dullish)	24	18	3	0
White agate (translucid, pearly)	23	11	2	0
Adularia felspar (diaph., dull, veined)	23	19	6	0
Amethyst (diaphanous, violet)	21	9	2	0
Amber, artificial (diaph., yellow)	21	5	0	0
Emerald (diaph. bluish green)	19	13	2	0
Agate, yellow (translucid, yellow)......	19	12	2	0
Borate of soda (translucid, white)	18	12	8	0
Green tourmaline (diaph., deep green)	18	16	3	0
Cowhorn (translucid, hazel)	18	4	0	0
Common gum (diaph., yellowish)	18	3	0	0
Sulphate of barytes (diaph., dull veined)	17	11	3	0
Sulphate of lime (diaph., colourless)...	14	5	0	0
Sardoine (translucid, brown)............	14	7	2	0
Citric acid (diaphanous, colourless) ...	11	2	0	0
Carbonate of ammonia (diaphanous, dull, striated)	12	3	0	0
Tartrate of potash and soda (diaphanous, colourless)......................	11	3	0	0
Amber, natural (translucid, yellowish)	11	5	0	0
Alum (diaphanous, colourless)	9	2	0	0
Glue, strong (diaph., yellowish brown)	9	2	0	0
Mother-of-pearl (translucid, white) ...	9	0	0	0
Sugar-candy (diaphanous, colourless)	8	0	0	0
Green fluate of lime (translucid, marbled green)..............................	8	6	4	3
Melted sugar (diaphanous, yellowish)...	7	0	0	0
Ice very pure (diaphanous, colourless)	6	0	0	0

Before we proceed to consider these results, it is necessary to recollect that they have all been obtained under the free action of an invariable radiation of 30° measured by the thermomultiplier. Now the half degrees of the galvanometer are very distinctly legible. Thus the transmissions are exact to $\frac{1}{60}$th of the incident heat; but the observations being repeated, the hundredth part becomes easily appretiable.

In the quantity of rays transmitted through the same substance there is a variation of several hundredth parts according to its greater or less purity. It was therefore useless in giving the measure of this element to attempt a degree of exactness exceeding the hundredth part of the whole; but it was desirable to ascertain the limits of the insensible transmissions with more precision. In this case therefore I have always carried the approximation to $\frac{1}{200}$, and sometimes to $\frac{1}{300}$, so that if the zero does not represent a transmission really equal to nothing, it is at least certain that, if there are any rays of heat transmitted, their amount does not exceed $\frac{1}{200}$dth of the whole incident quantity.

In order therefore to reduce the probability of error, it has been found necessary to operate on stronger radiations. Now the table of intensities given in my first Memoir does not exhibit the forces which move the galvanometric index beyond the 45th degree. I could have extended it to the higher degrees of the quadrant by the method followed in its construction. But I thought it better to employ at each step a very simple artifice which immediately gives the force of any radiation whatsoever as well as the required limit of error. To make this clear, let us suppose that it is desired to verify a particular case of the transmissions in the table; for instance, that it is requisite to prove that the transmissions of alum, sugar, or ice exposed to the rays emitted by copper heated to 390° are either null or less than $\frac{1}{200}$dth of the whole of the incident heat.

The table shows that a plate of glass, of rock crystal, or of Iceland spar transmits from five to six hundredths of those rays; that is to say, that for a free radiation of 30° we obtain about 2° through the plate. We know moreover that in this feeble indication there is a possible error of $\frac{1}{60}$th of the whole heat. The limit of error would be $\frac{1}{70{\cdot}6}$ if we wished to be rigorously exact, for by the table of intensities we see that, in the deviations below 20°, one degree is equivalent to $\frac{1}{35{\cdot}3}$ of the force which moves the needle to 30°. But let us admit only the limit $\frac{1}{60}$, which will have the advantage of rendering the values independent of a knowledge of the ratios existing between the degrees of the galvanometer and the corresponding forces of deviation. Let us bring the source near, in order that we may obtain through the same plate of glass a deviation exceeding 2°; a deviation, for instance, of 8°. The quantity of incident heat is now increased fourfold, and the probability of error is diminished in the same degree*. Let us now substitute for the plate of glass a flake of alum, sugar, or ice; we shall find that the needle of the galvanometer is perfectly at rest: if there is any heat transmitted, it is therefore not more than $\frac{1}{4} \cdot \frac{1}{60} = \frac{1}{240}$ of the whole radiation. Thus it is true that the transmission of these three substances reduced to plates of $2^{mm} \cdot 6$ in thickness and exposed to the radiation of a body heated to 390° is null or less than $\frac{1}{200}$dth part of the whole incident heat. It is by operations analogous to this that I have been able to ascertain the limits of the values of the zeros of transmission.

Now that we know the degree of exactness to which the measures contained in our table have been carried, we may proceed to state the consequences to which they lead.

Let us, for the moment, not notice the results obtained with the rock salt. The order of the transmissions has no relation to the degree of transparency, as we have already determined in our first series of experiments. It is not strictly the same when we change the calorific source; but each substance exposed to the successive action of the four radiations presents a like order of decrease in respect to the quantities which it transmits from each of the sources; that is to say, that all the substances transmit quantities of heat which are feeble in proportion as the temperature of the radiating source is low. There are several cases in which the transmissions are nothing; but these cases do not make

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[*] This mode of estimating the energy of the calorific radiations enables us to determine without difficulty the ratios existing between the arcs described by the magnetic needle of the galvanometer and the corresponding forces. Let us suppose the calorific source removed sufficiently far from the pile to produce but a feeble deviation of the galvanometer; one of 10°, for example. In the passage of the calorific rays let there be interposed a plate which transmits a certain fraction of the incident heat. We shall suppose this fraction to be $\frac{1}{5}$; the needle will descend to 2°. By bringing the source near, the deviation produced through the plate will be increased. Let us stop, when the needle shall have reached 4°, 6°, 8°, &c. successively; the calorific source will then emit upon the pile twice, thrice, or four times as much heat as before; for the transmission through the same plate exposed to a constant source of heat is always in a constant ratio, and the forces of deviation are proportional to the degrees in those arcs that are very near zero. Let the force which causes the galvanometer to describe the first degree of the scale be represented as 1, we shall then have 10 for the first force or quantity of incident heat, 20 for the second, 30 for the third, 40 for the fourth, &c. Now we know that the first force answers to 10°. In order to determine the deviation produced by the force 20 we have only to remove the plate when the galvanometer points to 4°; the calorific rays will then fall immediately on the pile, the angle of deviation will increase, and if the proportionality of the degrees to the forces continues through the whole extent of the arc of the first 20 degrees we shall see the index stop at 20°: at all events we shall have the corresponding indication. By repeating the same operation when the galvanometer points to 6°, 8°, we shall obtain the quantities sought, that is to say, the degrees answering to the forces 20, 30, 40, &c. Thus we may verify the results contained in the tables of intensities *already made*, or determine the elements necessary for the construction of *new* tables.

against the principle as the zero is never followed by appretiable transmissions.

The same principle holds in respect to all the liquids that I have been able to submit to experiment. It will be recollected that, in my mode of operating, the rays of heat, before they reach the liquid layer, must pass through a plate of glass. Now this substance becomes more and more interceptive in proportion as the sources employed are of a less elevated temperature, and consequently acts upon the calorific rays with an effect the same as that which a screen of variable transparency would produce in respect to light. The process therefore which I pursued in my first Memoir could not enable me to determine the exact ratios of the calorific transmissions through the same liquids when the source is changed; but it was possible to make it available for the purpose of establishing, in the greatest number of cases, the general law of decrease which we have just determined in respect to solid bodies.

Let us suppose that a thick plate of glass being submitted to the successive action of an equal quantity of heat, emanating from our four sources, gives these transmissions:

30, 18, 2, 0.

Let us suppose a parallelopiped, with sides parallel to the faces of the plate, to be cut out of the glass, and the cavity thus made to be filled with a given liquid: let us then suppose that the transmissions of the system become all respectively inferior to the preceding, and are reduced, for instance, to

20, 8, 1, 0,

it will be immediately concluded that the liquid acts on the calorific rays from different sources in the same manner in which its *glass case* does; that is, that it exhibits an order of decrease similar to that exhibited by the glass and by solid bodies in general. Now this is precisely the result furnished by the liquids contained in my glass vessels*.

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* In many instances I was unable to obtain any transmission, even by employing a very powerful radiation. It is thus that water, which transmits six or seven hundredths of the rays from a Locatelli lamp, completely intercepts the heat of the last three sources. Calculating the limit of error for the case least favourable to interception I found it $\frac{1}{616}$: the source was then brought very close to the liquid and an equal layer of oil employed, which caused in the index of the galvanometer a deviation of several degrees. Now if the water allows a passage to the radiation from bodies heated even to incandescence or brought to lower temperatures, the part transmitted must be less than $\frac{1}{616}$ of the incident quantity. I here speak of a layer of 3^{mm} or 4^{mm} in thickness: for it is possible and even very probable that layers much thinner than these may be in some slight degree permeable to rays of this kind. Thus we have seen glass of $0^{mm}\cdot07$ in thickness transmit $\frac{12}{100}$ of the rays emanating from boiling water, while a plate of 1^{mm} intercepted them totally. But as, in order to compare different transparencies, we must operate on a certain thickness of each medium (for the

In eight-and-twenty cases there have occurred but the three exceptions presented by carburet of sulphur, chloride of sulphur, and protochloride of phosphorus, in which the transmissions did not change when the liquid was substituted for glass. I found it therefore impossible to decide at first whether these three substances acted in the same manner as the others; for if they had acted even in a contrary way, provided their least transmission were equal to 30°, the result obtained would be the same. But in all probability these three anomalies are merely *apparent*; for the chloride of sulphur, the carburet of sulphur, and the protochloride of phosphorus being in a high degree permeable to radiant heat, the same thing will happen in respect to these three liquids inclosed in glass vessels that happens when very pure fluate of lime is substituted for them; that is to say, the transmissions of the system retain their proper values, though the fluate of lime itself be subject to the general law.

Thus the radiant heat from different sources is absorbed in greater or less proportions while it is passing through diaphanous bodies (solid or liquid); but while it is passing through *the same body* the absorption constantly increases as the temperature of the source decreases.

It happens quite otherwise to the luminous rays. Let us look through a plate of glass at the most vivid flame or at any other phosphorescent substance. If the plate is very pure, its interposition will produce no sensible effect, and the images will retain all the relations of intensity which they had when viewed directly. The pale phosphoric gleam therefore suffers in the interior of the glass screen the same absorption as the strong light of the flame does.

The bodies on which I have made my experiments have been taken indiscriminately from the three kingdoms of nature: some crystallized, others amorphous; some solid, others liquid; some natural, and others artificial: yet they all act in a similar order relatively to the rays of the different sources of caloric. Does not this constancy in their manner of acting, notwithstanding such great differences in their physical and chemical constitutions, indicate that this law of decrement belongs to the very nature of the heat? We should not however infer from this that there are not bodies which afford a passage equally free to calorific rays of every kind. For we see by the table that a flake of rock salt,

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most opake bodies become diaphanous when they are sufficiently attenuated), so, in order to judge of the calorific transmissions through different bodies, we must take the greatest possible care not to employ excessively thin plates, or at least, if we are compelled by particular circumstances to use such, the substances compared should be perfectly equal in thickness; for in that state of tenuity the least difference of thickness might disturb the order of permeability and cause us to attribute a greater calorific transparency to substances possessing this property in an inferior degree. This is probably the cause of the mistake into which those have fallen who have fancied that they could prove by their experiments that water is more diathermanous than glass.

whether exposed to the radiations of flame, of incandescent platina, of copper heated to 390°, or of boiling water, always transmits 92 of every hundred incident rays.

The same constancy of transmission is observable when we operate on sources of a temperature yet lower than that of boiling water; such, for instance, as vessels containing this liquid heated to 40° or 50°. It is observable also when we employ pieces of rock salt 15^{mm} or 20^{mm} thick. I have placed all the flakes of salt that I could dispose of side by side, so that the thickness of them all amounted to 86^{mm}. The quantity of heat transmitted by this series of flakes was considerably less than $\frac{92}{100}$, because of the great number of successive reflexions; but it was always invariable relatively to the four sources. Between these limits of thickness, therefore, *rock salt really acts in respect to radiant heat just as colourless glass and colourless diaphanous bodies in general act in respect to light.*

This being premised, it is clear that if each substance contained in the table acted like the second specimen of rock salt, that is, if it transmitted the heat in a proportion less than $\frac{92}{100}$ but always the same for each of the four sources, all these substances would be to radiant heat that which diaphanous bodies *more or less dusky* are to light. But they allow the rays from certain sources to pass through them and intercept the rays from others: they act therefore in respect to heat as coloured media act on light*.

What do we find when we expose the same coloured glass successively

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* It appears that Sir David Brewster had lately arrived at the same conclusion by means only of the experiments of Delaroche and Seebeck on the transmission through glass and on the distribution of heat in the solar spectra produced with different prisms. (See *Report of the First and Second Meetings of the British Association for the Advancement of Science.* London, 1833, p. 294.) But these experiments did not prove that the rays in passing through the different bodies suffer *a real internal absorption analogous to that which light suffers:* above all, they were far from proving that this absorptive force, varying in each substance according to the temperature of the calorific source, *could,* in some particular cases, *become constant, and in all respects similar to the action of colourless diaphanous media on luminous rays.* On this ground it may be said that the inference of Brewster was yet premature; besides, the illustrious Scotchman rested his conjectures on the erroneous supposition that water has the same absorbent force in respect to all sorts of calorific rays. Experiment indeed leads to the opposite conclusion, as we have already proved in respect to solar heat by the different action of a layer of water on the temperatures distributed in each band of the solar spectrum; an action so widely different relatively to two different rays that all the heat of the violet light passes through the liquid without suffering any sensible diminution, while the nonluminous heat of the isothermal band is totally absorbed, (*Annales de Chimie et de Physique,* December 1831,) and we have just seen in the preceding note that analogous phænomena are observable in the radiations from terrestrial sources also; for a mass of water some millimetres in thickness intercepts all but a very small portion of the radiant heat issuing from flame and the whole of those rays that issue from any other source.

to differently coloured lights? Lights of the same tint as the glass pass abundantly, the rest are almost totally intercepted.

These analogies lead us therefore to consider the radiations from different sources of heat as not being of the same nature. This seems indeed sufficiently established by the mere fact that the calorific transmission of glass, Iceland spar, or any other diathermanous body varies with the temperature of the radiating source.

Thus boiling water, copper heated to 390°, incandescent platina, and the flame of oil will be to us the sources of a heat that is more or less *coloured*, that is to say, sources each of which gives out a greater quantity of calorific rays of a certain quality; but the flame will furnish caloric rays of every kind as it furnishes light of all colours.

We shall distinguish bodies into diathermanous and athermanous*. The diathermanous we shall subdivide into *universal* and *partial*. The first of these subdivisions, which is analogous to colourless media, will contain but one substance, namely, rock salt; the second, which corresponds with the coloured media, will contain all the bodies comprised in our table, in addition to diaphanous liquids and diaphanous substances in general.

As to the class of athermanous bodies I had supposed at first that every substance which completely intercepted light intercepted the whole of the radiant heat also. This is found to be the fact in the greatest number of cases. But subsequent experiments have shown me that flakes of black mica and black glass, though they completely intercept the most intense solar light, yet exhibit very strongly marked calorific transmissions. The following are the results:

	Transmissions out of 100 rays issuing from			
	a Locatelli lamp.	incandescent platina.	copper at 390°.	copper at 100°.
Black glass (1^{mm} in thickness)	26	25	12	0
Ditto (2^{mm} ditto)	16	15·5	8	0
Black mica (0^{mm}·6 ditto)	29	28	13	0
Ditto (0^{mm}·9 ditto)	20	20	9	0

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* Athermanous, in contradistinction to diathermanous, evidently signifies the absence of the power of transmitting heat. I adopt this term merely for convenience, without attaching to it a definite meaning; for, as there is no body which, if reduced to an extremely thin plate, may not become in some degree transparent, I think also that some rays of heat may pass through all substances in a state of great tenuity.

The black mica and black glass then, though perfectly opake, are diathermanous, but yet only partially diathermanous, because while they allow some rays of heat to pass they intercept others.

We may see, besides, that the heat of incandescent platina and that of the flame of oil are transmitted in nearly equal quantities by these two substances. As soon as I had made my first experiments on the transmission of opake bodies I found that the rays from incandescent platina pass through a plate of black glass in a greater proportion than those from an Argand lamp. Now as it happens quite otherwise in respect to transparent glass and other diathermanous bodies, I thought at first that, in the particular case of the black glass, the variation in the quantity of heat transmitted was inversely as the temperature of the radiating source*. But it was not long before I discovered my mistake; for, exposing two flakes of glass, the one colourless and the other opake, first to the direct rays of a Locatelli lamp and next to the rays that passed through a screen of common glass, I found that if the transmission through the first plate increases, as I have already stated in my first Memoir, the transmission through the second *decreases*. These opposite variations exhibited by the transmissions of the black and the white glass relatively to the radiations from the Argand lamp and the incandescent platina, do not arise from any peculiar action of the calorific sources on the two bodies, but from a particular modification which the cylindrical screen or glass funnel attached to the Argand lamp produces in the calorific rays passing through it,—a modification which changes their capability of ulterior transmission and enables them to pass through the other bodies in a greater or less quantity than if they were in their natural state.

We shall presently see that almost all the screens produce analogous effects.

The similarity of the action of glass and transparent bodies in general upon radiant heat to that of coloured media upon light, is established even in its most minute details by all the phænomena of transmission that we have been able to observe. For we have seen that the calorific rays from the flame of an Argand lamp lose much of their intensity while passing into the interior of a thick piece of colourless glass, and that their subsequent losses decrease in proportion as the distance from the surface at which they enter increases. Now the same thing takes place if we expose to white light any coloured transparent body, a red liquid, for instance; for in this case nearly all the rays, blue, green, yellow, &c., which enter into the composition of this light are absorbed more or less rapidly by the first layers of the liquid, and the red rays alone penetrate to a certain depth.

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* *Bulletin de la Société Philomatique*, July 1833.

It is also known from the experiments of Delaroche and others that the radiant heat which has traversed a plate of glass and suffered a certain loss will in passing through a second plate sustain a second loss proportionally less than the first. In the same manner does the *incident* white light in passing through the first layer of a coloured substance become considerably weaker, while the *emergent coloured* light passes almost without suffering any diminution of intensity.

By exposing a given plate of a diaphanous substance successively to equal quantities of calorific rays from different sources we have seen their transmissions vary with the temperature of the source, that is to say, with the nature of the rays emitted. We have seen moreover that the differences between one transmission and another decrease in proportion as the plates employed are thinner, until within a certain limit of tenuity they vanish or have a tendency to vanish altogether. All these effects are observable in the differently coloured lights transmitted through a coloured medium; for if the medium be red the quantities of light transmitted will be greater in proportion to the greater number of red rays contained in each radiation. The other rays will be absorbed in a greater or less degree. But the quantities of light transmitted approach more nearly to an equality in proportion as the plate to be passed through is thinner. In short, the coloured media become more faint as their mass is reduced, and when sufficiently attenuated retain no sensible tint whatsoever, in other words, they become permeable to luminous rays of all colours.

We have several times remarked the striking differences exhibited in the calorific transmissions of diaphanous substances. But this curious fact, which constitutes, as it were, the basis of our inquiries, ceases to surprise us as soon as we feel convinced that bodies which are transparent and colourless act upon heat in a manner similar to that in which coloured media act upon light. For, as upon the intensity of the colour depends the degree of transparency, that is, the number of luminous rays that pass through the coloured substances, in like manner upon this species of *invisible calorific tint* which diaphanous bodies possess will depend whether a greater or a less quantity of heat be transmitted*.

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* Seeing that in respect to all the substances given in the table, the rock salt excepted, the order of decrement is similar though the sources of heat are different, one might be inclined at first to infer that they belong to the same species of partially diathermanous bodies, that is, that they may be compared with coloured media. But that such a conclusion is not legitimate will be shown by one example: let *a* be the species of rays transmitted by the medium A, *b* that species which is transmitted by the medium B, and *c* the rays intercepted by the same media. Let us suppose a calorific source that will give 30 *a*, 30 *b*, and 40 *c*; it is clear that the two media A and B will intercept 70 parts of the hundred and transmit 30. However, the rays emerging from A will be different from those which emerge from B. If we suppose a second source of heat such as will give 20 *a*, 20 *b*, and 60 *c*, we shall have 80 as the quantity intercepted and

We shall presently see yet more striking analogies between the two classes of phænomena when we consider the modifications which the calorific rays undergo in their passage from one screen to the other. But before we dismiss the present subject it may be advisable to bestow a few moments' attention on the purposes to which the calorific properties of rock salt may be applied.

Glass is a substance but very slightly diathermanous, especially when the temperature of the source is low. The common prisms or convex lenses could not therefore be employed for the purpose of ascertaining whether radiant heat be subject to changes of direction analogous to those of light in penetrating to the interior of refracting media. It was owing to the use of such instruments that some who applied themselves to the investigation of this point attained but very indecisive results, and often drew from them very false conclusions. Scheele asserted that "bright points not possessing the least heat may be formed before the fire with burning-glasses*." Carefully conducted experiments have more recently shown that a thermometer rises some degrees when placed in the focus of a lens exposed to the radiation of flame or of incandescent bodies†. But as the heat is then luminous, and as no very decided effect is observed if the operation is performed with nonluminous heat, it was inferred that the elevation of temperature was owing to the light absorbed by the thermometer and that isolated radiant heat is not susceptible of refraction. . This notion might derive additional support from the fact that lenses of rock crystal, Iceland spar, alum, and other diaphanous substances acted analogously to the glass lens: and yet it would have been wrong to attribute to the agent an effect which was due only to the particular structure of all those substances. To be satisfied of this we need only operate with a lens of rock salt; for the focal thermometer then always exhibits a marked elevation of temperature, even though the radiant heat be totally separated from the light. But it has been attempted to explain the effect of the lenses by an inequality in the heating of their different parts. It has been said that the heat is accumulated towards the centre, that the parts towards the margin, because of their thinness, quickly grow cold again, and that it is not surprising therefore to see the thermometer rise more rapidly when placed in the prolongation of the axis of the lens than in any other direction‡. It would however still remain to be explained why the experiment is no

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20 as the quantity transmitted by each of the screens. If the source gave 10 *a*, 10 *b*, and 80 *c*, the transmission would be 10 and the interception 90. Thus two substances exposed to different radiations may furnish calorific transmissions not only varying according to the same order of decrement, but equal in all their periods of variation, although the rays emerging from each may be of a different kind.

* Scheele, *Traité de l'Air et du Feu*, Paris, 1778, § 56.

† W. Herschel and Brande, Philosophical Transactions for 1800 and 1820.

‡ Philosophical Transactions, vol. cvi.

longer equally successful when for the salt we substitute alum or any other diaphanous substance. But as recourse might be had to supposed differences between the conducting, the absorptive, or the emissive powers of these bodies, it seems advisable first to prove the refraction of the nonluminous rays without using lenses.

With this view I place, at a certain distance from the thermoelectric pile and out of the direction of its axis, a plate of copper heated to 390° by an alcoholic lamp, or, what is still better, a vessel filled with water in a state of ebullition. The pile being lodged at the bottom of a metallic tube blackened inside, the rays of nonluminous heat emitted from the vessel in a direction oblique to the axis cannot reach the thermoscopic body, and the index of the galvanometer remains perfectly at rest. Matters being now in this state, I take a prism of rock salt and fix it at the mouth of the tube with its axis placed vertically and its refractive angle turned towards the angle formed by a line drawn from the source to the extremity of the tube. (See Plate I. fig. 2.) A considerable deviation is immediately perceived in the galvanometer. The rays of heat are therefore conveyed into the tube by the action of the prism.

To show that the effect is really due to the refraction and not to the heat of the salt it will be sufficient to turn the angle of refraction in a contrary direction; for as soon as this is done the needle falls again to zero, notwithstanding the presence of the prism. The experiment is no less successful with the heat of the lamp, or that of the incandescent platina. *Calorific rays of every kind are therefore,* like luminous rays, *susceptible of refraction.*

But on the principle of analogy, as each species of light, so will each species of heat possess a different refrangibility. Hence it is evident that if the prism be left in its position and the radiant source changed it would become necessary at the same time to change the angle formed by the axis of the pile with the direction of the rays, in order to obtain the desired effect on the galvanometer. If however we attempt to verify this conjecture we obtain no decisive result. This is easily conceived when we reflect that the aperture of the tube has a certain diameter and that it is placed quite close to the prism, so that the rays refracted at angles differing but very little from each other can always reach the pile though no change should be made in the inclination of the axis of the tube.

But there is another process by means of which, if we cannot exactly measure the refrangibility of each species of calorific rays, we prove at least that the angle of refraction varies with the measure of the radiating source. I took a graduated circle ABC (Plate I. fig. 3.) 22 inches in diameter carrying a ruler CD as a moveable radius. At the extremity of this ruler I placed a thermoelectric pile M composed of fifteen pairs disposed in one line perpendicular to the plane of the circle.

This apparatus being placed horizontally on a table, the centre C was brought within a little distance of the bottom of a vertical prism (N) of rock salt, so that when the ruler CD was properly placed the refracted parcel of hot rays fell on all the points of the linear pile.

By establishing the electric communications with the galvanometer and moving the ruler over the graduated arc, the point at which the deviation of the magnetic index attained its greatest value was easily determined. The radiating source was then changed while everything else was allowed to remain in the same state. We had now a calorific action more or less intense than the preceding; but in order to obtain the maximum of effect it was necessary to slide the ruler in one direction or the other. Thus, for instance, when I commenced the experiment with the incandescent platina, that is, when I had found the corresponding position of the pile that gives the greatest galvanometric deviation, it was necessary to move the ruler about two lines towards B, on the side to which the *most refrangible* rays are directed, if I substituted the Locatelli lamp for the platina. But if I substituted for the platina a plate of copper heated to 390° I was obliged to slide the ruler three lines towards A, in the direction of the *less refrangible* rays. The action of the boiling water in this experiment was too feeble to be compared with that of any of the three other sources.

The refraction and constant transmission of the calorific rays through the rock salt being placed beyond the possibility of doubt, we immediately see the use that may be made of this substance in investigating the nature of radiant heat. If, for instance, it is proposed to propagate to great distances the action of a heated body of small dimensions, we are now certain that we have only to place the body at the focus of a lens of rock salt, which will refract the calorific rays and make them form a real *pharos of heat* by issuing in a direction parallel to the axis. Is it desired that extremely feeble rays emanating from any source should be rendered perceptible? Let them be received on a lens of this substance having a thermoscopic body placed in its focus. In this manner we may, with the aid of an ordinary differential thermometer with small balls, obtain very decided indications of the heat issuing from a vessel filled with tepid water and placed at a great distance. In short, rock salt formed into lenses and prisms acts upon calorific rays in a manner perfectly analogous to that in which optical instruments act upon luminous rays. It constitutes then the *true glass* of radiant heat, and therefore the only glass that should be employed in appreciating the effects of its intensity. All other transparent bodies are but partial and incomplete transmitters of heat, totally intercepting calorific rays of a certain kind. It is easy to conceive, from these considerations, with what serious disadvantages those persons have had to contend who have undertaken to investigate the composition of solar heat with common prisms of flint or crown glass, water, alcohol, or some other diaphanous body. It was exactly the same as if they pretended to be able to analyse solar light with a prism formed of coloured glass.

Of the properties of the calorific rays immediately transmitted by different bodies.

The radiant heat which has passed through a plate of glass is transmitted in a greater proportion by a second plate of the same substance and the same thickness; the rays issuing from the second will be transmitted in a still greater proportion by a third, and so through any number of successive screens. The losses sustained by the calorific rays in their passage through a succession of screens, as compared with the quantity incident on each plate, will therefore form a decreasing series. But the difference between every two terms of this series becomes less and less as the number of terms increases, so that there must be somewhere a limit beyond which the difference has a tendency to vanish. We may conclude therefore that the rays after they have passed through a certain number of screens, will in their further transmission be subject to a loss reducible to a constant quantity as compared with the quantity of heat incident to each of the screens through which this further transmission is made.

The same phænomena may be traced in a continuous mass of diathermanous matter; that is to say, that if we imagine a piece of glass divided into several equal layers and measure the loss sustained by the radiant heat in its passage through each layer, the greater the distance of the layer from the surface at which the heat enters, the less will be the diminution suffered by the rays passing through that layer, and the losses have a tendency to become constant within a limit depending on the thickness of the layers. Some of these results we have already verified in the preceding memoir, and it is easy to establish their truth, in reference to the sources of heat employed in our present inquiry, by means of the numbers which represent the transmissions of the plates contained in the first table[*].

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[*] Let us imagine the screen of 8^{mm} divided into seven layers having for their degrees of thickness the differences between two consecutive plates. (See the first table in this Memoir.) The quantities of heat incident on the layers when the radiation is from a Locatelli lamp are

$$100,\ 77,\ 54,\ 46,\ 41,\ 37,\ 35,\ 33{\cdot}5,$$

and the quantities lost in the successive transmissions are

$$23,\ 23,\ 8,\ 5,\ 4,\ 2,\ 1{\cdot}5.$$

Now the mean losses for the hundredth part of a millimetre of each screen will be

$$\frac{23}{7},\ \frac{23}{43},\ \frac{8}{50},\ \frac{5}{100},\ \frac{4}{100},\ \frac{2}{100},\ \frac{1{\cdot}5}{100},$$

or $3{\cdot}286,\ \ 0{\cdot}535,\ \ 0{\cdot}160,\ \ 0{\cdot}050,\ \ 0{\cdot}020,\ \ 0{\cdot}010,\ \ 0{\cdot}007.$

Hence the losses sustained by the rays of the lamp in the first hundredth part

The only difference observable between the transmission through a continuous medium and the transmission through a series of detached screens is in the amount of the losses, which, for a given thickness, are found to be greater in the latter, because of the reflexions produced by each separate surface.

These facts cannot surprise us after the idea we have formed to ourselves of the influence exercised by diaphanous substances on radiant heat. For the calorific sources always emit a certain portion of rays heterogeneous (if we may use the expression) to the *calorific tint* of the glass, which, through the absorbent action of the matter constituting the continuous medium or the detached screens, are successively extinguished until no rays remain but those that are homogeneous to this tint. Now these homogeneous rays must suffer a loss greater or less in its amount, but constant in respect to layers of equal thickness, as is the case, in the transmission of light, with red rays passing through a medium of the same colour, and with white rays passing through a medium diaphanous and colourless. What we have said of glass is equally true of every other partially diathermanous substance.

The calorific transmission through a series of homogeneous screens is then absolutely of the same nature as that which is effected through the

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of a millimetre of each layer, when referred to the quantities of incident heat, will have the values

$$\frac{3{\cdot}286}{100} \quad \frac{0{\cdot}535}{77} \quad \frac{0{\cdot}160}{54} \quad \frac{0{\cdot}050}{46} \quad \frac{0{\cdot}020}{41} \quad \frac{0{\cdot}010}{37} \quad \frac{0{\cdot}007}{35}$$

that is, 0·0328 0·0070 0·0030 0·0011 0·0005 0·0003 0·0002.

By similar calculations the successive losses sustained by the radiations from the incandescent platina and the copper heated to 390° will be found to be

0·0614 0·0081 0·0032 0·0019 0·0010 0·0005 0·0003
0·0943 0·0155 0·0050 0·0022 0·0014 0·0010 0·0008.

Now the differences between every two terms of these series are for the

1st, 0·0258 0·0040 0·0019 0·0006 0·0002 0·0001;
2nd, 0·0523 0·0049 0·0013 0·0009 0·0005 0·0002;
3rd, 0·0780 0·0105 0·0028 0·0008 0·0004 0·0002.

As to the fourth source it is useless to speak of it, as its rays are completely extinguished at the distance of one millimetre.

Thus, notwithstanding the inequalities of the increase of the distance from the second and the third layer to the surface of entrance, we observe in the three series the two principles we have laid down, namely, 1st, the decrease of the losses; 2nd, the tendency of this decrease towards a limit at which the loss becomes constant: but for each particular case the points of the medium at which the rays begin to suffer this constant action are evidently placed at a fixed distance from the origin. Therefore, if the glass be divided into equal layers, the limit of the decrease of the losses will be attained more slowly in proportion as the layers are more numerous, that is to say, thinner. It is for this reason that in each series the limit at which the losses become constant depends, as we have already said, on the thickness of the elementary layers.

interior of one continuous medium. This transmission we have examined, and, as we have just seen, it presents nothing contrary to its analogy with the transmission of light through coloured media. There is however a particular case in which two homogeneous screens act in so singular a manner in respect to light that it must be interesting to know whether something analogous does not take place in respect to caloric.

The optical phænomena presented by most of the slices of tourmaline cut parallel to the axis of crystallization are universally known. If these slices are placed one over the other and their axes laid in the same direction, they transmit light in considerable quantities. But if they be laid at right angles to one another, the light is totally intercepted. Do these phænomena, arising, as is well known, from the polarization of the light in the interior of the slices, take place in respect to calorific rays also; or, in other words, is radiant heat capable of being polarized in its passage through tourmaline?

In order to ascertain this I have taken two square plates of the same dimensions. I have made an aperture in the centre of each. This aperture was likewise a square having its sides parallel to those of the plate and each equal to the least breadth of the two polarizing slices. I then took some soft wax and attached a tourmaline to each aperture, holding the axis of the former parallel to one of the sides of the latter. These two plates being laid one over the other, it evidently depended on one of the sides of the one plate being placed parallel or perpendicular to a side of the other whether the light was to be transmitted or intercepted. Yet this pair of plates being placed vertically on the stand of my thermoelectric apparatus and exposed to the radiation of a lamp or incandescent platina, uniformly produced the same calorific transmission, whatever might be the relative direction of the sides of each plate.

That this fact might be put beyond the reach of doubt the galvanometric index was carried to the 18th or 20th degree, and the calorific communication now established was suffered to remain while we placed one of the plates on each of its sides in succession. The flame or the incandescent platina was then observed to appear and disappear alternately while the magnetic needle continued invariably at the same point of deviation.

This experiment was repeated many times with several tourmalines, and the angle formed by the intersection of their axes varied. The result was in all cases the same. The quantity of calorific rays transmitted through the two polarizing slices is then independent of the respective directions given to their axes of crystallization; that is to say, the heat radiating from terrestrial sources is not polarized in its passage through tourmalines*.

* This result seems opposed to the experiments of M. Bérard on the polarization of reflected heat; but, ignorant as we are of the nature of the relations

Let us now proceed to consider the transmission of heat through heterogeneous screens. The calorific rays emerging from each plate exposed to the action of the same source produce a particular elevation of temperature when they fall on the thermoscopic body of our apparatus. Whence we have inferred that the *quantity* of heat which passes through a given screen varies according to the quality and thickness of the substance. But, it may be asked, is this the only difference between the rays immediately transmitted through bodies of different kinds?

For the purpose of answering this question we have made the following experiments.

If the rays from a Locatelli lamp be brought to act on a thermoelectric pile after having previously passed through a screen of diaphanous matter (such as citric acid) but in a slight degree permeable to radiant heat, the effect obtained in the ordinary case, in which the whole action is equivalent to 30° of the thermomultiplier, will be very inconsiderable; but it may be increased by bringing the source of heat nearer, or by concentrating its rays on the plate with the help of metallic mirrors or lenses of rock salt. I suppose then that a deviation of 25° or 30° of the galvanometer has been produced through a plate of citric acid. I now interpose a plate of alum in such a manner that the rays emerging from the citric acid may be forced to pass through it before they can reach the thermoscopic body; the magnetic needle descends only about 3 or 4 degrees.*

I now recommence the operation on any other diaphanous and colourless substance different from the citric acid; that is to say, I vary the distance from the lamp to the pile until I obtain the same galvanometric deviation of 25° or 30° by the action of the radiant heat on this new substance also. I then interpose the plate of alum, and the magnetic index, as in the case of the citric acid, descends again not more than about 3 or 4 degrees, but it approaches nearer to zero, and the retro-

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that caloric and light bear to one another, we have no means of proving that, as no polarization of heat is produced by the transmission through the tourmalines, none can be produced by reflexion at the surface of the glass. I am bound also to remark that some very able experimental philosophers having lately tried to polarize light by M. Bérard's process, their efforts proved unavailing. Mr. Powell informs us that although he had taken the necessary precautions against the heating of the glass and other causes of error he has never been able to discover the least appearance of polarization when operating with nonluminous heat. But he *thinks* that when he employed luminous sources he was enabled to observe a small perceptible effect by making the rays previously pass through a screen of glass (Edinb. Journal of Science, N. S., vol. vi.) Mr. Lloyd communicated at the last meeting of the British Association for the Advancement of Science (Cambridge 1833) some new results tending to support the conclusions derived by Mr. Powell from his own experiments. [No communication upon this subject by Professor Lloyd appears in the Report of the British Association for 1833.—EDIT.]

grade movement is sometimes so marked that the needle nearly resumes its natural position of equilibrium.

If instead of alum other substances were employed as the invariable plate on which the rays issuing from each diaphanous body are successively made to fall, we should still observe differences in the corresponding deviations of the galvanometer; but they would be in general of a less decided kind. It is on this account that we have preferred the alum.

The following are the results, in hundredth parts, of the constant quantity of heat that falls on the plate of alum:

Screens from which there issue 100 rays of heat which are made to fall successively on the same plate of alum.	Number of rays transmitted by this plate.
No screen	9
Rock salt (limpid)	9
Rock salt (dull)	9
Borate of soda	11
Adularia felspar	14
Iceland spar	22
Rock crystal	25
Mirror glass	27
Carbonate of ammonia	31
Sulphate of lime	72
Tartrate of potash and soda	80
Citric acid	85
Alum	90

We see that radiations of the same intensity emanating from the diaphanous and colourless bodies contained in the tables pass through the same plate of alum in very different quantities. In the same manner sheaves of luminous rays issuing from different coloured media are transmitted some in greater and others in less proportions by a second transparent substance equally coloured, as the tint of each medium happens to be more or less analogous to that of the invariable substance through which they are to pass.

The calorific rays issuing from the diaphanous screens are therefore of different *qualities* and possess (if we may use the term) the *diathermancy* * peculiar to each of the substances through which they have passed. The citric acid, the tartrate of potash and soda, and the sulphate of lime transmit rays which pass abundantly through alum; the

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* I employ the word *diathermancy* as the equivalent of calorific coloration or *calorific tint*, lest the latter should be confounded with tints or colours properly so called. The word has been suggested to me by M. Ampère, who has continued to assist me with his valuable advice in the composition of this Memoir, for which I here take the opportunity to tender him my grateful acknowledgements.

diathermancy of these bodies therefore approximates nearly to that of the alum. The glass, the rock crystal, and the Iceland spar have evidently a different diathermancy, for the rays which pass through them are less transmissible by the invariable screen. The same may be said of borax, adularia, and carbonate of ammonia. As to the heat emerging from rock salt (limpid or dull) it acts in a manner similar to that in which the unobstructed light of the lamp would. The reason is evident, since the salt, acting equally on the different species of calorific rays, must transmit them all without reflecting their relative properties in any manner whatsoever.

These facts then completely confirm the conclusions which we had drawn from the preceding experiments: namely, that, 1st, flame sends forth rays of several kinds; 2nd, that diaphanous colourless bodies, with the exception of rock salt, act so as to extinguish certain caloric rays and allow others to pass, just as coloured media act in respect to light.

Here a very interesting question is naturally suggested. If the diathermancy or quality which constitutes the tint of a medium relatively to the radiant caloric is invisible, what part then do colours act in the transmission of heat?

When the quantity of radiant heat that passes through coloured glass is measured, it is always found to be less than that which passes through white glass of the same thickness. The difference indeed is sometimes considerable, though having no apparent relation to the prismatic order or intensity of the colour. We have already remarked this in the first memoir, and the truth of the remark will be readily admitted by any one who casts an eye over the following little table.

Screens of glass exposed to the radiation of a Locatelli lamp. (Common thickness $1^{\mathrm{mm}}\cdot85$.)	Transmissions out of 100 rays of heat.
Glass, white	40
— red (deep)	33
— orange	29
— yellow (brilliant)	22
— green (apple)	25
— green (mineral)	23
— blue	21
— indigo	12
— violet (deep)	34
— black (opake)	17

It is therefore not to be doubted that an absorption of caloric is caused by the colouring matter. But is the power of absorption *elective* like the action of the invisible calorific tints in colourless diaphanous bodies, or does it affect all sorts of rays indiscriminately? We are about to investigate this point by means of experiments similar to the preceding, in which we have taken equal quantities of heat issuing from different screens of differently coloured glass, in order to make them pass through one common screen of alum.

Screens from which the 100 rays issue that are made to fall successively on the same slice of alum.	Number of rays transmitted by this slice.
Glass, white	27
— red	27
— orange	27
— yellow	27
— green (apple)	5
— green (mineral)	3
— blue	27
— indigo	27
— violet	27
— opake (black)	1

We see here that the rays emerging from the red, orange, yellow, blue, indigo, and violet are transmitted through the plate of alum in the same proportion as the rays that issue from the white glass. The colouring matter introduced into the composition of these different kinds of glass has no other effect than to extinguish part of the calorific sheaf which passed through the white, without perceptibly altering the relations of quantity between the several species of rays of which that sheaf is composed: they act in respect to radiant heat just as brown or blackish substances dipped in a transparent fluid would act in respect to light. But the case is different with respect to green and opake black; for these being introduced into the composition of glass, it will stop nearly all the rays that the alum is capable of transmitting. This effect arises from the green or opake colouring matter producing a certain modification in the diathermancy of the glass, and we have just seen that this species of *calorific colouration* is invisible and totally independent of coloration properly so called, since it exists in bodies possessing the greatest transparency. It is then extremely probable that the black or the green should not be supposed to enter as mere neutrals into this phænomenon, which will thenceforth depend on such or such a property of these colouring materials. I have found, in fact, some green glasses, which produced a much feebler action than others of the same tint but possessing a less brilliant colouration. The green glasses which act most powerfully are of a bluish cast; from which circumstance it would seem to follow that they contain a considerable quantity of oxide of copper. Whatever may be said of this singular property of green and black opake glasses and the cause by which it is produced, it is nevertheless an indisputable fact which every man can easily verify and of which we intend to give some new proofs presently. But it will perhaps be advisable previously to adduce the results furnished by several diathermanous substances examined by that process to which we have submitted the coloured glasses and the diaphanous colourless bodies.

Screens emitting 100 rays of heat which are made to fall successively on the same slice of alum.	Number of rays transmitted by this slice.
Mica, black opake	2
Tourmaline, green	7
Sulphate of barytes	12
Acid chromate of potash	14
Mica, white	15
Beryl	19
Emerald	19
Agate, pearly	24
Agate, yellow	24
Amber, yellow	30
Gum	45

On these numbers we have two remarks to make: first, that the green tourmaline and the black mica act in a manner analogous to glass of the same colour; second, that the beryl and emerald emit rays equally transmissible by the alum, although the colours of these two kinds of the same substance are different. The same happens to the two kinds of agate. These facts may perhaps be turned to some account by the mineralogist in examining certain coloured substances which belong to the different varieties of one mineralogical species.

We have been hitherto investigating the action of alum on a constant quantity of rays emerging from several diathermanous substances. Let us now reverse the problem and see what will be the effect when these substances are interposed in the passage of an invariable radiation issuing from alum.

In the third column of the following table will be found the results furnished by this class of experiments. It is almost unnecessary to observe that they have been obtained by successively placing the several bodies between the alum and the pile, after having produced in the galvanometer the ordinary deviation of 30° through the first substance. I have placed in the columns after the third the values of the transmissions of the same bodies exposed to the rays emerging from four substances different from alum; namely, sulphate of lime, acid chromate of potash, and green and black glass. The natural values of the calorific transmissions, that is to say, the results obtained under the immediate action of the lamp, are indicated in the second column.

Names of the substances interposed. (Those plates whose thickness is not specially indicated have the common thickness of $2^{mm}{\cdot}6$.)	Transmissions out of 100 rays.					
	Rays immediately from the lamp.	Rays emerging from the alum, (thickness $2^{mm}{\cdot}6$.)	Rays emerging from the sulphate of lime, (thickness $2^{mm}{\cdot}6$.)	Rays emerging from the chromate of potash, (thickness $2^{mm}{\cdot}6$.)	Rays emerging from the green glass, (thickness $1^{mm}{\cdot}85$.)	Rays emerging from the black glass, (thickness $1^{mm}{\cdot}85$.)
Rock salt	92	92	92	92	92	92
Fluate of lime	78	90	91	88	90	91
Beryl	54	80	91	66	70	57
Iceland spar	39	91	89	56	59	55
Glass (thick. $0^{mm}{\cdot}5$)	54	90	85	68	87	80
Glass (thick. 8^{mm})	34	90	82	47	56	45
Rock crystal	38	91	85	52	78	54
Chromate of potash	34	57	53	71	28	24
Sulphate of barytes	24	36	47	25	60	57
White agate	23	70	78	30	43	17
Adularia felspar	23	23	58	43	50	23
Yellow amber	21	65	61	20	13	8
Black opake mica (thick. $0^{mm}{\cdot}9$)	20	0·4	12	16	38	43
Yellow agate	19	57	64	24	35	14
Emerald	19	60	57	26	20	21
Borate of soda	18	23	33	23	30	24
Green tourmaline	18	1	10	14	24	30
Common gum	18	61	52	12	6	4
Sulphate of lime	14	59	54	22	9	15
Sulphate of lime (thick. 12^{mm})	10	56	45	17	5	0·4
Carbonate of ammonia	12	44	34	11	6	5
Citric acid	11	88	52	16	3	2
Tartrate of potash and soda	11	85	60	15	2	1
Alum	9	90	47	15	0·5	0·3
Coloured glasses, (common thickness $1^{mm}{\cdot}85$.)						
Glass, white	40	90	83	50	67	55
— violet	34	76	72	42	56	47
— red	33	74	69	41	54	45
— orange	29	65	58	36	48	39
— green (apple)	25	3	20	22	55	50
— green (mineral)	23	1	15	19	52	58
— yellow	22	49	46	27	35	30
— blue	21	47	42	26	34	29
— black (opake)	16	0·5	18	11	42	52
— indigo	12	27	26	14	20	17

Several of the numerical results contained in this table may be verified by calculation.

For, when two plates of different kinds are exposed together to the radiation of the source, their position relative to the entrance and the issue of the calorific rays does not affect the quantity of heat which passes through this system. This is easily proved by putting the first plate in the place of the second; for the thermomultiplier, notwithstanding this change of order, continues to mark the same degree of its scale. Let us now take two plates and place them alternately in each of the two positions, for instance, the plate of alum and the chromate of potash. These two substances, exposed separately to 100 rays of heat emanating directly from the source, transmit 9 and 34 respectively. The quantities of heat that should fall on each of the two plates in order that 100 may emerge in each case is easily determined by these simple proportions:

$$9 : 100 :: 100 : x,$$
$$34 : 100 :: 100 : x,$$

which give 1111 for the alum and 294 for the chromate of potash. Now we know by experiment that chromate of potash exposed to 100 rays issuing from alum transmits 57, and that alum exposed to 100 rays issuing from chromate of potash transmits 15.

But the order of succession has no influence on the transmission of the pair: let us therefore reverse the system only in one case or the other. We shall then have the same plates exposed in the same manner to the two radiations of 1111 and 294. The quantities transmitted under both circumstances should accordingly be proportional to the incident quantities, as is actually proved within the limits of approximation compatible with the nature of the experiments; for we have,

$$57 : 15 :: 1111 : 294.$$

The table contains ten pairs which are submitted in both ways to the radiations of the source; there are in it consequently twenty numbers which should be in proportions analogous to the preceding. It is evident too that these calculations require that the five plates emitting the 100 rays which fall successively on the whole series of diathermanous bodies should be those that are indicated by the same names in the first column. I have accordingly taken care that this condition should be satisfied.

The bodies submitted to the heat emerging from the screens present no longer the same order of transmission that they presented under the immediate action of the radiation of the lamp. The changes which take place have no apparent regularity whether we compare one series with another or consider only the different terms of the same series. Thus glass, Iceland spar, and rock crystal are more diathermanous to the heat emerging from the five screens than to that which comes directly from the source. Citric acid and tartrate of potash become more permeable to the rays issuing from the alum and sulphate of lime, and less permeable to those which proceed from black or green glass. With the opake mica and the tourmaline the case is directly the contrary. Some substances are equally permeable to the heat radiating from several screens. Others experience variations so great as to exhibit all the phases of the phænomenon, from an extremely abundant to an excessively feeble transmission*.

Through all these vicissitudes the action of the rock salt continues the same and uniformly transmits 92 rays out of 100. Hence follows the inverse proportion that if the series of plates be exposed to one hundred rays emerging from a plate of rock salt, the ratios of the quantities of heat transmitted would be the same as those obtained through the action of the immediate radiation; a proposition which I have besides verified by direct experiments.

After what we have so often repeated respecting the action of universal and partial diathermanous bodies, it would be superfluous again to point out the perfect similarity between these facts and the analogous phænomena presented by the transmission of light through diaphanous media, colourless and coloured. We shall therefore confine ourselves to a single observation on the nature of the rays which traverse certain screens.

The heat emerging from alum is almost totally absorbed by the opake

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* This change in the faculty of ulterior transmission is not the only modification that radiant heat undergoes in passing through the diathermanous bodies. It becomes also more or less susceptible of being absorbed in different quantities by the black and the white surfaces. This fact can be thus proved by experiment:

We take two thermometers of equal sensibility, and after having coloured one of the balls black and the other white we expose them simultaneously to the radiant heat, sometimes direct, sometimes transmitted through a plate of glass. The two thermometers are then observed to rise unequally, but the inequality is greater when the transmitted heat is employed. Mr. Powell, to whom we are indebted for this ingenious experiment, has performed it on calorific radiations from a bright red hot iron and from an Argand lamp. The means of several series of observations furnished, as the ratio of absorption of the thermometer with the black to that of the thermometer with the white ball, 100 : 78 when the red hot iron was employed, and 100 : 72 when the lamp was used. These ratios became 100 : 50 and 100 : 57 when he operated on the rays transmitted through glass. (*Report of the First and Second Meetings of the British Association for the Advancement of Science*, pp. 274, 275.)

I have obtained numerical data perfectly analogous, by means of the thermomultiplier. The pile of the apparatus was well washed, afterwards whitened on one side and blackened on the other. The two colours were made from lamp black and Spanish white mingled with gumwater. Turning the pile on its stand I caused the direct or transmitted rays of a Locatelli lamp to fall successively on the two coloured surfaces, and observed the corresponding indications of the galvanometer. This experiment is promptly and easily executed. It has moreover the advantage of requiring no more than one thermoscopic body, a screens, but is abundantly transmitted by *all* the diaphanous colourless plates. It suffers no appreciable loss when the thickness of the plates is varied within certain limits. Its properties of transmission therefore bear a close resemblance to those of light and solar heat.

Let us now direct our attention to the rays which issue from the last two screens. The opake bodies transmit nearly the half of them; the

circumstance which makes it easier to compare the results than it is found to be when we are obliged to have recourse to *two* thermoscopes, which seldom or never possess the same degree of sensibility.

I shall now give the ratios derived from this process applied to direct heat, and to heat transmitted through several screens. The calorific effect produced each time on the black surface is represented by 100.

Radiant heat from a Locatelli lamp, (direct, or transmitted through several screens).	Absorbent power of the faces	
	black.	white.
Rays direct from the lamp	100	80·5
Rays transmitted through rock salt	—	80·5
——— alum	—	42·9
——— glass, colourless	—	54·2
——— bright red	—	60·6
——— deep red	—	77·8
——— bright yellow	—	55·5
——— deep yellow	—	63·6
——— bright green	—	67·4
——— deep green	—	70·5
——— bright blue	—	61·0
——— deep blue	—	66·9
——— bright violet	—	67·6
——— deep violet	—	76·7
——— opake black	—	84·6

Thus the interposition of the rock salt has no influence on the ratio of the quantities of heat absorbed by the two surfaces; but the alum affects it so strongly that the heat which has traversed a plate of this substance is much less capable than the direct heat is of being absorbed by the white surface. Colourless glass acts in a similar manner though with somewhat less energy. As to coloured glasses, their action is more feeble in proportion as their tint is less vivid. In short the greatest decrease in the absorption of the white surface is produced by the interposition of a yellow glass, and the least by the interposition of the red and the violet, and, as to each pair of plates of the same tint, the less effect is *invariably* derived from that in which the tint is deeper. This decrease of action which takes place in the vitreous matter in proportion as its transparency is diminished by addition of colouring substances more and more sombre, continues even when the glass loses its transparency altogether; for the plate of opake black glass is that which produces the least difference of absorption between the black and the white surfaces. It is however an exceedingly curious fact that the rays of heat in their passage through the black glass become more absorbable by the white surface than the rays issuing immediately from the lamp, so that the interposition of the black glass has on the direct heat an effect contrary to that produced on it by the interposition of the white glass.

diaphanous substances intercept them in very different quantities, and the portions transmitted are considerably diminished by increasing the thickness of the flakes. Thus, the rays emerging from the black or the green glasses are in respect to their properties of transmission as it were antagonist to the preceding, and analogous to those of the direct heat of the flame though still more decidedly marked, for they are almost completely absorbed by bodies possessing the greatest transparency.

I have availed myself of these last facts for the purpose of proving by a very simple process that solar light contains some calorific rays analogous to those which compose the radiant heat of terrestrial sources. With this view I introduced a solar ray into a dark room through an aperture having a screen of green glass as a stopper. To the light transmitted I exposed one of the blackened balls of a very delicate differential thermometer. The liquid column descended several degrees. I now placed quite close to the mouth of the aperture a thin plate of colourless glass; the liquid came back a little, but the retrograde movement became more decided when I interposed instead of the thin glass a plate of greater thickness. I took away the white glass and put in its place a plate of rock salt: the column was forcibly driven back, but reascended very nearly to its original position when I substituted for the salt a plate of very limpid alum. It is clear therefore that amongst the calorific rays of the sun there are some which have a resemblance to terrestrial heat. On the other hand we have seen that the rays from terrestrial flame which traverse a flake of alum suffer, like solar heat, only a very slight diminution in passing through glass and other diaphanous substances. Whence we infer that amongst the calorific rays from flame some are found similar to the heat of the sun. *The differences observed between solar and terrestrial heat, as to their properties of transmission, are therefore to be attributed merely to the mixture, in different proportions, of several species of rays.*

But, to return to the heat emerging from the screens exposed to the radiation of the lamp. We have said that the red, orange, yellow, blue, indigo, and violet matters which enter into the composition of the coloured glasses, act upon radiant heat as the black substances introduced into a coloured medium act relatively to light; that is, they diminish the quantity of heat transmitted by the glass without altering its diathermancy [*diathermansie*]. This proposition being admitted, it will necessarily follow, when rays of different species, such as issue from the five screens contained in the table, fall on a series of coloured glasses, that the calorific transparencies of these plates will be increased or diminished in proportion to the variation produced in the diathermaneity [*diathermanéité*] of white glass. It has so happened in our experiments: for if we take the natural transmissions of the white, red, orange, yellow, blue, indigo, and violet, and compare these with their transmissions when submitted to the rays emerging from any one of our five screens, we shall always find the same ratios between the different terms of each series.

As to the black and the green glasses, their changes of transmission occur sometimes similar, sometimes contrary to those of the other plates. We should not however be surprised at these irregularities, as the green and black colours alter the natural diathermancy of the glass and give it an aptitude to transmit quantities of heat which will be more or less considerable in proportion as the rays issuing from the different screens possess themselves a diathermancy more or less analogous to that introduced into the vitreous substance by these two colouring materials*.

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* In a note to the preceding Memoir (page 8) I have said that, for the study of calorific radiations the thermomultiplier is preferable to every former thermoscopic apparatus. The great number of experiments that I have since performed by means of that instrument have produced in my mind a thorough conviction of the truth of that opinion. As there are still many experimental researches to be made not only in that class of phænomena, of the history of which we have scarcely given an outline, but in every branch of the study of radiant heat, it is to be wished, for the interests of science, that every investigator would furnish himself with a thermomultiplier. This apparatus, in the state of perfection necessary to ensure good observations, is unfortunately one of those which a person cannot construct for himself until he has made several attempts which are attended with a great loss of time, and which cannot succeed in many places for want of the requisite means. For these reasons I have thought it advisable to put some one in Paris in the way of supplying them to the public. There are excellent ones to be had at M. F. Gourjon's, *rue des Nonandières*, N° 2. The description of the ingenious means employed by this able mechanic to give to the instrument every improvement which I was desirous of having introduced into it would occupy too much time. I shall therefore confine myself to the mention of the principal defects found in the first instruments of this kind presented to the Academy of Sciences by M. Nobili and myself (at the sitting of the 5th of September 1831), but now laid aside for the improved thermomultipliers constructed by M. Gourjon.

In the first place the volume of the thermoelectric pile was too bulky, (being from 36 to 40 centimetres square in section,) a circumstance which rendered it impossible to operate on small pencils of calorific rays: in the next place the galvanometer did not mark fractions lower than half a degree, and the magnetic needles, instead of standing at the zero of the scale, settled sometimes to the right and sometimes to the left at a particular distance for each galvanometer, amounting in some instances to 10 degrees. In fine, the mountings being almost all of wood the pieces became warped by the hygrometrical variations in the atmosphere, and the instrument was rendered unserviceable.

The thermomultipliers of M. Gourjon have thermoelectric piles the acting surfaces of which are not larger than the section of a common thermometer (3 centimetres square). As to the galvanometers they are mounted entirely in copper with the exception of the small pieces necessary for the purpose of isolation: the minuteness of their indications extends to a fourth and even a sixth part of a degree, and the needles, when at rest, stand exactly at the zero of the scale. It is almost needless to add that with these improvements the instrument has lost nothing in sensibility.

Conclusion.

I had intended to introduce here some general reflections on the different hypotheses which have been proposed to explain the phænomena of heat, and on the question of the identity of radiant heat and light. But as these two agents are nowhere more intimately united than in the rays of the sun, such considerations should be preceded by a tolerably complete statement of the numerical results obtained by the application of our several processes to solar heat. The experiments however which I have hitherto been able to make with this view are too deficient in number and variety to justify my attempting any statement of the kind. I will therefore not enter, for the present, into any dissertation on the nature of heat, but will conclude with a recapitulation of the principal consequences to which I have been led by my inquiries into the properties of the radiant heat emitted by terrestrial sources, in order that being thus comprehended at a single glance they may be more easily compared with the analogous properties of light.

Radiant heat passes instantaneously, and in greater or less quantities, through a certain class of bodies, solid as well as liquid. This class does not consist exactly of diaphanous substances, since opake plates or plates possessing but a feeble transparency are more diathermanous or permeable to radiant heat than other plates possessing perfect transparency.

There are different species of calorific rays. They are all emitted simultaneously and in different proportions by flame, but in the heat from other sources some of them are always wanting.

Rock salt reduced to a plate and successively exposed to radiations of the same force from different sources always transmits immediately the same quantity of heat. A plate of any other diathermanous substance will, under the same circumstances, transmit quantities less considerable in proportion as the temperature of the source is less elevated: but the differences between one transmission and another decrease as the plate on which we operate is more attenuated. Whence it follows that the calorific rays from different sources are intercepted in a greater or less quantity, not at the surface and in virtue of an absorbent power varying with the temperature of the source, but in the very interior of the plate and in virtue of an absorbent force similar to that which extinguishes certain species of light in a coloured medium.

The same conclusion is attained by considering the losses which the calorific radiation from a source at a high temperature undergoes in passing through the successive elements which constitute a thick plate of any other diathermanous substance than rock salt. For if we imagine the plate divided into several equal layers, and determine by experiment what ratio the quantity lost bears to the quantity incident upon each of the layers, we find that the loss thus calculated decreases rapidly as the distance from the surface of entrance increases; but the diminution becomes less and less perceptible, so that it must become invariable when the rays have penetrated to a certain depth. This is precisely what happens to a pencil of ordinary light when it enters a coloured medium; for, those rays that are of a colour different from that of the medium being extinguished in the first layers, the losses of intensity sustained by the luminous pencil are at first very great, but they afterwards become gradually less and are at last very small, but constant when the only rays remaining are those of the same colour as the medium.

In fine the successive transmissions through heterogeneous screens furnish a third proof of the analogy which the action of diathermanous bodies on radiant heat bears to that of coloured media on light. The luminous rays issuing from a coloured plate either pass in abundance through a second coloured plate or undergo in it a powerful absorption according to the greater or less analogy of the colour of the second to that of the first plate. Now we observe facts perfectly similar to this in the successive transmission of radiant heat through screens of different kinds. And in this case too the rock salt acts in respect to the other bodies as it does in the case of rays emanating from sources of different temperatures. A given plate, if it be of rock salt, being successively exposed to calorific radiations of the same force emerging from different screens, transmits a constant quantity of heat; if the plate be of any other diathermanous substance the quantity transmitted will be variable.

There is therefore but one colourless and diaphanous body that really acts in the same manner on luminous and calorific rays. All other diaphanous bodies besides this indiscriminately suffer all kinds of light to pass through them, but of the rays of heat they allow some to pass while they absorb others: thus we discover in this one substance a real calorific coloration, to which, as it is invisible, and therefore totally distinct from coloration properly so called, we have given the name of diathermancy.

The colours introduced into a diaphanous medium always diminish its diathermancy in a greater or a less degree, without communicating to it any tendency to arrest certain calorific rays rather than others: they affect the transmission of radiant heat as dusky bodies affect the transmission of light. There is, it is true, an exception to be made in respect to green and opake black, at least in certain kinds of coloured glass. But these two colouring matters appear, in this case, to do no more than modify the quality to which we give the name of diathermancy, and which, as we have already seen, is totally independent of coloration.

The quantity of radiant heat which passes through polarizing plates of tourmaline is not affected by any change made in the angle at which their axes of crystallization are made to cross one another. Rays of heat are therefore not polarized in this mode of transmission and are in this respect entirely different from rays of light*. But they resemble them in the property of refrangibility. This is completely proved by means of the rock salt, the only diathermanous body that is capable of transmitting the calorific rays emanating from every source.

As to lenses and common prisms they refract a certain portion only of the radiant heat; for the glass intercepts several sorts of calorific rays issuing from sources at a high temperature, and absorbs nearly the whole of the heat given out by bodies whose temperature is below incandescence. To this circumstance it is that we must attribute the doubt hitherto entertained as to the refrangibility of nonluminous heat.

NOTE.

[We annex to the foregoing papers of M. Melloni, various references to other Memoirs on the Transmission of Radiant Heat, and to former views of the results obtained by him.

In the "Report of the Third Meeting of the British Association," p. 381, is an "Account of some recent Experiments on Radiant Heat," communicated by Professor Forbes, and reciting M. Melloni's Experiments; and also, p. 382, an abstract of his subsequent discoveries communicated by himself to Professor Forbes, in order to be laid before the British Association.

The "Notices of Communications to the British Association at Dublin, August 1835," contains, p. 9, some remarks by Professor Powell on Melloni's repetition of his original experiment described in the Philosophical Transactions for 1825, and in the Philosophical Magazine, First Series, vol. lxv. p. 437, and a notice of Dr. Hudson's Experiments with the Thermomultiplier, rendering it questionable, in his judgement, whether the results obtained by Melloni on diathermanous bodies were not attributable to conduction. These notices will also be found in the London and Edinburgh Philosophical Magazine, vol. vii. pp. 296, 298.

Prof. Forbes's Memoir "*On the Refraction and Polarization of Heat*" is contained in the Transactions of the Royal Society of Edinburgh, vol. xiii. p. 131, *et seq.*; and also in Lond. and Edinb. Phil. Mag., vol. vi. p. 134, *et seq.*

The following papers and notices have appeared exclusively in the London and Edinb. Philosophical Magazine:

A Note relative to the Polarization of Heat, by Professor Forbes

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* [Professor Forbes, however, in his Memoir, Lond. and Edinb. Phil. Mag., vol. vi. p. 205, *et seq.*, referred to in the Note which we have annexed, has established the fact of the polarization of rays of heat by this means, as well as by those of refraction and reflexion.—Edit.]

arising from Professor Powell's remarks in the Notices of the British Association, referred to above, with a Postscript containing Professor Powell's explanation: vol. vii. p. 349.

M. Melloni on the Immediate Transmission of Calorific Rays through Diathermal Bodies, in reference to the objections of Dr. Hudson and Professor Powell, vol. vii. p. 475. Remarks on M. Melloni's paper by Professor Powell, vol. viii. p. 23. Remarks on both the foregoing papers, by Dr. Hudson, *ibid.*, p. 109, confirming Melloni's original inductions. Professor Powell's Note on the Transmission of Radiant Heat, in reference to his remarks on Melloni's results, *ibid.*, p. 187.

Professor Forbes's Note respecting the Undulatory Theory of Heat, and the Circular Polarization of Heat by Total Reflexion, *ibid.*, p. 246. Dr. Hope's Address on the Delivery of the Keith Prize Medal to Prof. Forbes, giving a sketch of the history of our knowledge of radiant heat, *ibid.*, p. 424.—EDIT.]

In Part II. of SCIENTIFIC MEMOIRS will appear translations of the subsequent papers of Melloni, and also of other memoirs relating to the same subject.

ARTICLE III.

Experiments on the Circular Polarization of Light.
By H. W. DOVE.

From J. C. Poggendorff's *Annalen der Physik und Chemie;* Berlin,
Second Series, vol. v. p. 579.

1. *Circular Polarization of Light by Compressed Glasses.*

WHEN two systems of waves, of equal intensity, propagated in the same direction, and polarized perpendicularly to each other, differ in their path by an odd number of quarter-undulations, the *particles* in the resulting system of waves will describe small circles of a similar velocity around their points of equilibrium; that is to say, the light will be circularly polarized. Every means of equally satisfying these two conditions, namely, that of the similar intensity of the system of waves polarized perpendicularly to each other, and that of the determinate difference of path, consisting of an uneven number of quarter-undulations, will therefore furnish a method of circularly polarizing light. Fresnel and Airy have effected this in different ways. The third mode, which I shall here explain, is in practice at least as convenient as those hitherto used, and gives moreover a fuller explanation of the phænomena of compressed and cooled glasses in polarized light.

The condition of the equal intensity of the systems polarized perpendicularly to one another is satisfied by Fresnel by polarizing the incident light in a plane which forms an angle of 45° or 135° with the plane of the total reflexion in a glass parallelopiped. The quantities of light polarized in, and also perpendicularly to the plane of reflexion, are then, according to Fresnel's formula of intensity, equal to each other. He obtains the difference of phases of a quarter-undulation by twice-repeated total reflexion, since after a single one under the given circumstances the periods of vibration of the reflected waves no longer coincide, but exhibit a difference of phases of an $\frac{1}{8}$-undulation.

The method which Airy has adopted depends upon another principle. When a thin plate of an uniaxal crystal cut parallel to the axis, and whose axis forms with the plane of polarization of the incident light an angle a, is observed through a rhombohedron of Iceland spar, the principal section of which is inclined toward the plane of primitive of polarization under the angle b, then, if I_o, I_e indicate the intensities the two figures polarized perpendicularly to one another, we have generally,

$$I_o = \cos^2 b - \sin 2\,a \sin 2\,(a - b) \cos^2 \pi \left(\frac{o - e}{\lambda}\right)$$

$$I_e = \sin^2 b + \sin 2\,a \sin 2\,(a - b) \sin^2 \pi \left(\frac{o - e}{\lambda}\right),$$

in which λ indicates the length of undulation for a definite colour, $o-e$ the difference of path of both rays, and I the intensity of the polarized light falling perpendicularly upon the crystal plate. Now if the axis of the plate is made to form an angle of 45° with the plane of primitive polarization, that is to say, if we suppose $a = 45°$, we shall have,

$$I_o = \cos^2 b - \cos 2\,b \cos^2 \pi \left(\frac{o - e}{\lambda}\right)$$

$$I_e = \sin^2 b + \cos 2\,b \sin^2 \pi \left(\frac{o - e}{\lambda}\right).$$

If then by any means we can make the difference between the paths of both rays equal to an uneven number of quarter-undulations, the second condition will also be satisfied as well as the first, viz. that of the equal intensity. Suppose, for instance,

$$o - e = \left[\left(\frac{2\,n - 1}{4}\right) \lambda\right],$$

then will

$$I_o = \cos^2 b - \tfrac{1}{2} \cos 2\,b = \tfrac{1}{2}$$
$$I_e = \sin^2 b + \tfrac{1}{2} \cos 2\,b = \tfrac{1}{2}.$$

The difference of path $o-e$ depends on two quantities; on the thickness of the plate, to which it is in direct proportion, and on the difference of velocity of the two rays which pass through the plate, that is to say, on the constant of double refraction.

Airy's method consists only in varying the thickness of the plate by splitting it, whilst the double refraction remains the same, until the difference between the paths of the rays is equal to an uneven number of quarter-undulations. As biaxal mica under a perpendicular incidence of the light is similar to an uniaxal crystal and best allows splitting into larger plates, its application will therefore be preferable. I, on the contrary, alter the double refraction of the substance, whilst the thickness remains the same, until the required difference of path is obtained.

To alter the refraction of rays in a crystallized lamina by pressure or change of temperature, so that it may exhibit the desired effect in a given thickness, would afford no convenient practical arrangement. It is, however, very easy by means of pressure or cooling to change the uncrystallized into a double-refracting body, which gives precisely the required effect. In the apparatus proposed by Fresnel, consisting of four prisms, by which the double refraction of the glass is directly indicated, one of the two images which arise is polarized parallel to the axis of compression and the other perpendicular to it; whence it follows that the axis of the double refraction coincides with the axis of compression. If a square or circular plate of glass therefore is compressed so that the axis of compression forms an angle of 45° or 135° with the plane of primitive polarization, the light passing through the centre of the glass at a certain degree of the pressure will be circularly polarized. Let us now suppose a division of a circle so placed upon the incident ray that the plane of polarization passes through the points 90° and 270°; then, if the axis of compression passes through 45° and 225°, a plate of Iceland spar cut perpendicularly to the axis exhibits in the light passing through the centre of the compressed glass, instead of the black cross, rings in the second and fourth quadrants (on the right side above and on the left side below) advanced forwards by a quarter-interval from the centre, and on the contrary in the same proportion approaching nearer to the centre when in the first and third quadrant (on the left above and on the right below). Exactly the reverse takes place when the axis of compression passes through the points of division 135° and 315°. Hence we see that the angles which in the parallelopiped of Fresnel are formed by the plane of the twice-repeated total internal reflexion with the plane of primitive polarization, must be equal to the angles under which the plane perpendicular to the axis of compression is inclined towards the plane of primitive polarization, when the same phænomena are to be produced by both those arrangements.

No further particular explanation is now required to show that during a complete revolution of the plate in its plane round the perpendicular incident ray as an axis of revolution, the light is polarized four times rectilinearly and four times circularly; rectilinearly when the compressing screw acts on the points 0°, 90°, 180°, 270°, that is to say, when the axis of compression is perpendicular to the plane of primitive polarization or lies within it; and on the contrary, it is polarized circularly when that point of action corresponds to the points of division 45°, 135°, 225°, 315°, whilst 45° and 225°, as also 135° and 315°, exhibit a similar effect.

By a combination of two compressed plates and two tourmaline plates, so that the mutually perpendicular axes of compression of the glass plates, which are between the crossed tourmaline plates, form with their axes an angle of 45°, a lamina of Iceland spar laid between the glass plates exhibits the rings without a cross with the black spot in the centre, and complementary ones on the contrary when we make the axes of the tourmalines or the axes of compression of the glass plates parallel to each other. If we make an axis of compression parallel to a tourmaline plate we obtain displacement of the rings in the four quadrants by a quarter-interval; but the phænomenon is in that case not reciprocal, as a revolution here takes place similar to that which occurs when we look from the opposite side at an electric current in which the circuit is complete, and which is made to proceed in a circular form; the first and third quadrant then become the second and fourth, and *vice versâ*. By placing the tourmaline axes and the axes of compression parallel severally to each other, we obtain the phænomena of rectilinearly polarized light.

If between the crossed mirrors we insert a round or square plate compressed to a certain degree, so that the axis of compression coincides with one of the planes of reflexion of the mirror, we see upon it a black cross with white vacant spaces at the corners. If by means of the plate of Iceland spar these four white vacant spaces be examined, we find that those which belong to the same diagonal are similar to each other, but in opposition to the two white vacant spaces of the other diagonal; and it will be found that the light proceeding from them is circularly polarized, in the one diagonal to the right and in the other to the left. Hence it directly follows, that when the plate is turned in its plane 90°, all the white vacant spaces have exactly exchanged their effect in the diagonals. The plates I made use of in these experiments were $11\frac{1}{2}$ lines in diameter, and $3\frac{1}{4}$ lines in thickness.

2. *Circular Polarization by Cooled Glasses.*

I carefully cooled a glass cube of 17 lines each side, so that when the diagonals of the surface of the cube turned towards the eye form with the plane of polarization an angle of 45°, it exhibited between the crossed mirrors in the centre a dark cross, and in the four corners only the white surrounding it. The light of the four white vacant spaces was exactly similar to the light of the four white vacant spaces of the compressed plate, when their axis of compression lay perpendicularly to, or within, the plane of polarization. By turning the cube excentrically round the ray perpendicularly escaping through one of the white vacant spaces, as round an axis of revolution, similar variations are produced, whilst at 90° revolution the diagonals interchange their effect. Instead of turning the cube round, it may, in order to obtain the same variations, be so moved that two of the parallel sides of the surface turned towards the eye are carried forwards perpendicularly to their direction, whilst the other two advance in their own path. We pass from the white vacant space of the one diagonal into that of the other. The combinations of the cooled glasses, for the purpose of analysing circularly a circularly polarized light, explain themselves. In order to obtain the system of rings without the cross with the black spot in the centre, they must be combined as in Plate II. fig. 5.

So far as I am aware, we possess as yet no direct experiments upon the double refraction of the cooled glass; and as in the theory of the so-called moveable polarization the double refraction was not considered as a necessary consequence of the appearance of its colour in the rectilinearly polarized light, it is desirable to confirm by new experiments the proofs that these colours originate in the difference of path of the rays passing through the glass. The following therefore, for the explanation of the colours upon the principle of interference, seems to me not unimportant.

When a ray polarized rectilinearly in the azimuth of 45°, after two total reflexions in the interior of a Fresnel's parallelopiped, exhibits a difference of phase of a quarter-undulation, between the quantities of light polarized perpendicularly to each other, of uniform intensity, this difference will in this case, after four reflexions, become a half-undulation; the ray consequently will be again polarized rectilinearly, but perpendicularly to the plane of primitive polarization. After six reflexions it is again circular, but left-handed, if after the two reflexions it was right-handed, since the azimuth of the rectilinearly polarized incident light is now $-45°$ instead of $+45°$. Finally, after eight reflexions the plane of the restored polarization coincides with that of the primitive one. The explanation of the observed phænomena of circular polarization in the above-mentioned experiments, depended upon making the difference of path of the two rays exactly equal to the quarter-undulation, by means of a determinate change of heat in the interior of the body made use of, its thickness remaining unaltered. If this explanation is correct, precisely the same phænomena would be obtained by gradual heating as by successive reflexions in the interior of the Fresnel's rhomboid, but with this difference, that instead of the direction of the polarization varying by successive steps we should expect a continual transition through all degrees of elliptic polarization. The experiments confirm this perfectly. They must of course be made in homogeneous light.

3. *Phænomena during the Heating and Cooling of the Glasses.*

The apparatus (Plate II. fig. 1.) more particularly described in the succeeding paper was adjusted before a monochromatic lamp giving yellow light, so that the plate of Iceland spar in the ring *l*, cut perpendicularly to the axis, exhibited distinctly the black rings with the dark cross, when the glass cube reduced by a new heating and cooling to perfect loss of action upon polarized light, was thus interposed between *k* and *o*, before the Nicol's polarizing prism. In order to heat it conveniently over a lamp, the three-sided prism or rod *bc*, carrying all the polarizing arrangements, was placed in such a manner in its case as to bring those arrangements from their vertical situation over the rod to a position in which they projected on one side of it; their position as represented in the figure must therefore be imagined as altered 120°. In the ring *m* the screw was withdrawn a turn, in order that the motion of the rings, either away from the central point or towards it, might be the more easily observed.

The lamp having been lighted, the black cross began directly to open in the centre; the circular arcs in the second and fourth quadrant receded from the central point, whilst the first and third approached it. After some time the dark arcs of the odd quadrants exactly corresponded with the bright vacant spaces of the even ones; the light was circularly polarized, and the difference of path was a quarter-undulation. Whilst this was going on, with the exception of the points proceeding from the centre which remained black, the dark cross had become brighter and brighter. When it had entirely disappeared, the arcs, growing shorter at their ends, had gradually advanced, so that the two black spots proceeding from the centre formed with the parts approaching each other from the two other quadrants, the inner ring, separated by four bright intervening vacant spaces. All the other rings were in the same state. The figure given by the Iceland spar had thus changed, precisely as if the polarizing prism had been revolved 90°; the light was therefore polarized linearly and perpendicularly to the plane of primitive polarization: the difference of path of both rays was a half-undulation. On a further heating, as the difference of path became three quarters of an undulation, the light was again circularly polarized, with the difference, however, that now the rings in the first and third quadrant were the nearest, those in the second and fourth the more distant; in which case the direction of the motion of the arcs in the single quadrants naturally remained the same. Finally, when the difference of path amounted to an entire undulation, the white cross became darkened into a perfect black; the arcs previously separated closed in whole circles; the light was polarized rectilinearly in the same direction as at the beginning of the experiment. The lamp was now removed and the opposite phænomena were observed in regular succession during the cooling of the apparatus*; consequently the action of the glass, becoming gradually heated from below upwards, upon the incident light, is as follows. The particles of æther, which at first vibrate rectilinearly, begin to open into ellipses, the excentricity of which diminishes continually, until they become circles. The axis which at first was the

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* Precisely the same succession of phænomena may naturally be produced by the gradual increase of pressure or its relaxation. With the plates, however, which I had employed I was able to carry it only as far as a difference of path of three quarters of an undulation in the proximity of the points of action of the screw. On applying a stronger pressure the plates broke. Now it is evident that when a cooled glass plate, which in white light exhibits a regular series of colours proceeding from black, is interposed, in homogeneous light the same phænomena will be observed in the plate of Iceland spar, if it be slowly moved along before the aperture of the polarizing prism. The thicker the plate the nearer to each other are the differently-acting vacant spaces.

larger now becomes the smaller one, and *vice versâ*. With increasing excentricity the elliptic vibrations, which are perpendicular to the initial ones, pass directly over them. During all this process, the direction of the vibrations did not change; supposing it to have been from left to right, it remained so. When however the second rectilineal vibration opens into an elliptic one, and the direction of the motion has become inverted, the vibration now takes place from left to right, supposing it to have been before from right to left. The vibrations then return through circular again into the initial vibrations.

The light proceeding from the cube was now circularly analysed, by means of the interposition of a lamina of mica *f* of a proper thickness between the plate of Iceland spar and the analysing prism. The axis of this lamina lay so that the segments of the arcs were removed from the central point to the first and third quadrants. When the cube was yet unheated, its action was thus in direct opposition to its action in the first degree of its heating. When, proceeding from this point, the rings without the cross and with the black spot in the centre were formed, this spot, on the heat being increased, divided itself into two, which removed themselves from the centre into the second and fourth quadrants, and after having passed through the figure in the circular light, closed into a circle with the arcs proceeding from the first and third quadrants, so as to produce the system of rings with a bright centre, which would have been obtained at the very beginning by turning the polarizing prism 90°. The arcs, approaching nearer to the central point from the first and third quadrants, formed then the opposite circular figure, and united themselves at last in the centre into a black spot, whilst all the arcs closed themselves into circles. In this process, the phænomena before described of the linear analyses will again be easily recognised as a conditional element, without the necessity of particularly describing the alteration in form of the rings before they disunite into separate arcs.

To make circular light incident, is simply to add to the difference of phases produced by the heated cube a constant quantity, viz. $\frac{2n-1}{4}$ or $\frac{2n+1}{4}$ undulations; that is to say, to alter the starting-point of the experiment. Having therefore inserted the lamina of mica *g* between the polarizing prism and the heated cube, I obtained by linear analysis the phænomena first described, and by circular analysis those last described, beginning at another starting-point.

4. *Phænomena in the different Colours of the Spectrum.*

The foregoing experiments were made in incident homogeneous light, the length of whose waves was λ. In another part of the spectrum, however, λ has another value. Let $\lambda_{\prime}$ represent this; and if

$$o - e = m\,\lambda, \qquad o - e = m_{\prime}\,\lambda_{\prime},$$

then will $$m - m_{\prime} = (o - e)\left(\frac{1}{\lambda} - \frac{1}{\lambda_{\prime}}\right).$$

As $\frac{1}{\lambda} - \frac{1}{\lambda_{\prime}}$ is a constant quantity for a definite substance, the difference $m-m_{\prime}$ will be proportional to the quantity $o-e$. Hence it follows, *That when for one definite colour the light is circularly polarized by an interposed crystallized lamina, it may for the other colours be linearly and oppositely circularly polarized, and that the difference between the single colours increases with the thickness of the lamina and with the intensity of the double refraction.*

If the incident light is circular for the centre of the spectrum, when the difference of path is $\frac{1}{4}$ for this centre, the light is not yet linear for the extreme limits of the spectrum. If it is here linear in the red, with a $\frac{1}{2}$ undulation difference of path, in the blue it is circular. With $\frac{3}{4}$ difference of path in the red, it will, if it is circular to the right, be linear in the blue, and circular to the left in the extreme violet. Linear light in the red, with difference of path 1, gives on the left in the green a circular light, in the indigo a linear light perpendicular thereto and approaching the circular on the right in the extreme violet; finally, on the left, circular in the red, with difference of path $\frac{5}{4}$, will give linear in the yellow, circular on the right where the blue passes into the indigo, and perpendicular to it linear at the commencement of the violet, and so forth. In order to prove this by experiment an equilateral prism of Guinand's flint glass was placed upright, so that after the removal of the condensing-lens *p* the red end of the spectrum fell exactly upon the aperture *e* of the Nicol's polarizing prism. The cube had by gradual heating exhibited the phænomena which corresponded to a difference of path of $\frac{1}{4}$, $\frac{1}{2}$, $\frac{3}{4}$ undulation, and the other coloured rays were brought into the axis of the polarizing-apparatus, and the alteration of the Iceland spar figure examined. This might easily be accomplished without revolving the prism, as the height of the instrument may be altered at pleasure by means of the sliding-tube, as may its inclination by means of the motion of the prismatic rod. Mica plates of various thicknesses were examined in the same manner as the heated cube. The changes may be seen most beautifully when, beginning with the violet, the instrument is slowly lowered in the sliding-tube through the single colours of the spectrum. The gradual transitions are, in respect to the difference of colours from one end of it to the other, exactly the same as those which are obtained by the heating and cooling of the cube.

In the same manner the phænomena, when the incident light is circularly analysed by a mica plate inserted before the Iceland spar, are throughout similar to those before described. Instead of the homogeneous rays of the spectrum, we can of course also employ in these experiments a monochromatic lamp or absorption by coloured glasses. It is only when the light has been circularly polarized in one colour through a plate of definite thickness that we can determine, whether the difference of path of the two rays be $\frac{2n-1}{4}$ or $\frac{2n+1}{4}$ undulation. If, however, the same plate is examined in the different parts of the spectrum, we obtain by the experiments just mentioned n itself. It is manifest that if we wish to obtain by refraction phænomena of circular polarization in white light, it is advisable so to determine the thickness of the plate or the temperature of the glass that the difference of path for the central rays will become $\frac{1}{4}$ undulation. For this purpose I use the flame of alcohol coloured yellow by common salt or nitrate of soda.

5. *Phænomena of Colours of combined Crystals in White Light.*

It now becomes easy to account for the complicated phænomena of colours obtained by the insertion of a crystallized plate parallel to the axis and of any given thickness behind a crystallized lamina cut perpendicularly to the axis. For as the light is circularly polarized for one colour on the right, for the other on the left, and rectilinearly for an intermediate one, the black tufts on their two sides assume different colours: the phænomena in the even quadrants differ essentially from those in the odd ones, but the rings of colours in both are essentially different from the succession of colours in Newton's rings. The phænomenon may be previously determined from the known values of the indices of refraction, the length of waves for the homogeneous rays of the spectrum, and the thickness of the plate; but it may also be experimentally exhibited by adjusting the condensing-lens p of the apparatus so that the spectrum in the aperture of the Nicol's polarizing prism e be concentrated to white; a confirmation, the frequent repetition of which, however, is not advisable, on account of the intensity of the light of the apparatus.

6. *Phænomena of Colours in Twin-Crystals.*

In passing from the artificial combinations of two crystals to natural twin-crystals we have to distinguish them into three classes: namely, the axes of the united individual crystals are either perpendicular or parallel to each other, or they are inclined at some angle with one another. The section is always to be made perpendicular to the axis of one of the individual crystals. Though the first case may immediately give the phænomena just mentioned, yet, as far as I am aware, it does not occur with transparent crystals, whilst the second case may occasion the phænomena of colours with biaxal crystals only. Thus, if (as for instance in arragonite,) a very thin crystal is so united with another that its crystallographic axis lies parallel to the axis of the crystal which is divided by it into two parts, these two parts (since the optical axes of this lamella render perceptible, however small, angles with the bounding planes,) will operate as double-refracting prisms upon the light passing through these axes, because their optical axes do not lie in the plane of the axes of the lamella. The particular construction of this natural polarizing apparatus described by Erman, which from the thinness of the lamella exhibits the systems of rings of an unusual size, and considerably removed toward their optical axes on account of the obliquity of the surface of emergence, is obtained optically by comparing these systems of rings seen without previous polarization, in size and position, with those which evolve light previously polarized rectilinearly and afterwards analysed also around the optical axes of the including individuals, of which the one serves for the polarizing, the other for the analysing arrangement. That this last is the case, is moreover apparent from the following observation, that when a tourmaline is revolved before the crystal viewed in ordinary light, one of the systems of rings disappears alternately without changing its form. As however the phænomenon remains the same when the crystal is revolved, the same holds good for the polarizing prism, with which also the alterations of intensity of the rings agree when the crystal is viewed with the naked eye in rectilinearly polarized light. A decisive proof, however, that the individual behind polarizes rectilinearly, lies, as it seems to me, in the following fact, that the rings seen with the naked eye do not take the form which corresponds with the light when this is circularly incident.

The third case, in which the axis of the lamina growing into the other is inclined at an angle toward the axis of the including crystal, is also of importance for uniaxal crystals. The modification of the system of rings round the axis of the including crystal thus produced must coincide with that in two exactly central plates when a crystallized lamina of definite thickness is inserted between them. As that lamina may here be replaced by another similarly acting crystal, this case may be treated in the same way without difficulty. Among seven plates of Iceland spar exhibiting a deviation from the usual system of rings, I found two which produced a very regular figure, namely, a black cross with curves alternately osculating, which appeared to me to be circles and lemniscates; the interior curve was completely entwined into a figure of 8. If the plate is turned in its own plane, the interior part of the system of rings consists of four triangular vacant spaces. I obtained precisely the same phænomena by inserting a lamina of mica of definite thickness between two plates exactly centred and producing the regular system of rings, and by turning that lamina in its own plane.

7. *Experiments on Circular Polarization by other Modifications.*

Fluor spar is the only crystallized substance of the regular system which I have examined with respect to the effect of an unequal distribution of temperature within the body. The fragment I used in this instance was quite colourless and transparent, $1\frac{1}{2}$ inch long, and was lent to me for these experiments by Professor Weiss. At a heat in which the difference of path had become $\frac{5}{4}$ undulation in the glass cube, it exhibited throughout no effect upon the rectilinearly polarized light, although, in order to increase the difference of heat, I was continually cooling its upper end with sulphuric æther, whilst the lower end stood upon the hot steel plate*. Sonorous plates vibrating transversely acted neither upon the linear nor the circular incident light. But it is well known that Biot obtained a flash of light between the cross mirrors by the longitudinal vibrations of long strips of glass. Although in the experiments made with reference to this, the cross of the Iceland spar figure appeared to me to open, yet those experiments stand in need of being repeated with a better acoustic apparatus.

8. *Difference between the Action of Glass when it is Heating and when it is Cooling.*

Two square plates 3 lines thick, the side of one 11½ lines, and that of the other 13¼ lines, produced on being heated at first a circular light on the right, and then a rectilinearly polarized one; on their cooling, however, after they had returned to the rectilinear through the circular one on the right, they produced circular light on the left. The reason of this phænomenon is as follows: The lower end of the glass plate heated upon the hot steel plate cools when the lamp is taken away quicker than the upper one, to which heat is also communicated by conduction. After some time therefore the centre of the plate becomes its warmest part. As the lower end, standing upon the rapidly cooled conductor of heat, becomes still cooler, the warmer spot moves upwards until finally the upper angle becomes the warmest. That this is truly the reason of the phænomenon may be seen by examining the cooling plate between the crossed mirrors. The four white vacant spaces of the diagonals do not disappear on the spot where they had been formed; the lower ones rather move upwards, so that the dark cross becomes changed into two parallels, which are intersected by a perpendicular line. Finally, the central white vacant spaces dislodge the upper ones, whilst those newly arrived from below occupy the lower spot. By heating the plate so that its lower part constantly preserves the strongest heat, the progress of the phænomena must of course be more simple.

The action of a determined point of a cooled or compressed glass as a circularly polarizing apparatus, in the homogeneous rays of the spectrum, gives immediately the elements of determination for the colour which the glass presents in rectilinearly polarized light.

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* Brewster says in reference to the colours which fluor spar acquires by rapid cooling, "Fluor spar was very slightly affected."

ARTICLE IV.

Description of an Apparatus for exhibiting the Phænomena of the Rectilinear, Elliptic, and Circular Polarization of Light, by H. W. DOVE.

From J. C. Poggendorff's *Annalen der Physik und Chemie*; Berlin, Second Series, vol. v. p. 596.

UPON a common tripod brass telescope-stand with a horizontal and vertical motion, which, from its containing a sliding-tube, may be raised from 16 to 25 inches by means of a tightening-screw a (Plate II. fig. 1.), is placed in a case h a three-sided moveable brass prism b c, two feet long, and divided into Paris inches and lines. This prism carries five sliders s_1, s_2, s_3, s_4, s_5, which, by means of tightening-screws, may be fixed at pleasure at any part of the scale. Two of them s_2, s_5, the front view of which is separately drawn of the actual size in fig. 2, carry stands terminating above in rings, which by means of a pivot at r (fig. 2.) may be placed horizontally and vertically, so that the apertures of the Nicol's prisms t t revolvable in these rings, with the centre of the convex lens k, screwed into the ring of the slider s_3, (the stand of the convex lens being provided with exactly such a pivot, and in a perpendicular position also to the centre of the condensing-lens p which is carried by the slider s_1, the focal distance of the condensing-lens being 12 inches and its aperture 3,) lie in a straight line parallel to the rod b c, this line being at the same time the optical axis of the instrument. The Nicol's prism of the stand s_2, which is the nearest to this condensing-lens, may be called the *polarizing*, and that which is more distant from the stand s_5, the *analysing* one.

If parallel light is incident upon the condensing-lens, the polarizing prism must be in its focus, in order to polarize all the incident light; if, on the contrary, the light of a lamp is employed, the polarizing prism must be in the point of convergence of the rays which fall divergingly upon the condensing-lens. During this process it is of course not the prism but the condensing-lens that is to be moved until the concentrated light of the lamp falls exactly upon the aperture of the prism.

In order to alter at will the planes of polarization of the two prisms, graduated brass plates are placed at the rings of the stands s_2, s_5, upon which plates is placed a moving index, which, when intended to be prolonged backwards over the fastening-point, coincides with the longer diagonal of the rhomboidal bases of the Nicol's prism. The graduation of the circle is arranged so that, in the vertical position of the stand, the straight line passing through the points 0° and 180° lies horizontally. Fig. 2. exhibits of the actual size a view of these plates, which are not drawn in fig. 1. It is preferable to graduate that side of both plates which is turned toward the eye. The dotted stand in fig. 2. is therefore to be imagined behind the plate, when it belongs to the polarizing prism, and on the contrary before the plate and the graduation on the back of the plate, when the plate belongs to the analysing prism *d*. It will seldom be requisite to alter the plane of polarization of the incident light; it is most convenient to place it once for all horizontally, that is to say, to place the index of the polarizing prism upon 0° or 180°. In clear weather, when the light reflected by the sky is already more or less polarized, the instrument is to be directed, where this is possible, toward a wall on which the sun shines. If, however, the light reflected by the sky is to be directly employed, and in the greatest possible intensity, this may be most completely performed as follows. The polarizing prism with its plate having been placed horizontally, the analysing one is turned, until the system of rings with the black tufts is obtained in a plate of Iceland spar cut perpendicularly to the axis; within the ring *l* of the stand s_4 the polarizing prism *e* is then placed vertically again, and turned round until the same phænomenon is perceived in the Iceland spar. The index of the polarizing prism *e* then indicates the direction of the plane of polarization of the incident light, and the rings appear with greater distinctness.

The light diverging from the polarizing prism is at first intercepted by a convex lens indicated by *v*, two inches in diameter, and distant $\frac{5}{4}$ inches from the aperture *e*, and which is screwed into the lower end of that part which passes through the plate and is the holder of the prism: it then falls upon the lens three inches distant upon the stand s_3, and having $1\frac{1}{2}$ inch focal distance. From this point it passes through the crystal of the stand s_4 in the ring *l*, and which is to be examined in the polarized light, and proceeds into the analysing prism *d*, into whose lower end is screwed a concave lens indicated by *u*, and of four or five inches focal distance. Any inclination to the axis of the instrument may be given at pleasure to the ring *l*, by means of a ball and socket which is represented in fig. 1., or by means of a motion on points (as in the illuminating lenses or mirrors of common microscopes). Since now the crystal in this ring may also revolve in its plane, its optical axes may be altered at will in reference to their position with respect to the plane of polarization of the incident light. If however, for the exhibition of the isochromatic curves, two crystal plates cut parallel to the axis, or two laminæ of mica of uniform thickness are to be combined, the process is as follows: two turns of the screw must be given to the ring, which is to be inserted, of which the one that is represented on the larger cylinder enters into *l*, but the other, which is on the narrower cylinder, passes through, so that on the side toward *k* a second crystal is screwed in, whose axis may in this manner be made to assume at pleasure any angle to the axis of the first crystal.

The ring *m*, nearly in the focal distance of *k*, is intended for the reception of cooled glasses, thin laminæ of gypsum, and amethysts. Fastened to a pin, its central point is exactly in the axis of the instrument, when the pin is exactly vertical. Similar rings of wood, provided with straight pins, may be placed in the case of the stand s_4. Biaxal crystals are fastened to the pins, so that when the ring is turned round the pin, the systems of rings of the two axes pass one after another through the field of view; if therefore the indexes of the two Nicol's prisms stand at 0° and 90°, the black tufts of the systems of rings lie in a horizontal line. The ring *m* may also serve for the reception of a micrometric arrangement for the systems of rings of the crystals observed in *l*.

In order to change the rectilinear into circular polarization, the arms *f* and *g*, which revolve round the pegs *n* and *o*, contain laminæ of biaxal mica* of such a thickness as to produce a difference of path of exactly a quarter-undulation between the two rays, when the axes of those arms *ff* and *gg* (Plate II. fig. 2.) form with the plane of primitive polarization *ee* angles of 45° and 135°. Instead of the laminæ of mica cooled or compressed glasses may be employed, and combined (fig. 5.) in the manner particularly described in the foregoing paper.

If the two thin plates are turned aside, the rectilinearly polarized light is rectilinearly analysed. In order to analyse circularly, the rectilinearly polarized light *f* is brought forwards. In order to analyse rectilinearly the circularly polarized light, *f* is to be turned aside, and *g* placed forward. The two plates must be brought forward, as in fig. 1., when the circular polarized light is to be circularly analysed. The axis of the thin mica plate is indicated upon its frame. If that axis, instead of corresponding with the points 45° and 135°, passes through other points of graduation, we obtain the phænomena of elliptic polarization. If a small pin be fixed in the direction of the axis *gg*, the position of the axis of the lamina of mica may easily be drawn upon the graduation of the stand s_2.

In order to perform the simple experiments of intensity, it is advantageous to uncover the field of view. This is accomplished by a hollow cylinder one inch in height screwed into the somewhat projecting end of the frame of the lens *k* up towards *m*. The aperture of the opake diaphragm in the bottom of this cylinder is $1\frac{1}{2}$ line. This well-defined bright circle furnishes a very good object for these experiments. If

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* Although the same phænomena may be obtained by the determinate inclination of a thin plate of uniaxal mica, yet the employment of the biaxal mica appears to me much more convenient.

the analysing prism is turned in its frame, we obtain the decrease according to the law of Malus; if one of the laminæ of mica is placed before, on turning the intensity of the light remains unchanged. If, instead of the analysing Nicol's prism an achromatic double-refracting prism in a similar frame is screwed in, the analogous phænomena are obtained for both images.

When the polarizing prism *e* is bent on one side, a double-refracting prism screwed into the ring *l* gives two mutually perpendicular polarized images of the aperture in the diaphragm, the changes of intensity of which are obtained by turning the analysing prism *u*. If the thin lamina of mica *f* is placed forwards, the images, when the principal section of the double-refracting prism lies perpendicularly or horizontally, become circular on the right and left, and an arrangement coinciding with the apparatus proposed by Fresnel is obtained, consisting of three rock-crystal prisms, of which two belong to a crystal turned to the right and the single one to that turned to the left. By turning the analysing prism, the intensity of the images remains unchanged. If the analysing prism be also a double-refracting one, on turning it, two images with unchanged intensity (the mica plate lying between) move round the two stationary images with the same property.

If a mica or gypsum plate of a determinate thickness be in the ring *m*, on its turning round the pin to which it is fastened we obtain the phænomena of the so-called coloured polarization between the two Nicol's prisms. The complementary colours appear of great intensity, and give white where they overlap each other, when the analysing Nicol's prism is exchanged for a double-refracting one. Should we wish to combine two double-refracting prisms as above, the mica plate *f* must be exchanged for a thicker one. When the aperture of the diaphragm is diminished the images separate from each other. If a plate of Iceland spar, cut perpendicularly to the axis, is screwed upon the universal setting of the Nicol's analysing prism, the corresponding modifications of the system of rings in the separated and circularly polarized vacant spaces are obtained, when the double-refracting prism is in *l*; if on the contrary there is in the ring *l* a second plate of Iceland spar likewise cut perpendicularly to the axis, it is easy by turning this ring to cause the centres of the second and first plates to coincide. In this manner we may imitate the phænomena (as described in the preceding paper) of certain twin-crystals of Iceland spar by interposing a mica plate of definite thickness in *f*. If *f* lies at the side, by turning the ring *l* the isochromatic curves originating from the combination of two plates of which the centres do not coincide are obtained*. In a similar

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* In order to obtain the four mutually involved spirals of a rock-crystal plate turned right and left, I combine a right-handed plate ground plano-concave with a left-handed crystal ground with parallel faces.

manner the plates of different crystallized bodies are combined, in order to examine the positive or negative character of their axes.

If, instead of white, either homogeneous or dichromatic light is to be made incident, small rings of wood one inch in diameter, with coloured glasses, must be fastened before the aperture of the polarizing prism *e*. When the concentrated light of a lamp giving white light falls upon dichromatic glasses, they exhibit with biaxal crystals different optical axes for the various colours, and with uniaxal crystals they yield beautiful changes of differently coloured rings. Blue glasses, which separately transmitted the extremes of the spectrum, exhibit (in arragonite, for instance,) the inner curve divided into two particoloured vacant spaces and corresponding changes within each ring; on the contrary, the two inner rings in the Iceland spar are exhibited of a deep red surrounded by violet rings gradually passing more and more into each other, during which, lighted by a flame of spirits of wine coloured by chloride of strontium, the three inner rings are violet, to which three red ones then succeed, and so forth. Through a ruby glass we now obtain only a very homogeneous red, then dark rings, in the red field of view. A flame of spirits of wine coloured yellow with common salt, or nitrate of soda, yields the most beautiful phænomenon. The dark rings and the junction-curves of the different systems of rings of twin-crystals of arragonite then appear in the linear and circular light with the utmost distinctness. For blue and violet it is best to employ the colours of the spectrum. The condensing-lens is then removed, in order that the light may fall directly upon the aperture of the polarizing prism.

The apparatus shown in Plate II. fig. 3. serves to analyse the light by reflexion, and is screwed into the pillar s_5 instead of the analysing prism. The screw at *u* holds a concave lens of an equal focal distance. The unbordered mirror inclined at the angle of polarization is $\frac{7}{4}$ inch long and $\frac{5}{4}$ inch wide. A line is drawn over the three parts of the hinge *q* on the left side of fig. 1. If the parts of this line form one straight line, the rod *bc* is inclined towards a horizontal mirror at the angle of polarization. If *k* and *v* are placed aside, the light polarized by reflexion may be analysed either linearly by the prism, by the mirror in *u*, or circularly by means of *f*. But in order to examine larger cooled glasses in circularly polarized incident light, I employ a larger lamina of mica than that in *g*, which may be called $g_\prime$, and which fixed to the screw of the condensing-lens *p* is screwed directly upon a wooden ring of 2 inches internal diameter. The axis of this mica lies like that of the thin plate in *g*, which is turned aside. The concave lens in *u* is taken out, and the stand supporting the cooled glasses is brought to the distance most suitable to the eye. By holding the glasses in the hand, the various phænomena of the linear and circular light may be observed without alteration of the apparatus. If the glass be held between the condensing-lens and the mirror, *f* and g_1 being placed forwards, there is seen a cooled cube upon the mirror darkened by the analysing prism, fig. 6.; and consequently when the cube is turned 45°, fig. 7, the same phænomena are observed as if both the mica plates had been removed. Between the two mica-plates, whose axes cross each other at right angles, appears fig. 8, and indeed unchanged when the tube is turned in its ring. Fig. 9. is the complementary figure to it, which is obtained by turning the analysing prism to 90°, without changing the position of the mica plates. If *f* is bent backwards, there appears the modification of the linear figure, which produces circularly polarized incident light linearly analysed.

Of this as well as of that modification produced by circular analysis of the linear light which follows when the cube is close to the condensing-lens, it is easy to form an idea, by imagining the linear figure divided into four equal quadrants by two perpendicular lines, and the even quadrants removed from the central point about $\frac{1}{4}$ interval, and the odd ones approaching to within the same distance; or *vice versâ*, these removed, whilst those are made to approach. To polarize lamp-light by reflexion, the better way is to fix upon the condensing-lens (itself capable of revolving,) a mirror inclined at the fixed angle of polarization. If, by means of the polarizing prism, the instrument has before been placed upon the lamp, after the prism is turned aside and the mirror is fixed, that instrument, without changing its inclination, is turned round its perpendicular stand, until the system of rings is seen anew in the Iceland spar within the ring. Instead of employing Nicol's prisms, the light may be polarized by absorption in tourmaline plates, or by successive refractions through a series of glass plates. These are screwed into the stands in similar frames.

In order to obtain the deviation of the plane of polarization by refraction, the refracting bodies are introduced into the stand s_3. The deviation by reflexion may be conveniently observed by turning the rod at an angle toward a definite point. As, however, this experiment is easily made in another manner, I thought it unnecessary to complicate the apparatus for it. In the same manner the apparatus may be changed into a polarizing microscope, with a still larger field of view, by the addition of some lenses and stands. But as this will be desirable in very few experiments, besides that the construction of such an apparatus by means of single rings fitting one another is easy, I omitted them in this instrument.

When a glass warming or cooling is to be examined in the polarized light, the prismatic rod is so inserted into the frame *h* that one of its faces which have hitherto formed the sides, is brought into a horizontal position below. All the stands are then at the side of the horizontal rod turned to 120°, which presents no obstacle to the heating by an interposed lamp. If instead of looking into the prism *u* we look into *e*, on a slight change of the distance of the lens we obtain precisely the same phænomena. Thus an inverted order may also be given to all the stands with respect to the condensing-lens.

The superior advantages of the apparatus just described appear to me to be as follows:

1. The intensity of its light, which is so great that the flame of spirit of wine, 12 feet distant and coloured yellow by common salt, exhibits the system of rings of Iceland spar with great distinctness in an undarkened room.

2. The easy change of the linear into circular and elliptic polarization.

3. Its rendering unnecessary a particular arrangement for illumination.

4. The extent of the field of view*.

5. The purity of the colours, which are produced by colourless crystals only.

6. The cheapness of the instrument, since it serves equally as a model of an open telescope and as a microscope (the condensing-lens is the object-glass of the telescope; the stands s_2, s_3, s_5 form the eye-glass, s_4 becomes the stand for the microscopic objects).

7. The easy execution of all single changes in the various experiments above described.

The mechanician Hirschmann, of this place [Berlin], whose Nicol's prisms are in the hands of many natural philosophers, has already executed this apparatus according to my instructions in several sets made to order. Its price, if it is to be used both as an open telescope and microscope, is 60 rix-dollars.

POSTSCRIPT.

Fig. 4. Plate II. represents a small apparatus consisting of a single piece of glass, which exhibits united the modifications of the light by reflexion. The mutually parallel surfaces *ad* and *bc* are perpendicular to the parallel surfaces *ac* and *bd*; but, on the contrary, *ab* is inclined at 45° towards *ad*, and *cd* towards *bd*. The light therefore, falling perpendicularly upon *ad*, will after being reflected by *ab* and *cd* proceed from *bd*. The prismatic arcs bounding the vacant space of total and partial reflexion, therefore, intersect each other, as in the annexed figure. In the vacant space *m*, the light, after two reflexions, is unpolarized; in the vacant spaces *o* and *n* it is polarized perpendicularly;

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* In order not to diminish this, the arm *f* must move to and fro, close by *u*. The cylindrical setting of the polarizing prism must not be higher than half an inch.

and in the vacant space *p*, on the contrary, the partially polarized incident light is changed in the direction of the second reflexion. The light of the vacant space *m* differs from that in a Fresnel's parallelopiped by having the planes of the two reflexions perpendicular to each other, instead of coinciding as they do when that is used.

The phænomena of cooled glasses in circular light have not yet been described particularly, and those of compressed glasses not at all; we shall therefore add a few words respecting them.

In circular analysis solid cooled cylinders have the same properties as Iceland spar plates. In linear analysis they exhibit the system of rings without the cross displaced in the quadrants. The same may be said of the rings of colours of hollow cylinders, which are concentric with the inner black ring, and abruptly separated. The cross in three-sided plates consists of four black points (with two plates placed upon each other it consists of four triangles), which, united by bright gray shades, form a Y. In six- and eight-sided plates the black central spot becomes a six- and eight-sided star, while the colours of the angles are arranging themselves into a very regular inclosure particularly when by turning the analysing prism the centre becomes white: figs. 8. and 9. represent the figures of cooled cubes. The isochromatic lines of rectangularly crossed parallelopipedal plates remain, with regard to their form, identical with those in the linear light, which appear when the plane of polarization bisects the right angle between the plates. All the figures remain unchanged when the glasses are turned in their plane at the time of circular polarization and analysis. The irregularities of the figures produced by unequal cooling appear in the circular light, particularly with thin plates, and often even with those which appear regular in the linear light; nevertheless, I have also observed precisely the reverse, and that indeed with a six-sided plate.

A cylinder* compressed by brass wire wound round it had the same properties as a cooled one. Square and circular plates diametrically compressed by a screw, exhibit between the rings originating at the compressing points of the screw a coloured junction without a cross. If the axis of compression lies in the plane of polarization of the rectilinearly polarized incident light, the figure is also here displaced in the quadrants, when the light becomes circularly analysed.

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* This application of Weber's method of compressing glass to the phænomena of polarization was shown to me by Prof. Mitscherlich. (Compare Poggendorff's *Annalen*, vol. xx. p. 1.)

ARTICLE V.

Memoir on Colours in general, and particularly on a new Chromatic Scale deduced from Metallochromy for Scientific and Practical Purposes. By M. LEOPOLD NOBILI *of Reggio.*

From the *Bibliothèque Universelle des Sciences, &c.* vol. xliv. xlv. Geneva. (1830, vol. II. p. 337, vol. III. p. 35, Aug. and Sept.)

I DISCOVERED in 1826 a new class of facts and gave them the name of *electro-chemical appearances.* The following is one of the principal experiments connected with those facts.

A plate of platina is laid horizontally at the bottom of a vessel made of glass or china. A platina point is vertically suspended over this in such a manner that the distance between the point and the plate may be about half a line. A solution of acetate of lead is next poured into the vessel so as not only to cover the plate, but to rise two or three lines higher than the point. The plate and the point are now brought into communication, the former with the positive and the latter with the negative pole of an electric pile. At the moment when the voltaic circuit is closed, a series of rings similar to those formed at the centre of the Newtonian lenses is to be seen on the surface of the plate precisely under the point. This fact, which could not fail to strike any one observing it for the first time, led me to the discovery of others, which I have communicated to the public in four successive Memoirs*. I foresaw from the very first the advantages that the arts were likely to derive from this new method of colouring metals; but it was not until toward the close of 1827 that I began to attend seriously to its application. My first attempts I forbear to mention, being more desirous to call attention to the productions which I obtained in the course of 1828, and in the November of that year presented to the French Institute. These productions consisted of several plates of coloured metal, and excited the particular attention of that illustrious body by the beauty and vividness of their tints, the precision of their outlines, and the softness of their blendings†.

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* *Biblioth. Univ.* vol. XXXIII. XXXIV. XXXV. XXXVI. (Old Series.) *Annales de Chimie et de Physique,* vol. XXXIV. and XXXV.

† [A specimen of the productions of this beautiful art was presented by the inventor to the Royal Society, in whose Library it may be seen.—EDIT.]

The invention being now so far advanced as to be entitled to a place among the arts, it was thought that it should have its distinctive appellation, and by the advice of the same illustrious body, that of *Metallochromy* was adopted. Since that period I have made such improvements in my method that the first results, though they appeared satisfactory at the time, make but a sorry figure when compared with those now obtained. One of the great difficulties consisted in the necessity of producing a uniformity of tint on plates of certain dimensions; for, my colours being obtained by the effect of very thin plates applied to the surface of metals, it is easy to conceive how hard it was to preserve such plates of a uniform thickness over the whole of an extensive surface. Great however as the difficulties were, I thought I owed it both to art and to science to do my utmost to surmount them. I thought it due to art, because this would be extended by means of the uniformity of the tints, and to science, because in the tints produced by plates of a particular thickness the experimental philosopher would find the means of investigating with peculiar advantage the nature and properties of colours.

At present I abstain from all detail relative to the method of obtaining the homogeneous tints. *The principle of the electro-chemical appearances* seems now so fertile in results that its full development requires a particular treatise. It will be a work of considerable labour, and I have already commenced it by collecting and classifying all the materials of this new department of physics in which, besides the other methods of coloration, I intend to explain in detail those connected with the production of uniform tints. In this place it is sufficient to state that these tints are produced by substituting plates for the platina point which forms the coloured rings.

The object of this Memoir is more limited. It is to arrange these homogeneous tints in their natural order, so that they may form a scale or gamut which I shall henceforth designate by the epithet *chromatic.*

Science never consults its interests so truly as when it aims at some useful object connected with the arts. Such, I would fain hope, will be the direction of these researches. Artists, it is true, being generally unacquainted with physical theories, will find it difficult to follow me in my inquiries. My labour, however will not be altogether useless to them, if, as I intended, I have succeeded in treating certain parts of the subject in a manner likely to bring them within the reach of every understanding.

The formation of the chromatic scales requires considerable time and a hand well practised in work of this description. As they might be generally useful I regret that the difficulty of their construction renders a prompt and wide-spread circulation of them impossible. I have tried and am still trying to have them copied in oil and water colours, but the attempts hitherto made give little or no hope that the best executed copies can give more than an imperfect idea of the original colours.

The effect produced by these tints when disposed in the order set forth in the scale baffles description; it bears a resemblance however to that produced on the ear by a scale of semitones executed by a perfect voice. I have shown my scale to several, and especially to those erudite and learned persons who have favoured me with a passing visit at Reggio. In all it excited but one feeling of delight. So gradual indeed is the transition from one tint to another and such the harmony with which they are blended, that if the eye be accidentally turned away, it reverts in a moment as if moved by an irresistible desire to gaze still longer on the display. This statement is no exaggeration. It is but the mere fact, in respect to which a language much more glowing would be perfectly consistent with truth: so overpowering is the charm which, if I may use the expression, pervades the scale of our coloured plates.

Chromatic Scale.

This scale consists of forty-four tints, each of which is applied to a plate of steel. A Table subjoined to this Memoir exhibits the forty-four plates arranged one under the other in a column, and opposite to each number is the name of its peculiar tint. These tints are disposed in the same order as the layers or thin plates by which they are produced. The colour of the thinnest plate is placed first, and the others follow in the order of the progressively increasing thickness of the plates*. In this arrangement I cannot be mistaken, because the layers or thin plates which produce the several colours are all applied by the same electro-chemical process. The pile, the solution, the distances remain exactly the same. There is nothing variable but the duration of the action, which in respect to the layer No. 1. is very short, a little longer in respect to the second, and increases progressively from the lowest to the highest number. Other criterions also contribute to verify the accuracy with which its place is assigned to each tint.

These colours are produced by very thin layers or plates analogous to those which produce the colours in soap-bubbles and the rings observed

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* The numerals placed within parentheses (in the Table) are designed to indicate the thickness of the plates which produce the different colours. These numbers are taken from Newton's table, the fractional parts only being omitted. The numbers are those which apply to thin layers of water. The unit of measure is the millionth part of an English inch. Our scale should then commence with a layer measuring four of these units in thickness and end with a layer measuring thirty, if we suppose our electro-chemical layers to possess the same refractive power as water. It is probably somewhat less. At all events it is useful to have these numbers immediately before our eyes, in order that we may know, if not the absolute, at least the relative thickness of the attenuated layers which effectively cover our plates of steel.

by Newton around the point of contact of two slightly convex glasses or lenses. The order of the latter colours should therefore correspond exactly with that of my scale. It does so in fact; but that the correspondence may be perceived, it will be necessary first to rectify some errors which have arisen respecting the rings of Newton, either in consequence of their small dimensions, or of their having been examined under the influence of some prejudice.

Our scale embraces the extent of the first four rings, and consists, as we have already stated, of forty-four tints.

The tints of No. 1 to No. 10 (inclusive) correspond to the 1st ring.
———— 11 —— 28 ———— 2nd —
———— 29 —— 38 ———— 3rd —
———— 39 —— 44 ———— 4th —

Fundamental Principle.

It is well known that the colours of the thin layers around the point of contact of Newton's glasses are formed in the following manner. At the point which allows all the rays of the transmitted light to pass there appears a dark speck, and this remains the same whatever may be the quality of the light. If the incident light is white, the central speck is succeeded by several irises or concentric rings. If the light is homogeneous or produced by one species of rays, the irises are changed into rings of the same colour as the incident rays, and separated from each other by dark intervals. These rings, whatever be their colour, have their commencement all at the verge of the central speck, but they occupy different spaces. The violet rings are the narrowest and nearest; the red are the widest and most distant; the rings of the intermediate colours are of intermediate dimensions and at intermediate distances. When the incident light is white, the series of homogeneous rings are formed simultaneously and overlap each other; all the colours are intermixed in different proportions, and none stands isolated. It is to these combinations that we are to attribute the tints of the thin layers which we are about to analyse on our scale.

First Ring.—From No. 1 *to No.* 10 (*inclusive*).

The Scale commences with the blond* colour: of this there are four gradations, the first of which is silvery, and 2, 3, 4 are gradually deeper. The blond is succeeded by the tawny†. Of this there are three species,

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* [The term *blond* employed in the original has been retained in the translation to avoid the difficulty of giving an exact equivalent. Those brownish tints which in reference to human hair we term *light* or *fair* are evidently intended. —Edit.]

† [In the original the name of this colour is *fauve*, from the Latin *fulvus*; and the author says that he employs it in order to avoid the circumlocution of 'lion-colour'.—Edit.]

5, 6, 7; the last of which is called a copper-red on account of the analogy it bears to the colour of that metal. No. 8 is an ochre colour, No. 9 a violaceous ochre, and No. 10 a violaceous fire-red.

According to Newton the first ring should be composed of

Blue, White, Yellow, Orange, Red.

I find neither blue, nor the tints placed after the white and designated as yellow and orange. It seems to me that the tints of Newton's ring may be defined easily enough. They differ essentially from yellow and orange, and are in reality nothing else than the *blond* and *tawny* colours of our scale mixed together. Of this we shall have a direct proof by compressing these seven tints into a space as narrow as that which they occupy in Newton's first ring: for as soon as this is done we see the orange-yellow* which succeeds the white in the ring make its appearance. The blond and tawny colours of the scale are very compound tints: they possess a certain fieriness on account of the red which enters into their composition, have a slight resemblance to the colours of gold and copper, and are very difficult to be imitated, because their composition is such as to remove them further than the others from the prismatic colours. In nature they are found particularly in

1. The hair of animals.
2. The feathers of birds.
3. The fibres of certain species of dry wood, such as the walnut-tree, the pear-tree, &c.
4. The beard of corn, such as wheat, barley, rye, &c.
5. The smoke at the top of a flame.
6. The decoctions of roasted grain, such as barley, coffee, &c.
7. The halo seen around the moon when overcast with fog or light clouds.

The colours which the clouds assume are in general

Black, or very pure ash-colour;
White, or very light ash-colour;
The colour of smoke or coffee;
Red, more or less fiery;
Blue, very deep, and sometimes approaching to violet.

These are exactly the tints that would constitute the first ring were we to include in it the first two colours of the second ring. The tints

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* The absence of the *blue* does not affect the theory of the colours of thin plates: indeed I take it as a necessary consequence of the theory. All the homogeneous rings commence at the same place; namely, at the verge of the central speck. In this position the thin plate reflects rays of every kind, and this circumstance it is that gives the white without any trace of *blue*. It is perhaps to the contrast between the *white* and the *black* that we are to ascribe the illusion at the place where the two contrary appearances are produced.

of *smoke* result from the more or less thorough blending of the blond and the tawny; those of *fire* from Nos. 8, 9, and 10; the deep blue is produced by the Nos. 10, 11, 12, which are the deepest tints of the scale.

The first blond is properly that of light hair in childhood, and it is a fact worthy of remark, that as children grow older it becomes progressively deeper and deeper, in the order of the Nos. 2, 3, 4 in the scale. The perfect resemblance of the first tints on the scale to those which we observe about the moon when she is surrounded by clouds is equally remarkable: it seems in fact that this luminous appearance may be thus definitively explained. Tints of this kind do not arise from refraction and diffraction; they are produced only by means of thin plates: the luminous halo in question is therefore a phænomenon produced by thin plates.

This observation, combined with the fact that the tints exhibited by the clouds in every variety of aspect are almost all comprised in the first ring, leads to another consequence relative to the constitution of vesicular vapours. The measurements and experiments of Newton have shown what are the dimensions of the layers of air, of water, and of glass, which produce the colours of the several rings. The red of No. 10 is the last tint of the first ring: the indigo (No. 12) belongs to the second, and the thickness of the layer of water which produces it by reflexion is about the ten-millionth part of an English inch. As we know then, on the one hand, that the vesicular vapours are formed of water, and on the other that they do not reflect or transmit any tint beyond No. 12, we may conclude that their external film is in no case thicker than the ten-millionth part of an inch.

This result appears to me so decidedly certain as to be entitled to a place in science.

Second Ring.—From No. 11 *to No.* 28 (*inclusive*).

This interval commences at the deep violet No. 11, and extends to the lake-red No. 28. It comprises the most beautiful of all the gradations; namely,

Blue, Azure, Yellow, Orange, Red.

Newton places a green tint between the azure and the yellow. My scale exhibits no trace of green, and, with whatever attention I have examined Newton's second ring, I have never been able to perceive, in the place where the green should be found, anything but white tinged with azure and answering to the Nos. 15, 16, 17 of my scale. It is true that in the solar spectrum we meet with green in passing from the blue to the yellow: but the colours of the prism are simple, those of the thin plates are compound, and the order of their succession resembles but very imperfectly that of the colours in the prismatic spectrum*.

My scale is developed in such a manner that no illusion can take place. The interval comprised in the second ring is entirely free from green; neither is it to be found in the first order. Hence it is inferred, that among the thin layers of the two first orders there is none capable of reflecting any portion of green. The result is curious, and we have remarked it in the hope that, under different circumstances, it may be turned to account.

In speaking of the tints of the first ring we have stated that they are further removed than the others from the nature of the prismatic colours. The tints which, on the contrary, approach it most nearly are those of the second ring: yet even these are too distinct from it to be confounded with the simple colours of the prism. We have the sky, their type in nature, constantly before our eyes; for who is there that knows not the dawn, "with rosy forehead and golden feet"? Beginning with No. 12 of the scale, let us run our eye over it as far as No. 28, and we shall find the tints of the sky disposed there in the order in which they present themselves in the magnificent spectacle of the dawning day. This succession, as we have already observed, is the most beautiful of all: Newton's second ring gives no idea of it, because its colours are not, and cannot be, sufficiently developed to produce the effect. Painters, if I mistake not, will do well to avail themselves of this part of the scale: they will find in it a faithful copy of the beautiful tints of the morning, and endeavour to transfer them to their compositions. Natural philosophers will not fail to remark, that among the various tints of the sky there is no trace of green. This would heretofore have been found a perplexing circumstance, but may now be satisfactorily explained, merely by reflecting that the tints of the sky belong to the second order, in which also there is no tinge of green. From the blue to the yellow the transition is through a very faint gradation of azure-yellow, and this is observed to be exactly the case in nature.

The tints produced by vapours and clouds belong to the second order. They contain in general more fire than the natural tints of the sky, but this quality is nothing in comparison with the purity, vividness and variety displayed in the tints of the second order. The appearance of the sun is never so magnificent as when the air is perfectly pure. Toward evening the lower regions of the atmosphere are always more or less

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* Professor Amici has been so kind as, at my request, to employ all the means at his command in a careful examination of Newton's rings. He has seen them exactly as I have; for he has found neither blue in the first nor green in the second ring. I value the testimony of my illustrious friend and colleague too highly not to avail myself of it in this case.

charged with vapours, the air no longer retains its morning transparency, and the setting of the sun is attended by a fiery tint which greatly mars the tranquil beauty of the spectacle. It is to those vapours that we are to attribute the inflamed appearance of the sky, because they possess the power of transmitting the tints of the first order, and these are of that fiery cast. Were it not for this circumstance the setting of the sun might justly vie with its rising.

Philosophers had long since settled their opinions as to the colours of the sky. These they explained by assigning to the air the property of reflecting the higher colours of the spectrum (violet, indigo, &c.), and that of transmitting the lower, (red, orange, &c.). The explanation was correct so far as it went, but to make it complete the exact quality of the tints should be determined by indicating the order to which they belong. It was necessary also to ascertain how light is affected by the presence of vapours. The considerations which we have just stated will perhaps supply both these deficiencies.

Third and Fourth Rings.—From No. 29 *to* 38, *and from* 39 *to* 44.

These two rings comprise (if I may use the expression) the richest tints. The tints of the first ring are distinguished by their fiery and metallic appearance; those of the second by their transparency and vividness; those of the third and fourth by their intensity, and by the presence of green, which is wanting in the first and second orders. The first appearance of green is in the third order at No. 32: it appears again in the fourth order at No. 41. These two greens differ but little from one another, and are both beautiful in a very high degree: they have a strong resemblance to the green of the emerald. The tints of the third ring do not differ much from those of the fourth: their most marked difference consists in the diminution of transparency observable in passing from the third to the fourth order.

The colours contained in these two series abound in the three kingdoms of nature; the vegetable kingdom however seems to present them in the greatest proportion.

The predominant colours in these two parts of the scale are the red, the green, and the yellow-green. There is here, properly speaking, no species of blue, but its absence is counterbalanced by the presence of the green, which is not to be found in the first two rings. It would seem as if the blue belonged peculiarly to the spacious vault of heaven, and the green to the surface of the earth. They are two dominant colours in nature, but their domains are separated, and the separation seems to me not to be accidental. It was necessary, I suppose, that the atmosphere should be composed of the most subtile particles, in order that they might remain suspended in space; the earth did not require to be of so delicate a texture. Hence we have two very distinct orders of particles or thin layers; the terrestrial, which are grosser and capable of reflecting the green tints; and the aërial, which are more subtile and capable of reflecting the azure tints.

Laws of Varying Colours.

Newton had observed that the colours of the rings changed their position as the angle of incidence, under which they were viewed, was changed. In certain rings a certain colour viewed at an incidence nearly perpendicular appears to form a given circle, but expands and forms a larger circle if viewed obliquely. These changes are much more perceptible in the outer than the inner rings. An obliquity of 40°, for instance, is sufficient to change the tone of a colour of the fourth order, though at the same angle of incidence a colour of the first or the second order undergoes little or no change. We must not omit to mention the effect of refraction, which is to render the transitions from one tint to another more slow in proportion to the greater density of the substance which forms the thin layer. This law may be included in the first, because the rings produced by dense layers are interior in reference to the corresponding rings produced by layers of inferior density, and the exterior rings are the more liable to change.

The colours of the scale are produced by thin plates, and are subject to the same laws as those of Newton's rings. It seems to me, however, that in respect to the law of the changing colours there is an anomaly that has not yet been mentioned. The higher tints comprised between the red (No. 44) and the yellow (No. 21) conform to the general law. If we view these tints at a certain inclination, we see No. 44 change to No. 43, No. 43 to No. 42, and so on in succession, each superior number exhibiting the appearance of the next inferior number. This law prevails until we come to No. 21: after this the phænomenon changes. The beautiful yellows 19 and 20 become azure-green; the brighter yellows 18 and 17 are changed to red; the azures 16 and 15 become yellowish; the blues 14 and 13 suffer no change, and with them the anomaly ends, for the general law prevails again from No. 12 to No. 1 inclusive.

This difference has not been indicated until now, and, as I mention it for the first time, I deem it necessary to state that it escapes the eye when we endeavour to observe it in Newton's rings, in consequence perhaps of their being so limited in extent*. The anomaly prevails in the central part of the second ring, where the thin plate reflects a great quantity of white light, and this part is the brightest of our scale. I remark this circumstance, in order that it may receive due attention from those who would thoroughly investigate this point. In such an investigation it will probably be necessary to take into account the

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* See additional Note at the end.

variations of the law of refraction, when the obliquities of inclination are great,—such, for example, as those to which we must have recourse in order to account for the changes of tone in the colours of the first two rings.

Exceptions to the Law of Varying Colours.

If bodies where composed of thin layers such as those which form the chromatic scale and Newton's rings, their colours would change with every change of incidence, conformably to the law which we have just indicated. In nature the number of those colours that change is but small in comparison with those that remain fixed. Hence it may be inferred, either that the colours of bodies depend in general on a principle different from that of the colours of thin plates, or that this principle is modified in its application, the bodies not being constituted exactly as such an explanation would require. A few observations will perhaps be sufficient to fix our ideas on this very interesting point in the theory of colours.

Varying Colours in Nature.

In each of the three kingdoms of nature we have specimens of these colours. The animal kingdom especially affords some that are highly interesting, both in respect to their number and their beauty. It will be sufficient to mention the wings of butterflies and insects, and above all the feathers of different birds. Who is there that does not know, for instance, the variety of pleasing hues displayed in the plumage of the peacock? In this case, as well as in others of a similar kind, the colour that we observe is not given out by one continuous surface, such as that of a single plate: it is produced by a multitude of threads or fibres, so nicely overlapping one another that they seem to form a perfect plane, although they are really but a vast number of distinct minute surfaces, the position and thickness of which it would be necessary to know in order to apply the general law to them with any prospect of success. The phænomenon possesses all the characteristics of that produced by thin plates; but instead of a single layer, the number in this case is infinite, and, though disposed in an order calculated to excite our admiration, still it complicates the action of the light so as to prevent us from tracing it through all its variations.

The varying hue most frequently exhibited by the plumage of birds, is a beautiful green of the same intensity as No. 32. This number in the scale retains almost all its intensity, even at an inclination of 40°: at an angle of 50° it presents the appearance of No. 31, which is a purple colour with a greenish tinge; beyond that the original colour completely vanishes, and in its stead we have the violet-lake of No. 30.

But the varying green of feathers begins to change much sooner: when the inclination is near the 40th degree, it already presents the violaceous tint of No. 12. The intermediate steps of the transition cannot be discerned,—a decisive proof that the surfaces of the fibres which produce the green when the incidence is perpendicular, are not those which produce it when the incidence is oblique. The transition from No. 32 to No. 12 is so abrupt as to warrant this inference.

At all events the properties of the varying hues presented in nature are sufficiently interesting to be made the subject of a specific inquiry. I am at present engaged in collecting these colours, and hope that naturalists and experimental philosophers will contribute whatever they can toward the execution of a design likely to be attended with advantages, not only to Optics, but to other branches of science.

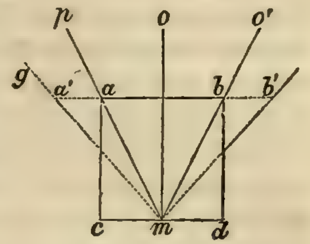
Unvarying Colours.

Nature presents a multitude of colours corresponding with the colours of the scale; but these are extremely changeable, while the natural colours are altogether unchangeable, except in the particular cases specified in the last paragraph but one. Let us fix our attention for a moment on the green, which is more prevalent than any other colour. Every herb, every leaf, is more or less of this colour. The green tints in the scale, of whatever order they may be, become red when the incidence is oblique: the same colours in herbs and leaves furnish no sign of such a transformation.

We know already, that the changes of tone to which the tints of the thin plates are subject diminish as the density of the plates increases. Were the substance of herbs and leaves much more dense than that of water, it might be said that it is owing to their excessive density that they suffer no perceptible change of tint from obliquity of incidence. But their density is far from being considerable; it is not so great as that of water. The phænomenon must therefore be explained in a different, and, as I think, in the following manner.

In applying the principle of the thin layers or plates to the explanation of the colours of bodies, it is supposed that those bodies are composed of layers analogous to the air and water introduced between Newton's glasses. Bodies are undoubtedly formed of very subtile particles; but have those particles or elementary groups the form of plates or laminæ? It does not appear so; it seems rather, on the principles of crystallography, which divides them into cubes, octahedrons, tetrahedrons, &c., that their forms are [polyhedral] solids. This circumstance makes a serious difference, and ought to be attentively examined.

Let us take, for example, the cubical, which is one of the simplest forms. Let us suppose the section of one of these cubes made in the plane of reflexion, and *ab* the side or face on which the incident rays fall. In passing from the perpendicular incidence *o m* to the oblique incidences *p m*, *g m*, it is evident that, allowance being made for the effect



of refraction, the last ray subjected to the influence of *interference* would be the ray pm which passes through the angle a. It falls on the inferior surface cd at the middle point m, and, being reflected in the direction mo', reaches the eye at o': every more oblique ray, such as gm, falls beyond the face ab, meets the vertical face ac, and contributes nothing to the coloration, which depends on the distance of the two faces ab and cd. In order to comprise the ray gm within this interval, ab should be prolonged to a' on the side of the incident, and to b' on the side of the reflected rays. But as it terminates at a and b, its field of coloration is confined within the limits mp, mo'. Now the angle omp, the sine of which is $\frac{1}{\sqrt{5}}$ (because $abcd$ is a square) does not amount to 27°, and this is an opening too small to admit the manifestation of any change whatever in the tints.

If the refraction (which precedes the reflexion) tends, as is evidently the case, to enlarge the field of coloration, it has a still greater tendency to diminish the effect of the change of the tints. It may therefore be considered certain, that the integrant particles of which bodies are composed cannot, in general, favour the play of the varying colours, unless, in defiance of all other observations taken collectively, we assign them a very considerable magnitude.

After the foregoing reflexions there remains, so far as I can see, but one point to be cleared. It being once admitted that the field of coloration of the integrant molecules is confined within narrow limits, how then, it will be asked, do bodies appear coloured in every direction? In general the molecules hold, in the bodies which they form, all sorts of positions, and are divided, relatively to the eye, into two classes; those of the one presenting their faces, and those of the others their angles toward the observer. The first are those which colour bodies; the second are those which in one position of the mass contribute, but in another do not contribute, to its coloration. In short, the eye is always in the field of coloration of a vast number of particles. When the field of one particle disappears, it is replaced by the field of another; so that the entire system always continues of a certain colour. Symmetrical arrangements present an exception, and we have already treated of these in the preceding paragraph.

Metallic Colours.

According to painters there are but three primitive colours, *red*, *yellow*, and *blue*. By combining these tints in various proportions with *black* and *white* they form the others. In richness and variety their productions are far surpassed by those of the thin laminæ. If we imagine one of the colours of the laminæ combined with another, we have the impression of a new tint. The combinations that may be obtained in this way are almost innumerable, and, it will be said, need well be so, in order to match the variety which nature exhibits. Such is our opinion too; but we shall not attempt to conceal the difficulty presented by the fact, that several of the natural colours, especially those of the metallic substances, have but a very slight resemblance to the colours of thin plates, among which it were vain to seek, for example, either the yellow of gold or the red of copper. The colours of the plates which approach them most nearly are found among the first seven or eight tints of the scale. The gold might be placed among the blond colours, and the copper among the tawny; but the difference is still so striking that it would be unwarrantable, before it is accounted for, to put entire and implicit confidence in the principle of the laminæ.

This principle requires, as a primary condition, that the integrant molecules of bodies should be transparent. It is true that almost all bodies reduced to a certain degree of tenuity are permeable to light; but it is equally true that the existence of a single body perfectly opaque and yet exhibiting a colour, would render it necessary to look for another principle of coloration besides Newton's, which is applicable only to diaphanous substances.

In my Memoirs on the electro-chemical appearances, I have shown that they are not exclusively produced by one of the poles of the pile. The appearances which constitute the chromatic scale are due to the electro-negative elements of the solution (oxygen and acid), which being transferred by the current to the positive pole, are there spread out into thin transparent films, from which all the colours of the scale arise. The electro-positive elements (such as hydrogen and the metallic bases) are, on the contrary, transferred to the negative pole, and there deposited in layers which never produce the colours of thin plates. Here it is impossible to mistake in any case, but more particularly in respect to the solutions of certain salts with a base of gold or of copper, which produce negative appearances invariably of the same colour as the metallic base. It cannot be said in this case that the substance has not been brought to the degree of tenuity necessary to render it transparent. The electro-chemical layers commence with the first degree of attenuation at the positive as well as the negative pole. If the layers of the positive pole produce the ordinary colours of the plates, while the opposite pole completely fails to present any other than that of the metallic base, it necessarily follows, either that these bases are perfectly opaque, or at least that their transparency is so imperfect as to render it impossible to apply the general laws to them, unless with very important restrictions. Indeed we have here a decisive proof that the colours which depend on the tenuity of plates are not to be traced on all classes of bodies; that they can be produced by those bodies only which are endowed with a certain degree of transparency; and that metallic substances are too opaque to be numbered among these. This is a positive fact, and ought therefore, without any regard to particular systems, to be entered in the register of science.

Gold and Copper.

It cannot be doubted, says Newton, that the colours of gold and copper belong to the second or the third order*. To us they seem, on the contrary, to belong to the first order, that being the only one which includes tints of a metallic appearance. If we only recollect that the first colours of the scale are far from being distinct in the first of Newton's rings, we shall feel less surprised that it should be necessary to correct the classification of that great philosopher. The resemblance in question is, however, as we have observed already, very far from being perfect. The tints that come nearest to the yellow of gold are the blond colours Nos. 2 and 3: but these are evidently less yellow, and at the same time more compounded than the colour of gold; for they contain a tinge of green, which does not exist in the more decided colour of gold. Transparent gold-leaf appears green when held before the light: this fact has been classed by several persons among the phænomena connected with thin laminæ, because these laminæ are known to reflect a given colour, in the same position in which they transmit its complementary colour. However I will say with a great philosopher, that "there is in Newton's rings no yellow that has green for its complement: the colour transmitted is invariably the blue; and this fact accords with the construction given by Newton for the composition of colours. But extract from this blue (which is necessarily compounded) a certain number of violet and blue rays, such as may be absorbed by the substance of gold, and there will remain green†."

It is a fact demonstrated by a great number of observations, that light in its passage through coloured substances is partially absorbed and extinguished. This fact not only renders Biot's explanation plausible, but warrants the supposition that light undergoes in reflexion a diminution analogous to that which takes place in its transmission. For if some of the rays destined to be transmitted are absorbed by the very substance of the gold, how can all the other rays, which are destined to be reflected in the interior of the same substance, escape undiminished? If the phænomenon be incomplete in respect to transmission, it will be equally so in respect to reflexion, and the tint formed will be

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* *Optics*, Book II. part 3. prop. 5.

† Biot, *Traité de Physique*, vol. iv. p. 127.

necessarily different from that produced by the ordinary thin plates, which are so transparent as to arrest no species of rays whatsoever.

The blond, as already observed, contains a tinge of green which is not found in the beautiful yellow of gold. If we leave out this green, by supposing it absorbed in the process of reflexion, the result will be a tint very closely resembling, if not exactly equal to, that of gold.

The red of copper requires a similar reduction. The colour nearest to it is the tawny of No. 7. But this contains a cast of violet, which is not in the copper, and the removal of it will make the resemblance, if not complete, certainly much less imperfect.

It is not my purpose in this place to enter further into the question, or to investigate the causes to which it is owing, that coloured bodies absorb certain rays more rapidly than others. The fact itself is proved, and it is unnecessary to go further for the attainment of our object, which was to discover the cause of the great difference between metallic colours and those of thin plates.

Colours developed on Metals by the Action of Fire.

The prismatic colours produced on steel and copper by the action of heat are universally known. Analogous colours are also exhibited by tin, bismuth, lead, &c., when they are in a state of fusion.

As to these colours, the most generally received opinion is, that they depend on a principle of oxidation. Berzelius calls the metallic layer which is thus coloured a *suboxide**.

I have always entertained some doubts as to the correctness of this explanation; because each degree of oxidation has a colour peculiar to itself, and in no way related to that variety of tints of which we speak. I was also struck by the well-known practice of giving steel a violet colour in order to secure it from rust. We know that this colour is produced by means of fire, in the process of giving steel a particular temper,—a *temper* which is called *violet,* because it is produced simultaneously with the colour. Were this tint, as it is presumed to be, the effect of oxidation, it would, in my opinion, instead of preventing, serve only to accelerate oxidation. A very high degree of polish, I allow, will keep off rust for a long time, but cannot stop it when once the action has commenced.

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* Some persons fancy that the phænomenon arises from the mere displacing of the parts, and thus exclude the intervention of any other substance. According to this notion it is but the metal dividing itself into laminæ of different degrees of thickness, and thus becoming capable of producing the different colours. Such an opinion, however, is opposed to a positive fact already demonstrated; I mean the fact of their opacity being in all cases too great to admit of their furnishing laminæ sufficiently transparent to produce the colours in question.

But this is not all: the superficial colours of which we speak are changeable, and belong evidently to the same class as those produced by thin plates. Now the pure metals, as we have already seen, are, from their opacity, incapable of this species of coloration. Can they acquire that capacity in their first degree of oxidation by becoming suddenly transparent in consequence of their union with a small quantity of oxygen? The hypothesis far exceeds the bounds of probability, and the phænomenon requires to be otherwise explained.

Let us return, for an instant, to the experiment of the coloured rings developed on a surface of platina by means of the electro-chemical apparatus described in the beginning of this Memoir. The platina surface belongs to the positive pole of the pile, and the electro-negative elements of the solution (which in the present case are the oxygen of water and the acid of acetate of lead) are deposited at this pole. I will not undertake to say by what species of affinity or force it is that these elements are attracted to each other and spread out into thin films on the platina. It is certain, however, that they attach themselves to the platina without oxidizing it in the slightest degree. We must not suppose that this happens because platina is a metal difficult to be oxidized. Iron and steel belong to the class of metals most easily oxidized, and yet it is well known that they will bear to be covered with electro-negative layers without becoming rusted. My electro-chemical experiments, multiplied and varied in a thousand ways, leave no room for reasonable doubt on this point: they show that oxygen and certain acids may adhere to the surfaces of metals without producing the slightest chemical change in them. This is a novel state for oxygen and the acids, and is distinguished from their ordinary combination by the three following peculiarities: 1st, The metal retains, beneath the deposited layer, its natural brilliancy; 2nd, this layer produces the phænomenon of the coloured rings in all its beauty; 3rd, instead of oxidizing the metal, these electro-negative elements contribute to secure it against oxidation in every part to which they are applied*.

A fact so unprecedented is interesting to chemistry and is entitled to particular attention, as tending to enrich the science by the introduction of new ideas†. Confining myself in this place to the colours produced on metals by the action of fire, I do not hesitate to say that I think

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* In order to give an idea of the efficacy of this preservative, it will be sufficient to quote the following experiment performed in Paris two years ago. I took two steel plates of the same quality and polish. I coloured one of them by the ordinary process, and exposed both in the open air to all the vicissitudes of a rainy autumn. At the end of a month the uncoloured plate was all rusted; the other had lost a little of its colour but was free from rust.

† If it were allowed me to offer an hypothesis relative to this novel state, I should say that the electro-negative elements disposed in thin layers on the surface of the metals are at too great a distance from the molecules of these

their origin now placed beyond the reach of doubt. It may be safely laid down as a general proposition that the oxygen of the atmosphere produces them, not, as is supposed, by oxidizing the surface of the metal, but by becoming fixed in the form of a thin plate or film similar to those of the electro-chemical appearances.

Copper, tin, and bismuth are pure metals, and I know not any layer by which they could be coloured, except that which has been just mentioned. Let a plate of copper be laid on a piece of red-hot iron: the plate becomes gradually heated, and all at once exhibits the most beautiful colours, but they disappear as suddenly. Before it becomes coloured the plate has a metallic lustre; it subsequently ceases to shine, and becomes evidently oxidized. It is therefore at the moment when the colours manifest themselves that the oxygen of the air precipitates itself on the copper. In the next moment the chemical combination is effected, which takes place whenever the action of the heat is sufficiently prolonged. If the plate of copper be removed from the red-hot iron as soon as the first indication of a change of colour is perceived at any point, the process of coloration will then go on more slowly, the copper will not be oxidized, and the oxygen, which would produce this effect under a more prolonged action of the heat, now covers the plate with a film, which adheres to it like a varnish, and by its transparency produces the usual colours.

The origin of the violet colour given to steel to prevent it from rusting is the same. The layer however which produces this tint in the steel does not perhaps consist solely of oxygen, as it does when the metals are pure. Steel is a carburet of iron, and the oxygen of the air in being precipitated on this compound, becoming combined with the carbon in some manner or other might form the layer in question. At all events the layer does not change its nature; it is always electro-negative, and secures the metal from oxidation as effectually as the layers applied by the electro-chemical process.

The electro-chemical appearances are formed with surprising rapidity, and the colours developed on metals exposed to the action of heat are produced with equal promptitude. It is therefore essential to the production of the phænomenon of thin plates that the electro-negative elements should be precipitated on the metal with a certain

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substances to enter into combination with them. This idea, which accords with the spirit of other theories, being admitted, we see at once how these layers preserve the transparency required to produce the coloured rings, and do not attack the metal so long as they are kept at such a distance as to be unable to combine with its particles. Berzelius was more sensible of the difficulty, perhaps, than any one else: but would not an open avowal have been better than the attempt to evade it by the adoption of the term *suboxide*, which is quite as vague and undefined as the *principle of oxidation*, for which it was offered as a substitute?

velocity. Does not the necessity of this condition show why these layers, in order to produce the desired effect, should be brought into contact with the metallic surface by the agency either of fire or electricity? The humid way is perhaps too tedious in all cases; it gradually oxidizes the surfaces of the metals, but never covers them with that thin and extended veil, the application of which requires a rapidity unattainable in this circumstance.

Nature presents in the specular iron a beautiful instance of the coloration which we have been considering. The ordinary colour of this ore is an iron gray; yet the faces of its crystals often display beautiful tints of every kind. They commence, in general, with the blue (No. 13) of the second, and go on as far as the reds (37 and 38) of the third order. These colours change as those of the scale do, and are so very like them that I thought they might be successfully imitated. I was not mistaken: a crystal of specular iron coloured by nature could not be distinguished from one coloured by the application of the electro-chemical process. There is no doubt as to the origin of these crystals; they are produced by fire, and it is that which has given them their colour by covering their surfaces with thin layers analogous to the electro-chemical. The humid way would have produced a very different effect: it would have destroyed their metallic brilliancy, and corroded their surfaces by the ordinary process of oxidation.

Singular Property of some Tints of the Scale.

A drop of alcohol is let fall on the violet (No. 11), and spread so as to cover part of the colour. The part thus made wet is no longer the same: we see instead of it a feeble tint resembling that of coffee mixed with milk; but the other part remains unchanged. The comparison can be instantaneously made, and the difference between the two tints is so striking, that we are at a loss to conceive how a transparent and very limpid film of alcohol can produce such a change in the violet colour on which it is placed. The alcohol gradually evaporates, and the colour recovers its former brilliancy.

Water, oil, and the different saline solutions produce the same effect; the thickness of the liquid film does not affect the phænomenon, and the colour undergoes the same change whether it be a thin film or a considerable mass. When transparent solids, such as glass, crystals, &c., are laid over the violet colour, it suffers no change. The liquids with which the plate is overspread adhere to its surface, so that this condition seems necessary to the production of the phænomenon.

Below the violet the indigo No. 12 and the blue No. 13, and (yet lower down) the red No. 10, the ochres Nos. 8 and 9 are subject to very marked variations. In the other colours of the scale when submitted to the experiment of the humid films no changes are visible,—none at least but such as are extremely slight in comparison with those which take place in the group of tints formed about the violet No. 11. There is no other fact connected with this. Such at least is my opinion, after having examined it under various aspects without being able to arrive at a satisfactory explanation. I am therefore unable to say more about it for the present.

Effect of Artificial Light during the Night.

It is an admitted fact, that we cannot judge of colours without the presence of daylight. But what changes do they undergo when viewed in the evening? The following are those that I have observed in the tints of my scale when examined at that time. The two conflicting opinions of those whom I consulted on this subject I forbear to mention.

1. The greens increase in beauty and intensity.
2. The yellows and the azure are tarnished and become deeper.
3. The blues and the indigo become greenish.
4. The violets approximate to a blue.
5. The violet-reds become more violet.
6. The first eight tints of the scale become more like each other, and approach more nearly to the metallic colours.
7. The other tints remain nearly unchanged.

Some of those changes are produced even by day if the colours of the scale are viewed through a green crystal, and others if they are viewed through yellow or azure crystals. Artificial light is without doubt differently constituted from that of the sun: it contains probably a scanty mixture of red rays with an abundance of the yellow, green, and azure. But what is the coloured diaphragm that should be interposed in the passage of the light of day, in order to reduce it to the same proportions as that of night? The problem is an interesting one, but it remains as yet without solution.

Harmony of Colours.

My scale appears to all persons to be eminently harmonious. I have already mentioned the delight which it afforded those who saw it. I have now to add that artists are astonished not to find the green in its usual place, between the yellow and the azure colours of the second order. But I take the two finest greens in the scale, Nos. 32 and 41, and call upon the most accomplished artists to assign them a more suitable place than that which they occupy. Influenced by habit they unhesitatingly place them among the yellows and the blues of the second interval, having no doubt that this is their proper place. They are however soon undeceived by the result; the green is found unpleasing here; the harmony is destroyed, and cannot be re-established until the colours are restored to their original position. But what is this harmony? It is an effect by a reference to the law of imaginary colours. It is necessary to give a brief development of the principle of this theory.

Let any colour whatsoever be exposed to some given degree of light and let the eyes be kept steadily fixed on it for some time: if the eyes be afterwards closed we have the impression of a different colour, which though it is never the same for one tint that it is for another, is always the same for the same tint. These colours, in some measure the offspring of the real colours, are called *imaginary* by philosophers, and by others they are named *fantastic* or *accidental.* The following is a table of them:—

Real Colour.	*Corresponding imaginary Colour.*
Red	Azure-green.
Golden	Indigo.
Green-yellow*	Violet.
Azure-green	Red.
Indigo	Golden.
Violet	Green-yellow.

After this table I cannot do better in reference to the present topic than give the following extract from Venturi.

"*The combination or succession of those colours which have such a mutual correspondence, that the perception of the one is followed by the imaginary sensation of the other, is agreeable and harmonious.*"

"Women of good taste know the colour of the trimming which has a good or a bad effect in combination with the fundamental colour of their dress. Leonardo da Vinci promised to give a table showing the colours which harmonize with one another and those which do not†: but he did not fulfill the promise, and no other painter that I know has pointed out precise rules for the harmony of colours. Several have observed merely that red combined with green has an agreeable effect; Newton apprises us that orange agrees with indigo; and Virgil was perhaps of the same opinion when he put this verse into the mouth of his Naiad,

Mollia luteola pingit vaccinia caltha.

"Mengs extols the union of violet and yellow; the same author says that the combination of red, yellow and azure is disagreeable‡; but that each of them should rather be joined with the colour intermediate between the two others; the red with the green, the yellow with the violet, and the azure with the orange."

"These different opinions have their origin and foundation in the transitions from the real to the imaginary state, which, as we have seen, naturally follow the involuntary movement of the retina; so that the

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* When the names of two colours are thus joined, the idea intended to be conveyed is that of the intermediate tint.

† On Painting, chap. 89.

‡ Mengs, *Leçons de Peinture.*

general law of the harmony of the eye will be this: *That the corresponding colours in the Table hereafter mentioned will be in harmony with one another.*"

"In fact, if the organ of vision, after having been fixed on an orange colour, directs itself spontaneously, and uninfluenced by any external impulse, towards the indigo, or *vice versâ*; or if in nature a violet-coloured hyacinth is placed beside a jonquil, and the optic axis is turned from the one flower upon the other, the centre of the retina passes over that succession of colours which is demanded by the nature of the organ and cannot be felt but with pleasure and satisfaction. These two colours harmonize, because the one leads to the other; and, for the contrary reason, if the eye has to pass from one colour to another, not corresponding with it in the table, it will necessarily have to make a disagreeable effort, because it will find itself in a position not in harmony with its former state. If the ocular *harpsichord* of Caslet were possible, the modulation of the colours would be executed on it according to the principle just laid down*."

I will not deny that the aptitude of the retina to cause an imaginary to arise from a real colour is of some account in the effect produced by colour. I am even inclined to believe that colours *attentively observed* are, by this tendency of the organ, associated with a *sentiment* and endowed with an *expression* which they could not otherwise possess. This however would be a species of *melody* and not of *harmony*.

Harmony is an instantaneous effect produced on the mind by several colours united altogether independent of the development of imaginary colours. Before this development can take place, the eye must be fixed for some time on a real colour; nor is this all, it is also necessary that the real colour should be seen in a bright light. Now, when I have one or two colours before my eyes, I can judge of their harmony without being obliged to look at them for a long time or requiring a very bright light. If I observe them for a single instant, my judgement is already pronounced, with the same promptitude with which the ear decides when it is affected by the harmony of sounds. Suppose for a moment that real sounds had their corresponding imaginary sounds, and the latter were determined when the ear had been for some time affected by a single quality of sounds suitably sustained. In the first place, these imaginary sounds could make no impression except in the particular case in which the notes are sustained for some time; but if we suppose that they accompanied the real sound necessarily and in every circumstance, they would not be in harmony with it; they would be perceived an instant after it, and would produce *melody*.

When a particular colour is ill-assorted with another, the eye is offended, as the ear is hurt by a discord. If we pass from one of these

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* Venturi, *Recherche Physique sur les Couleurs*, for which the prize of the Italian Society was awarded. Modena, 1802.

colours to the other through the intermediate tints, the first feeling will be changed into an agreeable sensation. Our scale, I repeat it, produces the same agreeable impression upon all, and it is to the inimitable beauty of its colours, and the manner in which they melt into each other, that this effect is due.

According to the law of imaginary colours red harmonizes well with green. In our scale the lakes, which are the finest reds in nature, are between the green tints and the orange, and combine agreeably with both. According to the same law the violet should agree only with the yellow; in the scale the violet tints are between the azures and the ochres, where they produce a very fine effect. The same law is opposed to the combination of yellow and azure, but the scale proves that these two tints combine agreeably, provided they have a certain tone and a certain degree of brightness. It is unnecessary, I believe, to multiply instances. The beauty of the tints and the graduation of the transitions constitute together one of the first secrets of art revealed by the effect of the chromatic scale. But it is not always allowed us to resort to the graduation of the transitions, and the artist requires another guide to show him what he is to do in all circumstances. It cannot be doubted that as there are combinations of sounds more perfect to the ear than others, such as the octave, the fifth and the third, there are likewise *concords of colours* more pleasing to the eye than others. But these concords should be determined. The field of inquiry is still new; it is possible however that the pursuit may be attended with most success by having recourse to the chromatic scale, which presents the tints in their greatest purity, and so arranged as to form the *gamut of colours.* This circumstance is an additional recommendation of the scale to the attention of philosophers as well as of artists.

Concluding Reflexions on the Qualities of Colours considered both philosophically and pictorially.

In physics it is usual to speak only of the brightness of colours. But besides being more or less bright, they are more or less intense or deep, beautiful, cheerful, &c. These epithets have been long in common use and are constantly on the lips of painters. In my opinion it is time that they should be admitted into science, and reduced to a more determinate signification than they have in ordinary language.

Brightness.

All who observe the seven colours of the spectrum will instantly perceive that they differ greatly in brightness. The clearest of them is the yellow. Fraunhofer, who has analysed the spectrum with so much care, assigns to that colour the highest degree of brightness.

The tints of our scale as well as the natural colours are far from being pure: they are all composed of several others. Hence arises a law of brightness different from that of the prismatic colours. The clearest on our scale are,

1st, The azures	Nos. 16 and 17.
2nd, The blonds	—— 1 and 2.
3rd, The yellows....................	—— 18 and 19.

The most obscure tints are Nos. 10, 11, and 12, in which the violet and blue predominate.

Depth.

Depth, or intensity, and brightness are very different qualities. No one indeed confounds the intensity of a fine red with the brightness of a fine yellow. In the scale of the latter quality the *white* occupies the first place. A bright tint may be considered as a mixture, in which there is a little colour with a great quantity of white light; and, *vice versâ,* a strong or deep colour, as a mixture of much colour with a little white light. Painters therefore when they want to give brightness to their colours add white, but when they want to increase their intensity they add a different colour.

The most intense colours of the scale are the lakes, especially No. 28 and No. 29. The feeblest are the azures No. 16 and No. 17, the blonds No. 1 and No. 2, and the yellow No. 18.

Some colours strengthen each other; some have no such effect. Thus, for example, the red of the spectrum combined with the violet forms a very beautiful lake, which is a much more vivid red than that of the prism. The same red combined with the green forms a mixture which possesses more intensity. The tints of the scale include all the prismatic colours, and their strength depends exactly on the proportion of the elements which enter into their composition. The lakes abound in red and violet, which are the two colours that give most depth to each other, as if one were the octave of the other. The sky-colours are too feeble, because with the exception of the blue, which they contain in a quantity rather excessive, the colours which enter into their composition will, when mixed, produce only white.

It is not strictly true that the intensity is in the inverse ratio of the brightness, because the more obscure tints Nos. 10, 11 and 12 are less intense than the lakes Nos. 28 and 29. Nevertheless there is a manifest relation between the two qualities; for it is certain that the feeblest colours are among those of the brightest class, and the most intense among those of the most obscure.

Thin plates according to their different degrees of tenuity reflect different colours; either these reflected colours are such as mutually to strengthen each other, so that there results from them a strong tint; or they do not strengthen each other, and the result of this is a white which predominates in the tint. Thus, the cause which generally renders it impossible to obtain brightness of tint unless by sacrificing intensity, is sufficiently demonstrated.

Beauty and Monotony.

Beauty consists in a certain variety which some tints possess in a higher degree than others. The yellow, for example, and the red of the spectrum have a tone peculiar to themselves: the golden contains the essence of the red and the yellow, and is more agreeable to the eye than either.

The most beautiful tints in the scale commence at the orange colours 22 and 23, and continue to the end.

The first element of pleasing is variety; in this point of view the purity and homogeneity of a colour are defects, of which philosophical painters must have been sensible when they recommended the use of compound in preference to simple colours *.

The purest tint of the scale is perhaps that of the yellow No. 19. At the first glance it is extremely pleasing, but soon becomes monotonous and the eye turns away for relief to the higher tints, each of which produces the sensation of several colours. A painting in which there is much yellow will therefore always fail to please on account of this *monotony*; for its effect is most disagreeable.

Nothing can be more beautiful than the varying colours: when we call them *varying* it is unnecessary to say why they please. Painters, we know, in order to give a finish to their productions, overlay them with certain tints. The colours of the painting appear through the tint, are mingled but not confounded with it, and thus are produced a variety and vividness unattainable by any other means.

Warmth and Coldness.

Those tints which contain the element of red are by painters called *warm*, and those in which the element of azure abounds are termed *cold.*

Red is the strongest and the most vivid colour: it is the colour of *fire* and of *blood*, and it warms and inflames all the tints into which it is introduced.

If the idea of warmth is associated with red, azure gives rise to a very different feeling: it is indeed preeminently the cold colour.

Yellow approaches more nearly to the nature of red than to that of azure, and is consequently rather warm than cold. Pure green cannot be said to be cold or warm: it inclines however to the former or to the latter accordingly as it is combined with blue or with yellow.

Cheerfulness and Gloominess.

Cheerfulness is not to be confounded with beauty, nor gloominess with monotony: they are more distinct sensations and seem to belong, the

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* *Leçons Pratiques de Peinture*, § v.

first to the lower colours of the spectrum, such as the red, orange, &c., and the second to the superior colours, such as the violet, indigo, &c.

The most gloomy tints on the scale are, according to the generally received opinion, those of Nos. 10, 11 and 12, in which the higher colours of the spectrum abound. These colours, it cannot be denied, are also the least bright, and this quality may well be the cause of the gloominess which is felt in viewing them.

It is possible however that there may be in this case an unknown general law, which it would be worth while to investigate with the aid of the analogies afforded by acoustic phænomena, of which the principles are better known.

On the Pathetic and the Cheerful in Music and Painting.

An exclamation or shout of joy consists of notes ascending from the grave to the acute; a cry proceeding from grief or pain consists, on the contrary, of notes descending from the acute to the grave. It is not more singular than true, although it has never before been remarked, that the same notes sung or executed on an instrument will produce in the ascending scale a very different effect from that which they produce in the descending scale. In the first case the feeling excited is decidedly cheerful; in the second it is as decidedly sad. This is a fact which in both a physical and a physiological point of view remains yet unexplained, but may serve nevertheless as a law for all analogous cases.

Violet is a colour which certainly awakes a feeling of sadness. Can it be owing to a similar law that it produces such a sensation? I inspect the table of imaginary colours, and find that the green-yellow corresponds to the violet. We know that according to the theory of vibrations the violet is produced by shorter and the red by longer vibrations. The transition then from the violet, which is the real colour, to the green-yellow, which is the imaginary, is a transition from the acute to the grave, and analogous to that which takes place in the notes that produce sadness. The only difference between the two cases is, that in the one the sensation is the direct and immediate effect of the notes conveyed to the ear from without, whilst in the other the eye receives from without nothing more than the impression of the violet colour, the rest of the effect depending on the internal action of the optical nerves which are endowed with the power of passing of themselves from the real to the imaginary colour. A difference of this kind however is not incompatible with the existence of the analogy: it only leads to the inference that the eye possesses the more exquisite sensibility, since in this organ a mere disposition or tendency is sufficient to produce an effect which in the ear is due to an external cause: for, the superior delicacy of the eye is evidently the cause of the existence of these imaginary colours, which have no counterpart in the other sense,—no succession of imaginary sounds resulting from those which had previously reached the tympanum.

In music there is a well-known and long-established distinction between *harmony* and *melody*: the former arises from a certain series of sounds produced all at the same time, the latter from the succession of certain sounds produced according to a certain rule. Can the science of colours lay claim to a similar distinction? I look at a fine painting, and am at once struck with the harmonious disposition of its beautiful colours. This is the first feeling excited, and it is excited in a moment. I afterwards examine and study the composition by looking attentively now at one point and then at another. The merit of the piece was at first confined to the beauty and harmony of the tints; now the same tints being observed with more attention awaken, or tend to awaken, the idea of the imaginary colours, and thus acquire an expression which was wanting to them when they were passed rapidly over. It has already been observed that the green-yellow arose from the violet, and that the latter colour had a tendency to produce a sensation of sadness on account of its involving a necessary transition from an acute to a grave tone. The lower colours of the spectrum (the red and the golden) have as their imaginary colours azure-green and indigo. In both these cases the passage is from the grave to the acute, and the two colours should, according to the law under consideration, excite a feeling of cheerfulness. The theoretical inference is confirmed by every one's experience.

This analogy between sounds and colours may, after all, be rather apparent than real. I thought myself bound nevertheless to mention it, with a view to its development, and on account of the new ideas which it might suggest.

Additional Note on the Law of Varying Colours.

In speaking of this law, I have remarked an analogy which presents itself in the central tints of the second ring. After having concluded my labours it occurred to me to examine this interval once more, and I noticed a fact which had escaped me in my first inquiries. Beginning with the perpendicular incidence, in order to pursue the examination through the other incidences, I observed the rings attentively. As my point of view I took the central part of the second ring, and there, at an angle between 70° and 80°, I perceived a new ring formed. This appearance was not accompanied by the disappearance of any of the other rings: it was really a *new ring* formed under this great inclination at the centre of the second, which was at first almost entirely white. I shall distinguish this ring from the others by the epithet *intruded* *.

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* It may not be useless, perhaps, to mention that my rings are *inverse* to those of Newton; his begin at the centre, mine at the circumference, where, from the nature of the electro-chemical process, the thinnest layers are deposited: the thickest layers are evidently those of the centre.

My rings can easily be so enlarged that the intruded ring may occupy a space two or three lines in breadth. The tints composing it will then be seen very distinctly, and will correspond exactly with those which are seen in detail on the plates 20, 19, 18, 17, 16 and 15; with this difference only, that, in place of these tints, a ring will be seen composed of *green*, *red* and *yellow*.

When the rings are smaller, as they usually are when obtained under the platina point, the intruded ring appears in the same place, and the observation, though made under circumstances less favourable, is equally decisive.

Newton's rings give no idea of this phænomenon: they vanish from the eye of the observer before the last degrees of obliquity are attained, and are consequently unavailable in an observation for which these great inclinations are an indispensable condition. The smallness of the dimensions of the rings cannot cause the observation to fail, whenever it can be made on my rings whether large or small.

I cover a portion of my rings with a layer of alcohol, oil, or water, &c., and when the observation is made at the before-mentioned inclination of from 70° to 80°, the intruded ring appears only where the humid layer is wanting. Thus the phænomenon connects itself still more with the law of refraction. In my opinion there are but few facts that can put a theory so severely to the test as this, and the theory which can completely explain it will have every claim to credit.

I shall always add to my chromatic scales a plate exhibiting on its surface the coloured rings as much enlarged as is requisite for the convenient study of the properties of the intruded ring. This I feel the more inclined to do, as these large rings are likely to be useful in other respects; they will serve, for instance, as a *key* to the chromatic scale, which is in reality no more than the development of the rings themselves; and this development is indispensable when we would judge of a colour. In the coloured rings, however large they may be, there is always found between every two tints a third into which they melt: its tone and the feeling which it produces are always confounded with those of the contiguous tints. For this inconvenience there is no remedy but to isolate the tints, so that the eye may be fixed on each of them without receiving at the same time any sensation from the others. The chromatic scale affords this advantage in its detached plates, not to mention the other advantages which in the course of this Memoir it has been proved to possess, and which it is therefore unnecessary to enumerate here.

Reggio, June 29, 1830.

Chromatic Scale.

44	Lacca-rosea.	Laque-rose.	Rose-lake.	(30
43	Verde-giallo-rossicc.	Vert-jaune rougeâtre.	Reddish yellow-green.	(28
42	Verde-giallo.	Vert-jaunâtre.	Yellowish-green.	(27
41	Verde.	Vert.	Green.	(26
40	Violaceo-verdognolo.	Violet-verdâtre.	Greenish-violet.	(25
39	Lacca-violacea.	Laque-violette.	Violet-lake.	(24
38	Lacca-rosea.	Laque-rose.	Rose-lake.	(22
37	Rancio-roseo.	Orange-rose.	Rose-orange.	
36	Rancio-verde.	Orange-verdâtre.	Greenish-orange.	(21
35	Verde-rancio.	Vert-orangé.	Orange-green.	
34	Verde-giallo.	Vert-jaune.	Yellow-green.	(20
33	Verde-giallognolo.	Vert-jaunâtre.	Yellowish-green.	
32	Verde.	Vert.	Green.	(19
31	Porpora-verdognola.	Pourpre-verdâtre.	Greenish-purple.	(18
30	Lacca-turchiniccia.	Laque-bleuâtre.	Blueish-lake.	(17
29	Lacca-purpurea.	Laque-pourprée.	Purpled-lake.	(16
28	Lacca-accesa.	Laque *éclatante.*	Brilliant-lake.	(15
27	Lacca.	Laque.	Lake.	
26	Lacca-rancia.	Laque-orangée.	Orange-lake.	(14
25	Rosso-rancio.	Rouge-orangé.	Orange-red.	
24	Rancio-rosso.	Orange-rouge.	Red-orange.	
23	Rancio-rossiccio.	Orange-rougeâtre.	Reddish-orange.	
22	Rancio.	Orange.	Orange.	(13
21	Giallo-rancio.	Jaune-orangé.	Orange-yellow.	
20	Giallo-acceso.	Jaune *éclatant.*	Brilliant-yellow.	
19	Giallo.	Jaune.	Yellow.	
18	Giallo-chiarissimo.	Jaune très-clair.	Very bright yellow.	(12
17	Celeste-giallognolo.	Azur-jaunâtre.	Yellowish-azure.	
16	Celeste.	Azur.	Azure.	
15	Bleu-chiàro.	Bleu-clair.	Clear-blue.	
14	Bleu.	Bleu.	Blue.	
13	Bleu-carico.	Bleu-foncé.	Deep-blue.	
12	Indaco.	Indigo.	Indigo.	(10
11	Violetto.	Violet.	Violet.	(8
10	Rosso-violaceo.	Rouge-violet.	Violet-red.	(7
9	Ocria-violacea.	Ocre-violette.	Violet-ochre.	
8	Ocria.	Ocre.	Ochre.	
7	Rosso di rame.	Rouge de cuivre.	Copper-red.	(6
6	Fulvo-acceso.	Fauve *éclatant.*	Brilliant-tawny.	
5	Fulvo.	Fauve.	Tawny.	
4	Biondo-acceso.	Blond *éclatant.*	Brilliant-blond.	(5
3	Biondo d'oro.	Blond-doré.	Golden-blond.	
2	Biondo.	Blond.	Blond.	
1	Biondo-argentino.	Blond-argentin.	Silver-blond.	(4

ARTICLE VI.

On the Mathematical Theory of Heat; by S. D. POISSON, *Member of the Institute, &c.**

From the *Annales de Chimie et de Physique*, vol. LIX. p. 71 *et seq.*

THE work which I have just published under the title of The Mathematical Theory of Heat (*Théorie Mathématique de la Chaleur*), forms the second part of a treatise on Mathematical Physics (*Physique Mathématique*), the first of which is the New Theory of Capillary Action (*Nouvelle Théorie de l'Action Capillaire*), which appeared four years ago. It contains twelve chapters, preceded by some pages in which I recapitulate in a few words the first applications of the calculus which have been made to the theory of heat, and the principal researches of geometers upon that subject, which have been made of late years, namely, since the first Memoir presented by Fourier to the Institute in 1807. I will here transcribe the contents of the Preface, on the important question of the heat of the earth.

"In applying to the earth the general solution of the problem of a sphere at first heated in any manner whatever, Laplace was led to participate in the opinion of Fourier, which attributes to the primitive heat of the earth the increase in temperature which is observed in descending from the surface, and the amount of which is not the same in all localities. This hypothesis of a temperature proceeding from the original heat of the globe (*la chaleur d'origine*), and which must rise to millions of degrees in its central layers, has been generally adopted; but the difficulties it presents appear to me to render it improbable. I have proposed a different explanation of the increasing temperature which has long since been observed at all depths to which man has penetrated.

"According to this new explanation the phænomenon depends on the inequality of temperature of those regions of space which the earth successively passes through in its translatory motion, and which are common to the sun and all the planets. It would be indeed opposed to all probability that the temperature of space should everywhere be the same; the variations to which it is subject from one point to another, separated by very great distances, may be very considerable, and ought to produce corresponding variations in the temperature of the earth, ex-

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* The work of which this article is an analysis, is described as a quarto volume of more than 500 pages, with a plate; published by Bachelier, Quai des Augustins, Paris.

tending to various depths according to their duration and amplitude. Suppose, for the sake of example, a block of stone transported from the equator to our latitudes; its cooling will have commenced at the surface, and then become propagated into the interior; and if the cooling has extended throughout the whole mass, because the time of its transportation has been very short, that body thus transported to our climate will present the phænomenon of an increase of temperature with the distance from the surface. The earth is in the case of this block of stone; —it is a body coming from a region the temperature of which was higher than that of the place in which it now is; or we may regard it as a thermometer moveable in space, but which has not had time, on account of its magnitude and according to its degree of conducting power, to take throughout its mass the temperatures of the different regions through which it has passed. At present the degree of temperature of the globe is increasing below the surface; the contrary has in former times been, and will hereafter be, the case: besides, at epochs separated by many series of ages this temperature must have been, and will in future be, much higher or lower than what it is at present; a circumstance, which renders it impossible that the earth should always be habitable by man, and has perhaps contributed to the successive revolutions the traces of which have been discovered in its exterior crust. It is necessary to observe that the alternations of temperature of space are positive causes which have an increasing influence upon the heat of the globe at least near its surface; while the original heat of the earth (*chaleur d'origine de la terre*), however slow it may be in dissipating, is but a transitory circumstance, the existence of which it would not be possible at the present epoch to demonstrate, and to which we should not be forced to have recourse as a hypothesis except in the case of the permanent and necessary causes being insufficient to explain the different phænomena."

The following are the titles of the different chapters of the work, together with a short abstract of the contents of each.

CHAPTER I. *Preliminary Notions.*—After having given the definition of temperature and many other definitions, it is explained how we have been led to the principle of a continual radiation and absorption of heat by the molecules of all bodies. The interchange of heat between material particles of an insensible magnitude, but yet comprising immense numbers of molecules, cannot disturb the equality of their temperatures when it actually exists. From this condition we conclude, that for each particle the ratio of the emitting to the absorbing power is independent of the substance and of density, and that it can only depend on temperature. In the case of the inequality of temperatures, we give the general expression of their variations during every instant, equal and contrary for two material particles, radiating one toward the other. We also give the law of absorption of radiant heat in the interior of homogeneous bodies.

CHAPTER II. *Laws of Radiant Heat.*—If a body be placed within a vacuous sphere on every side (*enceinte vide fermée de toutes parts*), the temperature of which is supposed to be invariable and everywhere the same, we demonstrate that the result of the interchange of heat between an element of its surface and an element of the surface of the inclosing sphere, is independent of the matter of which the sphere is formed, and proportional, *cæteris paribus*, to the cosines of the angle which the normal to the second element forms with the right line from one to the other element. Experiments, not as yet made, only can decide whether this law of the cosine is equally applicable to the elements of the surface of the body, of which the temperature is not invariable like that of the sphere; and until such experiments are made we may be allowed to doubt its existence while the body is heating or cooling. By considering the number of successive reflexions which take place at the surface of the sphere we demonstrate also that in general the passage (*flux*) of heat through every element in the surface of the body which it contains is independent of the form, of the dimensions, and of the material of the sphere; there is no exception, but when the heat, in the series of reflexions which it experiences, falls one or many times upon the surface of the body. It follows from this theorem that a thermometer placed in any point whatever of the space which the sphere terminates, will finally indicate the same temperature, which will be equal to that of the sphere; but in the case of the exception just mentioned, the time which it will employ in attaining that temperature will vary according to the place it occupies. The general expression of the passage of heat through every element of the surface of a body of which the temperature varies, is composed of one factor relative both to the state of that surface and to the material of the body, multiplied by the difference of two similar functions, one of which depends on the variable temperature of the body, the other on the fixed temperature of the sphere, which are the same for all bodies; a result which agrees with the law of cooling *in vacuo* discovered by MM. Dulong and Petit. We next suppose in this second chapter, that many bodies differing in temperature are contained in the sphere of which the temperature is constant, and arrive then at a general formula, which will serve to solve the problems of the *catoptrics* of heat, the principal applications of which we indicate. When all these bodies form round one of them a closed sphere the temperature of which, variable with the time, is not the same throughout, the passage of heat to the surface of the interior body does not depend on its temperature and that of the inclosure only, at least when these bodies are formed of the same material. After having considered the influence of the air upon radiation which we had at first eliminated, we give at the end of this chapter a formula which expresses the instantaneous variations of temperature of two material particles of insensible magnitude, by means of which the exchange of heat takes place after one or many reflexions upon the surfaces of other bodies through air or through any gas whatever.

CHAPTER III. *The Laws of Cooling in Bodies having the same Temperature throughout.*—While a homogeneous body of small dimensions is heating or cooling, its variable temperature is the same at every point; but if the body is composed of many parts formed of different substances in juxtaposition, they may preserve unequal temperatures during all the time that these temperatures vary, as we show in another chapter. In the present we determine, in functions of the time, the velocity and the temperature which we suppose to be common to all the points in a body placed alone in a sphere either vacuous or full of air, and the temperature of which is variable. If the sphere contains many bodies subject to their mutual influence upon each other, the determination of their temperatures would depend on the integration of a system of simultaneous equations, which are only linear in the case of ordinary temperatures, but in which we cannot separate the variables when we investigate high temperatures, and when the radiation is supposed not to be proportional to their differences.

Experiment has shown that in a cooling body, covered by a thin layer or stratum of a substance different from that of which it is itself composed, the velocity of refrigeration only arrives at its maximum when the thickness of this additive stratum, though always very small, has notwithstanding attained a certain limit. We develop the consequences of this important fact in what regards extension of molecular radiation, and explain how those consequences agree with the expression of the passage of heat found in the preceding chapter.

CHAPTER IV. *Motion of Heat in the Interior of Solid or Liquid Bodies.*—We arrive by two different processes at the general equation of the motion of heat; these two methods are exempt from the difficulties which the Committee of the Institute, which awarded the prize of 1812* to Fourier, had raised against the exactitude of the principle upon which his method was sustained. The equation under consideration is applicable both to homogeneous and heterogeneous bodies, solid or fluid, at rest or in motion. It was unnecessary, as they appeared to have thought, to find for fluids an equation different from the one I ob-

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* This Committee consisted of MM. Lagrange, Laplace, Legendre, Haüy and Malus.

tained long since for heterogeneous bodies. The variations of temperature which take place at every instant, and arise from the mutual radiation of the neighbouring molecules, depend in fact only on their actual positions, and not at all on the positions in which they will be the instant after in consequence of the motions produced by their calorific action or by other causes: it is thus that in the problem of the flux and reflux of the tides, for example, we calculate the attraction of the sea upon each point of its mass, as if it were solid and at rest at the moment under consideration, and independently of the motions which this attraction may produce.

Notwithstanding that the interior radiation takes place only between molecules the temperatures of which are extremely different, the equation of motion of the heat contains terms derived from the squares of their differences, and of the same order of magnitude as those which result from their first power; so that the exact equation differs, in the case of a homogeneous body, from that which we had already given; and it is not, like that, independent of the conductibility when the body has arrived at an invariable state. This equation of partial differences changes its form, when we cannot consider the extent of the interior radiation as insensible; it is then of a higher order, which introduces, in its integral, new constants or arbitrary functions. From this a difficulty of analysis arises, of which we give the solution, and explain how in every case the redundant quantities will be made to disappear, as will be seen from a particular example in another chapter. We form in this the general expression of the passage of heat through every element of a surface traced in the interior of a body which is heated or cooled, or has arrived at an invariable state, and in which the extent of the interior radiation is considered as insensible. This passage proceeds from the exchange of heat between the molecules of the two parts of that body near their surface of separation, and the temperatures of which are very different; whilst the interior passage results from the exchanges between the molecules adjacent to the surface of the body and those of a surrounding medium, or of other bodies which may have much higher or much lower temperatures; and notwithstanding that the respective magnitudes of these two passages (*ces deux flux*), due to causes also unequal, must be of the same order and comparable with one another. We show how that condition is fulfilled, by means of a quantity resulting from the rapid decrease of temperature which takes place very near the surface of a body whilst heating or cooling. In this manner interior and exterior passages are found united with one another; and the law of interior conductibility expressed in functions of the temperature is deduced from that of exterior radiation which MM. Dulong and Petit have discovered.

In a homogeneous prism which has arrived at an invariable state, and the lateral surface of which is supposed to be impermeable to heat and its two bases retained at constant temperatures, the passage of heat across every section perpendicular to its length is the same throughout its length. Its magnitude is proportional to the temperature of the two bases, and in the inverse ratio of the distance which separates them. This principle is easy to demonstrate, or rather it may be considered as evident. Thus expressed, it is independent of the mode of communication of heat, and it takes place whatever be the length of the prism: but it was erroneous to have attributed it without restriction to the infinitely thin slices of one body, the temperature of which varies, either with the time, or from one point to another; and to have excluded from it the circumstance, that the equation of the movement of heat, deduced from that of extension, is independent of any hypothesis and comparable in its generality to the theorems of statics. When we make no supposition respecting the mode of communication of heat, or the law of interior radiation, the passage of heat through each face of an infinitely thin slice is no longer simply proportional to the infinitely small difference of the temperatures of the two faces, or in the inverse ratio of the thickness of the slices; the exact expression of it will be found in the chapter in which we treat specially of the distribution of heat in a prismatic bar.

Chapter V. *On the Movement of Heat at the Surface of a Body of any Form.*—We demonstrate that the passages of heat are equal, or become so after a very short time, in the two extremities of a prism which has for its base an element of the surface of a body, and is in height a little greater than the thickness of the superficial layer, in which the temperature varies very rapidly. From this equality, and from the expression of the exterior radiation, given by observation, we determine the equation of the motion of heat at the surface of a body of any form whatsoever. The expression of the interior passage not being applicable to the surface itself, it follows that the demonstration of this general equation, which consists in immediately equalizing that expression to the expression of the exterior radiation, is altogether illusory.

When a body is composed of two parts of different materials, two equations of the motion of heat exist at their surface of separation, which are demonstrated in the same manner as the equation relative to the surface; they contain one quantity depending on the material of those two parts respectively, and which can only be determined by experiment.

Chapter VI. *A Digression on the Integrals of Equations of partial Differences.*—By the consideration of series, we demonstrate that the number of arbitrary constants contained in the complete integral of a differential equation ought always to be equal to that which indicates the order of that equation: we prove by the same means, that in the integral of an equation of partial differences the number of arbitrary functions may be less, and change as the integral is developed in series, according to the powers of one or other variable; and when the equation of partial differences is linear, we show that by conveniently choosing this variable all the arbitrary functions may disappear and be replaced by constants, infinite in number, without the integral ceasing to be complete. To elucidate these general considerations, we apply them to examples by means of which we show that the different integrals, in the series of the same equation of partial differences, are transformed into one another, and may be expressed under a finite form by definite integrals, which also contain one or several arbitrary functions. In the single case, in which the integral in series contains only arbitrary constants, every term of the series by itself satisfies the given equation, so that the general integral is found expressed by the sum of an infinite number of particular integrals. Integrals of this form have appeared from the origin of the calculus of partial differences; but in order that their use in different problems should not leave any doubt respecting the generality of the solutions, it would have been necessary to have demonstrated *à priori*, as I did long since, that these expressions in series, although not containing any arbitrary function, as well as those containing a greater or smaller number of them, are not less on that account the most general solutions of equations of partial differences; or else it would have been necessary to verify in every example that, after having satisfied all the equations of one problem relative to contiguous points infinite in number, the series of this nature might still represent the initial and entirely arbitrary state of this system of material points; a verification which, until now, it has not been possible to give, except in very particular cases. The solution which Fourier was the first to offer of the problem of the distribution of heat in a homogeneous sphere, of which all the points equidistant from the centre have equal temperatures, does not satisfy for example either of these two conditions; it was no doubt on this account that the members of the Committee, whose judgement we mentioned above, thought that his (Fourier's) analysis was not satisfactory in regard to generality; and, in fact, in this solution it is not at all demonstrated that the series which expresses the initial temperature can represent a function, entirely arbitrary, of the distance from the centre.

For the use of these series of particular solutions, it will be necessary to proceed in a manner proper to determine their coefficients according to the initial state of the system. On the occasion of a problem relative to the heat of a sphere composed of two different substances, I have given for this purpose in the *Journal de l'Ecole Polytechnique*, (*cahier* 19, p. 377 *et seq.*,) a direct and general method, of which I have since made a great number of applications, and which I shall also constantly follow in this work. The Sixth Chapter contains already the application to the general equations of the motion of heat in the interior and on the surface of a body of any form either homogeneous or heterogeneous. It leads in every case to two remarkable equations, one of which serves to determine, independently of one another, the coefficients of the terms of each series, and the other to demonstrate the reality of the constant quantities by which the time is multiplied in all these terms. These constants are roots of transcendental equations, the nature of which it will be very difficult to discover, by reason of the very complicated form of these equations. From their reality this general consequence is drawn; viz. when a body, heated in any manner whatever, is placed in a medium the temperature of which is zero, it always attains, before its complete cooling, a regular state in which the temperatures of all its points decrease in the same geometrical progression for equal increments of time. We shall demonstrate in another chapter, that, if that body is a homogeneous sphere, these temperatures will be equal for all the points at an equal distance from the centre, and the same as if the initial heat of each of its concentric strata had been uniformly distributed throughout its extent.

The equations of partial differences upon which depend the laws of cooling in bodies are of the first order in regard to time, whilst the equations relative to the vibrations of elastic bodies and of fluids are of the second order; there result essential differences between the expressions of the temperatures and those of the velocities at a given instant, and for that reason it appears at least very difficult to conceive that the phænomena which may result from a molecular radiation should be equally explicable by attributing them to the vibrations of an elastic fluid. When we have obtained the expressions of the unknown quantities in functions of the time, in either of these kinds of questions, if we make the time in them equal to zero, we deduce from that, series of different forms which represent, for all the points of the system which we consider, arbitrary functions, continuous or discontinuous, of their coordinates. These expressions in series, although we might not be able to verify them, except in particular examples, ought always to be admitted as a necessary consequence of the solution of every problem, the generality of which has been demonstrated *à priori*; it will however be desirable that we should also obtain them in a more direct manner; and we might perhaps so attain them, by means of the analysis of which I had made use in my first Memoir on the theory of heat, to determine the law of temperatures in a bar of a given length, according to the integral under a finite form of the equation of partial differences.

CHAPTER VII. *A Digression on the Manner of expressing Arbitrary Functions by Series of Periodical Quantities.*—Lagrange was the first to give a series of quantities proper to represent the values of an arbitrary function, continuous or discontinuous, in a determined interval of the values of the variable. This formula supposes that the function vanishes at the two extremes of this interval; it proceeds according to the sines of the multiples of the variable; many others exist of the same nature which proceed according to the sines or cosines of these multiples, even or uneven, and which differ from one another in conditions relative to each extreme. A complete theory of formulæ of this kind will be found in this chapter, which I have abstracted from my old memoirs, and in which I have considered the periodical series which they contain as limits of other converging series, the sums of which are integrals, themselves having for limits the arbitrary functions which it is our object to represent. Supposing in one or other of these expressions in series, the interval of the values of the variable for which it takes place to be infinite, there results from it the formula with a double integral, which belongs to Fourier; it is extended without difficulty, as well as each of those which only subsists for a limited interval, to two or a greater number of variables.

CHAPTER VIII. *Continuation of the Digression on the Manner of representing Arbitrary Functions by Series of Periodical Quantities.*—An arbitrary function of two angles, one of which is comprised between zero and 180°, and the other between zero and 360°, may always be represented between those limits by a series of certain periodical quantities, which have not received particular denominations, although they have special and very remarkable properties. It is to that expression in series that we have recourse in a great number of questions of celestial mechanics and of physics, relative to spheroids; it had however been disputed whether they agreed with any function whatever; but the demonstration of this important formula, which I had already given and now reproduce in this chapter, will leave no doubt of its nature and generality. This demonstration is founded on a theorem, which is deduced from considerations similar to those of the preceding chapter. We examine what the series becomes at the limits of the values of the two angles; we then demonstrate the properties of the functions of which its terms are formed; then it is shown that they always end by decreasing indefinitely, which is a necessary consequence and sufficient to prevent the series from becoming diverging, for which purpose its use is always allowable. Finally, it is proved, that for the same function there is never more than one development of that kind; which does not happen in the developments in series of sines and cosines of the multiples of the variables. This chapter terminates with the demonstration of another theorem, by means of which we reduce a numerous class of double integrals to simple integrals.

CHAPTER IX. *Distribution of Heat in a Bar, the transverse Dimensions of which are very small.*—We form directly the equation of the motion of heat in a bar, either straight or curved, homogeneous or heterogeneous, the transverse sections of which are variable or invariable, and which radiates across its lateral surface. We then verify the coincidence of this equation with that which is deduced from the general equation of Chapter IV., when the lateral radiation is abstracted and the bar is cylindrical or prismatic. This equation is first applied to the invariable state of a bar the two extremities of which are kept at constant and given temperatures. It is then supposed, successively, that the extent of the interior radiation is not insensible, that the exterior radiation ceases to be proportional to the differences of temperature, that the exterior conductibility varies according to the degree of heat, and the influence of those different causes on the law of the permanent temperatures of the bar is determined. Formulæ are also given, which will serve to deduce from this law, by experiment, the respective conductibility of different substances, and the quantity relative to the passage from one substance into another, in the case of a bar formed of two heterogeneous parts placed contiguous to and following one another. After having thus considered in detail the case of permanent temperatures, we resolve the equation of partial differences relative to the case of variable temperatures; which leads to an expression of the unknown quantity of the problem, in a series of exponentials, the coefficients of which are determined by the general process indicated in Chapter VII., whatever may be the variations of substance and of the transverse sections of the bar. We then apply that solution to the principal particular cases. When the bar is indefinitely lengthened, or supposed to be heated only in one part of its length, the laws of the propagation of heat on each side of the heated place are determined; this propagation is instantaneous to any distance; a result of the theory presenting a real difficulty, but the explanation of which is given.

CHAPTER X. *On the Distribution of Heat in Spherical Bodies.*—The problem of the distribution of heat in a sphere, all the points of which equally distant from the centre have equal temperatures, is easily brought to a particular case of the same question with regard to a cylindrical bar. It is also solved directly; the solution is then applied to the two extreme cases, one of a very small radius, and another of a very great one. In the case of an infinite radius, the laws are inferred of the propagation of caloric in a homogeneous body, round the part of its mass to which the heat has been communicated, similarly in all directions.

We then determine the distribution of heat in a homogeneous sphere covered with a stratum, also homogeneous, formed of a substance different from that of the nucleus. During the whole time of cooling, the temperature of this stratum, however small its thickness may be, is different from that of the sphere in the centre, and the ratio of the temperatures of these two parts, at the same instant, depends on the quantity relative to the passage from one substance into the other, of which we have already spoken. From this circumstance an objection arises against the method employed by all natural philosophers to determine, by the comparison of the velocities of cooling, the ratio of the specific heat of different bodies, after having brought their surfaces to the same state by means of a very thin stratum of the same substance for all these bodies. The quantity relative to the passage of the heat of each body in the additive stratum, is contained in the ratio of the velocities of cooling; it is therefore necessary that it should be known in order to be able to deduce from this ratio, that of their specific heats. A recent experiment by M. Melloni proves that a liquid contained in a thin envelope, the interior surface of which is successively placed in different states by polishing or scratching it, always cools with the same velocity, whilst the ratios of the velocity change very considerably, as was known long before, when it is the exterior part of the vessel that is more or less polished or scratched. The quantity relative to the passage of caloric across the surface of separation of the vessel and the liquid, is therefore independent of the state of that surface, a circumstance which assimilates the cooling power of liquids to that of the stratum of air in contact with bodies, which in the same manner does not depend on the state of their surface, according to the experiments of MM. Dulong and Petit.

When a homogeneous sphere, the cooling of which we are considering, is changed into a body terminated by an indefinite plane, and is indefinitely prolonged on one side only of that plane, the analytical expression for the temperature of any point whatever changes its form, in such a manner that that temperature, instead of tending to diminish in geometrical progression, converges continually towards a very different law, which depends on the initial state of the body; but however great a body may be, it has always finite and determined dimensions; and it is always the law of final decrease enunciated in Chapter VI. which it is necessary to apply; even in the case, for example, of the cooling of the earth.

If the distribution of heat in a sphere, or in a body of another form, has been determined, by supposing this body to be placed in a medium the temperature of which is zero, this first solution of the problem may afterwards be extended to the case in which the exterior temperature varies according to any law whatever. In my first Memoir on the theory of heat, I have followed, in regard to this part of the question, a direct method applicable to all cases. According to this method, one part of the value of the temperature in a function of the time is expressed in the general case by a quadruple integral, which can always be reduced to a double integral like each of the other parts. By the method which I have used to effect this reduction we obtain the value of different definite integrals, which it would be difficult in general to determine in a different manner, and the accuracy of which is verified whenever they enter into known formulæ.

CHAPTER XI. *On the Distribution of Heat in certain Bodies, and especially in a homogeneous Sphere primitively heated in any Manner.*— It is explained how, in every case, the complete expression of exterior temperature, which may depend on the different sources of heat, and which must be employed in the equation of the motion of heat relative to the surface of bodies submitted to their influence, will be formed.

After having enumerated the different forms of bodies for which we have hitherto arrived at the solution of the problem of the distribution of heat, the complete solution is given for the case of a homogeneous rectangular parallelopiped the six faces of which radiate unequally.

In order to apply the general equations of the fourth and fifth chapters to the case of a homogeneous sphere primitively heated in any manner, the orthogonal coordinates in them are transformed into polar coordinates; the temperature at any instant and in any point is then expressed by means of the general series of Chapter VIII., and of the integrals found in Chapter VI.; the coefficients of that series are next determined according to the initial state of the sphere, by supposing at first the exterior temperature to be zero: by the process already employed in the preceding Chapter, this solution is finally extended to the case of an exterior temperature, varying with the time and from one point to another. Among the consequences of this general solution of the problem the most important is that for which we are indebted to Laplace; it consists in this: That in a sphere of very large dimensions, and at distances from the surface very small in proportion to its radius, the part of the temperature independent of the time does not vary sensibly with these distances; and, that upon the normal at each point, whether at the surface or at an inconsiderable depth, it may be regarded as equal to the invariable part of the exterior temperature which corresponds to the same point. Hence it results, that the increase of heat in the direction of the depth which is observed near the surface of the earth cannot be attributed to the inequality of temperatures of different climates, and that it is necessary to look for the cause in circumstances which vary very slowly with the time. Whatever this cause may be, the difference of the mean temperatures of the surface and beyond, corresponding to the same point of the superficies, is proportional (according to a remark made by Fourier) to the increase of temperature upon the normal referred to the unity of length, so that this difference may be determined from the observed increase, and from a quantity depending on the nature of the ground. This remark and that of Laplace are not applicable to the localities where the temperature varies very rapidly round the vertical: it is shown that in these cases of exception the temperature varies even upon the vertical: and the law of this variation is determined from the variation which has taken place at the surface or in the exterior temperature. The mean temperature at a very small distance contains also a term which is not proportional to this depth, and which arises from the influence of the heat on the conductibility of the substance.

CHAPTER XII. *On the Motion of Heat in the Interior and upon the Surface of the Earth.*—It is shown that the formulæ of the preceding chapter, although relating to a homogeneous sphere the surface of which is everywhere in the same state, may notwithstanding serve to determine the temperatures of the points of the earth at a distance from the surface which is very small with regard to its radius, but which exceeds however all accessible depths. These formulæ contain two constants, depending on the nature of the soil, the numerical values of which may be determined in every point of the globe from the temperatures observed at different known depths.

Observation in harmony with theory shows that the diurnal inequalities of the temperature of the earth disappear at very small depths, and the annual inequalities at greater depths, in such a manner that at a distance from the surface of about 20 metres and beyond those two kinds of inequalities are entirely insensible. In this chapter are given tables of the temperatures, indicated by the thermometer, of the caves of the Observatory, at the depth of 28 metres. The mean of 352 observations, made from 1817 to the end of 1834, is 11°·834.

The increase of the mean temperature of the earth, in proportion as we descend below the surface, has long been established as a fact in all deep places, at different latitudes, and at different elevations of the soil above the level of the sea. The most adequate means to determine it is by *sounding* and boring. The results, still very few, which have hitherto been obtained are given. At Paris, this increase appears to be one degree for about 38 metres of increase in depth.

As to the cause of this phænomenon, the difficulties are stated which the explanation of Fourier presents, founded upon the original heat of the globe, still sensible at the present time near the surface; the new explanation alluded to at the beginning of this article is then proposed. The following reflections extracted from the work tend to prove that the solidification of the earth must have commenced by central strata, and that before reaching the surface the cooling of the globe must have been incomparably more rapid.

"The nearly spherical form of the earth and planets, and their flattening at the poles of rotation, evidently show that these bodies were originally in a fluid or perhaps in an aëriform state. Beginning from this initial state, the earth could not, wholly or partly, become solid, except by a loss of heat arising from its temperature exceeding that of the medium in which it was placed. But it is not demonstrated that the solidification of the earth could have commenced at the surface and been propagated towards the centre, as the state of the globe still fluid in the greatest part of the interior would lead us to suppose; the contrary appears to me more probable. For the extreme parts, or those nearer to the surface, being the first cooled, must have descended to the interior and been replaced by internal portions which had ascended to cool at the surface and to descend again in their turn. This double current must have maintained an equality of temperature in the mass, or at least must have prevented the inequality from becoming in any way so great as in a solid body, which cools from the surface; and we may add that this mixture of the parts of the fluid, and the equalization of their temperatures, must have been favoured by the oscillations of the whole mass, which must have taken place until the globe attained a permanent figure and rotation. On the other hand, the excessively great pressure sustained by the central strata may have determined their solidification long before that of those nearer the surface; that is to say, the first may have become solid by the effect of this extreme pressure at a temperature equal or even superior to that of the strata more distant from the centre, and consequently subjected to a much less degree of pressure. Experiment has shown, for example, that water at the ordinary temperature being submitted to a pressure of 1000 atmospheres, experiences a condensation of about $\frac{1}{20}$th of its primitive volume. Now let us conceive a column of water whose height is equal to one radius of the globe, and let us reduce its weight to half of that which we observe at the surface of the earth, in order to render it equal to the mean gravity which would exist along each radius of the earth upon the hypothesis of its homogeneity; the inferior strata of this liquid column would experience a pressure of more than three millions of atmospheres, or equal to more than three thousand times the pressure which would reduce water to $\frac{19}{20}$ths of its volume; but without knowing the law of the compression of this liquid, and although we do not know in what manner this law may depend on the temperature, we may believe, notwithstanding, that so enormous a pressure would reduce the inferior strata of the mass of water to the solid state, even when the temperature is very high. It seems therefore more natural to conceive that the solidification of the earth began at the centre and was successively propagated towards the surface: at a certain temperature, which might be extremely high, the strata nearer the centre became at first solid, by reason of the excessive pressure which they experienced; the succeeding strata were then solidified at a lower temperature and under a less degree of pressure, and thus in progressive succession to the surface."

If the increase observed in the temperature of the earth near its surface is due to its original heat, it follows that at the present epoch at Paris this heat raises the temperature of the surface itself only by the fortieth part of a degree. Not knowing the radiating power of the substance of the globe, we cannot estimate the quantity of this initial heat which traverses in a given time from within to without an extent, also given, of the surface; but such would be its slowness in dissipating into space, that more than one thousand million of centuries must elapse to reduce the small increase of the fortieth of a degree to one half.

With regard to periodical inequalities, the relation which exists between each inequality at a given depth and the inequality corresponding to the exterior temperature is determined. Relations of this nature, for the knowledge of which we are indebted to M. Fourier, take place between the interior inequalities and those of the surface of the ground; these relations leave unknown the ratios of these latter inequalities to those of the outside which are the immediate data of the question.

The interior temperature to which the earth is subjected arises from three different sources, namely, from sidereal heat, from atmospherical heat, acting either by radiation or by contact, and from solar heat. These three sources of heat are successively examined. With regard to the first it is observed, that it is not at all probable that radiant heat emanating from the stars has the same intensity in all directions when it arrives at the earth. The experiments are indicated which it would be necessary to make in order to ascertain whether it really varies in the different regions of the sky. M. Melloni intends immediately to apply himself to these experiments, employing in them an extremely sensible instrument, of which he has made use in his researches on heat; a circumstance which cannot fail to lead to the solution of this important question of celestial physics.

Before considering the influence of atmospherical heat, I have formed a complete expression for the temperature, marked every instant by a thermometer suspended in the air, at any height above the surface of the earth exposed in the shade or in the direct rays of the sun. Although the greatest part of the quantities which this formula contains are unknown to us, many general consequences may however be deduced from it, which accord with experiment; it hence follows, that to determine the proper temperature of the air, it is necessary to employ the simultaneous observation of three thermometers, the surfaces of which are in a different state, and not two thermometers only, as is generally said. This formula also furnishes the means of comparing the temperatures indicated by different thermometers in relation to their radiating powers and to their property of absorbing the rays of the sun.

The mean of the annual temperatures, marked by a thermometer exposed in the open air and in the shade, forms the *climateric* temperature. It varies with the elevation of places above the level of the sea, and with the longitude and latitude, according to unknown laws. At Paris it is 10°·822, as M. Bouvard has concluded after 29 years of observations. There will be found in this Chapter a table of the mean temperatures for the twelve months of each of those years, which that gentleman has been pleased to communicate to us, and which had not before been published. It appears that in every point of the earth this climateric temperature differs very little from the mean temperature of the surface of the soil, as is shown by several examples. Notwithstanding, the variable temperature of this surface, and that which is marked at the same instant by a thermometer as little elevated above the surface as may be, are often very different from each other; it hence follows, that in a year the excess of the highest above the lowest temperature of the soil is at Paris nearly 24°, as will be seen in the course of this Chapter; and only about 17° for the thermometer suspended in the air and in the shade.

We now determine the part of exterior temperature which results from the atmospherical heat combined with sidereal heat. The necessary data for calculating its numerical value, *à priori*, being unknown to us, we show how this value, for every point of the globe, may be deduced from the mean temperature of its surface. At Paris this exterior temperature is 13°. Although we cannot determine separately the portion of this temperature of the earth which arises from the atmospherical heat, there is reason to think that it is also negative, so that the other portion arising from sidereal heat must be less than 13° below zero. If we suppose that radiant heat emanating from the stars falls in the same quantity on all points of the globe, this temperature, higher than 13°, will be that of space at the place where the earth is at this time. Without being able to assign the degree of heat of space, we may however admit, that its temperature differs little from zero, instead of being, as had been asserted, below the temperature of the coldest regions in the globe, and even of the freezing-point of mercury. As to the central temperature of the whole mass of the earth, even supposing its original heat to be entirely dissipated, and that it is no longer equal to the present temperature of space, we have no means of obtaining a knowledge of it.

According to a theorem of Lambert, the whole amount of solar heat which falls upon the earth is the same during different seasons, notwithstanding the inequality of their lengths, which is found to be compensated by that of the distances from the sun to the earth. This quantity of heat varies in the inverse ratio of the parameter of the ellipse described by the earth; it also varies with the obliquity of the ecliptic, but it does not appear that these variations can ever produce any considerable effect on the heat of the globe. The quantities of solar heat which fall in equal times upon the two hemispheres are nearly equal; but on account of the different states of their surfaces, those quantities are absorbed in different proportions; and the power of absorbing the rays of the sun increasing in a greater ratio than the radiating power, which is greater for dry land than for the sea, we conclude that the mean temperature of our hemisphere, where dry land is in a greater proportion, must be greater than that of the southern hemisphere; which agrees with observation.

The solar heat, which reaches each point of the globe, varies at different hours of the day; it is null when the sun is beneath the horizon; during the year it varies also with its declination; and the expression changes its form as the latitude of the point under consideration is greater or less than the complement of the obliquity of the ecliptic. I have therefore considered the part of the exterior temperature which arises from this source of heat as a discontinuous function of the horary angle, and of the longitude of the sun, to which I have applied the formulæ of the preceding Chapters, in order to convert it into series of sines and cosines of the multiples of these two angles. By this means I have obtained the complete expressions of the diurnal and annual inequalities of the temperature of the earth which arise from its double motion. These formulæ show, that at the equator the annual inequalities are much less than elsewhere; a circumstance which furnishes the explanation of a fact observed by M. Boussingault in his journey to the Cordilleras, and upon which he had relied in order to determine with great facility the climateric temperatures of the places which he visited. The same formulæ agree also, in a remarkable manner, with the temperatures which M. Arago has observed at Paris during many years, and at depths varying from two to eight metres (from 6·56 to 26·24 English feet).

Article VII.

Researches on the Elasticity of Bodies which crystallize regularly; by Felix Savart.

(Read to the Academy of Sciences of Paris, January 26th, 1829.)

From the *Annales de Chimie et de Physique*, vol. xl. p. 5, *et seq.*

HITHERTO precise notions respecting the intimate structure of bodies could be acquired only by two means: first by cleavage, for opake or transparent substances regularly crystallized; secondly, for transparent substances only, by the modifications which they occasion in the propagation of light.

The first of these means has taught us that crystallized bodies are collections of laminæ parallel to certain faces of the crystal; but it has given us no information respecting the force with which these laminæ adhere together nor their elastic state. The second, far more powerful than the first, because it renders evident actions depending on the very form of the particles, has given rise to the discovery of phænomena the existence of which cleavage alone would never have allowed us to suspect. But although these two experimental processes have introduced many new ideas and notions into the science, yet it may be said that the part of physics which treats of the arrangement of the particles of bodies, and the properties resulting from it, as elasticity, hardness, fragility, malleability, &c. is still in its infancy.

The investigations of Chladni respecting the modes of vibration of laminæ of glass or metal, and the researches which I have published on the same subject, especially those which relate to the modes of division of discs of a fibrous substance, such as wood, allow us to suspect that we might acquire by this means new notions respecting the distribution of elasticity in solid bodies; but it was not clearly seen by what process this result might be attained, though the road which it was necessary to follow was one of great simplicity.

But if this mode of experiment, which we are about to describe, is simple in itself, it is not the less surrounded by a multitude of difficulties of detail, which cannot be removed without numerous attempts; and I hope this will serve to excuse the incompleteness of these researches, which I only give as the first rudiments of a more extensive investigation.

§ 1. *Statement of the Means of Examination employed in these Researches.*

Circular plates which produce normal vibrations are susceptible of several modes of division; sometimes they are divided into a greater or fewer number of equal sectors, always even in number, which perform their vibrations in the same time; at other times they are divided into a greater or fewer number of concentric zones; and these two series of modes of division again may be combined together, so that the acoustic figures which result are circular lines divided into equal parts by diametrical nodal lines.

If the plate which is caused to sound is perfectly homogeneous, circular, and equal in thickness, it is obvious that in the case when the figure consists of diametrical lines only, the system which they form ought to be capable of placing itself in every direction, that is to say, that any point whatever of the circumference of the plate, being taken as the place of excitation, this single condition determines the position of the nodal figure, since the point directly put in motion is always the middle of a vibrating part. In the case of circular lines, under the conditions we have just supposed, these lines would be exactly concentric with the circumference of the plate. These results are a natural consequence of the symmetry which is supposed to exist either in the form or in the structure of the plate; but if this symmetry is deranged, it will easily be conceived that an acoustical figure composed of diametrical nodal lines ought no longer to place itself in a direction depending solely on the position of the point of excitation, and that, with regard to a figure consisting of circular lines, these lines ought to be modified, and will become, for example, elliptical or of some other more complicated form. It is thus that the system of two nodal lines which intersect each other rectangularly, can upon an elliptical plate only place itself in a single position, which is on the axes of the ellipse. There is however a second position in which this mode of division can establish itself; but then it is modified in its form, and it resembles the two branches of a hyperbola, the transverse axis of which corresponds with the greater axis of the ellipse: in this latter case, the number of vibrations is less than in the first, and more so as the axes of the ellipse differ more from each other. A similar phænomenon is observed when the same mode of division is attempted to be produced on a circular plate of brass, of very equal thickness, and in which several parallel saw-cuts have been made, penetrating only to a small distance from the surface: one of the crossed nodal lines always corresponds to a saw-cut which has been made in the direction of a diameter, and the system of the two hyperbolic lines arranges itself in such a manner that the same saw-cut becomes the conjugate axis of the hyperbola. Thus, in both cases, the transverse axis of the hyperbola is always in the direction of the least resistance to flexure.

Let us now suppose that, the plate remaining perfectly circular and of equal thickness, it possesses in its plane a degree of elasticity which is not the same in two directions perpendicular to each other; the symmetrical disposition round the centre being then found to be destroyed, although in another manner than in the two examples we have just adduced, an analogous result ought still to be obtained.

Thus, if we take a plate of this description, a plate of wood, for instance, cut parallel to the fibres, and fixing it lightly by its centre, endeavour to make it produce the mode of division consisting of two lines crossed rectangularly, we shall find that when it thus divides itself, the lines of rest always place themselves according to the directions of the greatest and least resistance to flexure, and that putting it afterwards in motion at the extremity of the preceding lines, it may be made to produce a second mode of division, which presents itself under the aspect of a hyperbola the branches of which are much straightened, and which would have for its conjugate axis that line of the cross which corresponds to the direction of the greatest resistance to flexure. In short, when the symmetrical disposition round the centre is destroyed, no matter in what way, the mode of division formed by two nodal lines which intersect each other rectangularly can place itself only in two determinate positions, for one of which it presents frequently the appearance of two hyperbolic branches more or less straightened; and, as we shall soon see, it may even happen that, for certain distributions of elasticity, this mode of division presents itself under the form of two hyperbolic curves in the two positions in which it becomes possible. Lastly, if a similar plate be caused to produce some of the high modes of division, but yet consisting of diametrical lines, experiment shows that they can likewise place themselves in two invariable positions, and pass through certain modifications analogous to those which the system of two lines crossed at right angles undergoes. Thus the immoveability of the nodal figures, and the double position which they can assume, are distinctive characters of circular plates all the diameters of which do not possess a uniform elasticity or cohesion.

It follows therefore from the preceding, that by forming with different substances circular plates of very equal thickness, we may, by the fixed or indeterminate position of an acoustic figure consisting of diametrical nodal lines, ascertain whether the properties of the substance in question are the same in all directions. By applying this mode of examination to a great number of plates formed of different substances regularly or confusedly crystallized, as the metals, glass, sulphur, rock-crystal, carbonate of lime, sulphate of lime, gypsum, &c., it is constantly found that the acoustic figure, formed of two lines crossed rectangularly, can only place itself on them in a single position; and that there is a second position in which two hyperbolic curved lines are obtained which are accompanied, according to the different cases, by a sound which differs more or less from that which is produced when the crossed lines occur. Plates are also met with which are incapable of assuming the mode of division formed of two straight lines, and in which only two systems of hyperbolic curves are obtained, sometimes similar, yet giving different sounds. In short, I have yet found no body for which the same nodal figure can place itself in every direction; which seems to indicate that there are very few solid substances which possess the same properties throughout. But what appears still more extraordinary is, that if in the same body, a mass of metal for instance, plates are cut according to different directions, some are susceptible of the mode of division consisting of two lines which cross each other rectangularly, whilst others present only two systems of hyperbolic curves. In both cases, the sounds of the two systems may differ greatly: there may, for example, be an interval between them of more than a fifth.

To arrive at the discovery of the experimental laws of this kind of phænomena, it would be necessary therefore to be able to study them, at first in the most simple cases, for example, upon bodies the elastic state of which, previously known, would differ only according to two directions. This would obtain in a body which might be composed by placing flat plates formed of two heterogeneous substances upon each other in such a manner that all the odd plates might be of one substance, and all the even plates of another, the elasticity in all directions of the plane of each of them being the same. But it has appeared to me difficult to attain this condition, since I have yet found no body the elasticity of which was the same in all directions.

The most simple structure after the preceding would be that of a body composed of cylindrical and concentric layers, the nature of which should be alternately different for the layers next each other, as is nearly the case in the branch of a tree free from knots. It is evident that the elasticity ought to be sensibly the same in every direction of the plane of a plate cut perpendicularly to the axis of the cylinder, and it ought to differ greatly from that which is observed in the direction of the axis. Consequently we shall commence by examining this first case; after which we shall pass to that in which the elasticity would be different according to three directions perpendicular to each other, as would take place in a body composed of flat plates alternately of two different substances, and the elastic state of which would not be the same, according to two directions perpendicular to each other. Wood fulfills again these different conditions; for in a tree of very considerable diameter, the ligneous layers may be considered as sensibly plane for a small number of degrees of the circumference; and if we confine ourselves to plates of a small diameter, cut at a little distance from the surface, we may suppose without any very notable error, at least for the whole of the phænomena, that the experiments have been made on a body the elasticity of which is not the same, according to three directions rectangular to each other, since, as is well known, this property does not exist in the same degree according to the direction of the fibres, according to that of the radius of the tree, and according to a direction perpendicular to the fibres and tangential to the ligneous layers.

After these two cases—the most simple that we have been able to study—we shall pass to the much more complicated phænomena which regularly crystallized bodies, such as rock crystal and carbonate of lime, present.

§ II. *Analysis of Wood by means of Sonorous Vibrations.*

Let us suppose that fig. 1 (Plate III.) represents a cylinder of wood the annual layers of which are concentric to the circumference; let B C D E, fig. 2, be any plane passing through the axis A Y of the cylinder, and let $n\,n'$ be a line normal to this plane: it is obvious that the plates taken perpendicularly to B C D E, and according to the different directions 1, 2, 3, 4, 5, &c. round $n\,n'$, ought to present different phænomena, since they all will contain the axis of least elasticity $n\,n'$ in their plane, and the resistance to flexure, according to the lines 1, 2, 3, 4, 5, will go on increasing in proportion as the plates shall more nearly approach being parallel to the axis of greatest elasticity A Y.

For the plate No. 1, fig. 3, perpendicular to this axis, all being symmetrical around the centre, the mode of division consisting of two lines which intersect each other at right angles, ought to be able to place itself in all kinds of directions, according as the place of excitation shall occupy different points of the circumference: this is really the case; but it is no longer so, for the plate No. 2 inclined 22° 5′ to the preceding. In the latter, the elasticity becoming a little greater in the direction $r\,s$ contained in the plane B C D E, than in the direction $n\,n'$ normal to this plane, this circumstance ought to determine the nodal lines to place themselves according to these two directions. However, as this difference is very slight, the system of these two lines may still be displaced, when the place of excitation is made to vary; but it will change its form a little, and it will assume the appearance of two hyperbolic branches when it has arrived at 45° from its first position. In the plate No. 3, inclined 45° to the axis A Y, the difference of the two extreme elasticities being greater, the system of crossed lines becomes entirely fixed, or rather it can only move through a few degrees to the right or left of the position which it assumes in preference; but the hyperbolic system, the summits a and b of which recede more from each other than in fig. 2, will present the remarkable peculiarity of being capable of transforming itself into the rectangular system, when the position of the point put directly in motion is made to vary.

Examining with care the nodal lines in fig. 2, it is found equally that its two nodal systems can thus change themselves one into the other; and the same phænomenon is reproduced in the plate No. 4, in which the values of the extreme elasticities differ still more, and in which the points *a* and *b* recede from each other at the same time as the curves become more straightened. In the plate No. 5, parallel to the axis A Y, these curves are no longer susceptible of assuming any other position than that indicated in the figure. Thus, in No. 1, the centres *a* and *b* coalesce into one, and there is only a single figure consisting of two crossed lines, the system of which can assume any position; these centres afterwards gradually receding, the modes of division can change themselves from one into the other, and at last, when the branches of the curve are nearly straight lines, the two figures become perfectly fixed.

The existence of these nodal points or centres is, without doubt, a very remarkable phænomenon, and which it will be important to study with great care. In order to give an accurate idea of it, I have in fig. 4 indicated by a dotted line the successive modifications which the two hyperbolic lines assume when the plate is fixed at one of the points *a* or *b*, and the place of excitation moves gradually from *e* to e' e'', passing over a quarter of the circumference of the plate. When the motion is excited in the vicinity of e'', the curves are by the union of their summits transformed into two straight lines which intersect each other rectangularly; and it is obvious that if it had been excited near e''', the two branches of the curve would re-appear, but with this peculiarity, that their transverse axis would take the position assumed by the conjugate, when the motion was produced on the other side of e''.

As to the numbers of the vibrations which correspond to each mode of division, for the different degrees of inclination of the plates, it will be seen by examining fig. 3, that, at first equal in No. 1, they go on continually increasing and receding from each other up to No. 5, which contains the axis of the cylinder; and it is indeed evident, that the elasticity in the direction perpendicular to the axis remaining the same for all the plates, whilst that which is perpendicular to this direction goes on continually increasing, this ought to be, in general, the progress of the phænomenon.

These experiments were made with plates of oak 8·4 cent. (3·3071 inches) in diameter, and 3^{m}·7 (·1456 inch) in thickness: they were repeated with plates of beech-wood, and analogous results were obtained; only the ratio between the two elasticities not being the same, the interval between the two sounds of each plate was found to be greater.

The most general consequence that can be deduced from the preceding experiments is, that in wood in which the annual layers are nearly cylindrical and concentric, the elasticity is sensibly uniform in all the diameters of any section perpendicular to the axis of the branch. We shall see further on, that plates of carbonate of lime or rock crystal, cut perpendicularly to the axis, very seldom present this uniformity of structure for all their diameters, although the modifications which such plates impress on polarized light appear symmetrical round this same axis.

In the case which we have just examined, two of the three axes of elasticity being equal, the phænomena are, as we have just seen, exempt from any great complication. It is not so when the three axes possess each a different elasticity: it would then be indispensable to cut, first a series of plates round each of the axes, then a fourth series round a line equally inclined with respect to the three axes, and lastly, it would be necessary again to take a series round each of the lines which divide equally into two the angle contained between any two of the axes; and notwithstanding the great number of results which would be obtained by this process, the end would be far from attained, since these different series would want connexion with each other, and consequently this process cannot give a clear idea of the whole of the transformations of the nodal lines. Nevertheless, I shall content myself to follow this route, which appears to me less complicated than any other, and is sufficient to render fully evident all the principal peculiarities of this kind of phænomena.

In order that the relative positions of the lines round which I have cut the different series of plates of which I have spoken, and the relations they have to the planes of the ligneous layers, as well as to the direction of their fibres, may be more easily represented, I shall refer them all to the edges of a cube A E fig. 5, the face of which A X B Z I shall suppose is parallel to the ligneous layers, and the edge A X to the direction of the fibres, which will allow the three edges A X, A Y, A Z to be considered as being themselves the axes of elasticity. Afterwards I shall indicate the different degrees of inclination of the plates of each series, on a plane normal to the line round which they are to be cut; the position and outline of this plane being at the same time referred to the natural faces of the cube.

But before commencing to describe the phænomena which each of these series presents, it is indispensable to endeavour to determine the ratio of the resistance to flexion, in wood, in the direction of each of the three axes of elasticity: this may be easily done by means of vibrations, by cutting three small square prismatic rods, of the same dimensions, according to the three directions just indicated; for, the degree of their elasticity can be ascertained by comparing the numbers of the vibrations which they perform, for the same mode of division, knowing besides that, in reference to the transversal motion, the numbers of the vibrations are as the square roots of the resistance to flexion, or, which is the same thing, that the resistance to flexion is as the square of the number of oscillations.

Fig. 6 shows the results of an experiment of this kind which was made upon the same piece of beech-wood from which I cut all the plates which I shall mention hereafter. In this figure I have, to impress the mind more strongly, given to these rods directions parallel to the edges A X, A Y, A Z of the cube fig. 5, and I have supposed that the faces of the rods are parallel to those of the cube. It is to be remarked that two sounds may be heard for the same mode of division of each rod, according as it vibrates in *a b* or *c d*; but when they are very thin the difference which exists between them is so slight that it may be neglected. The inspection of fig. 6 shows, therefore, that the resistance to flexion is the least in the direction A Z, and is such, that being represented by unity, the resistance in the direction A Y becomes 2·25, and 16 in the direction of A X. It is evident that the elasticity in any other direction must be always intermediate to that of the directions we have just considered.

This being well established, we shall proceed to the examination in detail of the different series of plates we have mentioned above.

FIRST SERIES.—*Plates taken round the axis* A Y *and perpendicular to the face* A X B Z *of the cube.*

In the plates of this series, one of the modes of division remains constantly the same. (See figs. 5, 7 and 8.) It consists of two lines crossed rectangularly, one of which, *a y*, places itself constantly on the axis A Y of mean elasticity; but although this system always presents the same appearance, it is not accompanied, for the different inclinations of the plates, by the same number of vibrations; this ought to be the case, since the influence of the axis of greatest elasticity ought to be more sensible as the plates more nearly approach containing it in their plane: the sound of this system ought therefore to ascend in proportion as the plates become more nearly parallel to the plane C Y A X. As to the hyperbolic system, it undergoes remarkable transformations, which depend on this circumstance, that the line *a y* remaining the axis of mean elasticity in all the plates, the line *c d*, which is the axis of least elasticity in No. 1, transforms itself gradually into that of the greatest elasticity, which is contained in the plane of the plate No. 6. It hence follows that there ought to be a certain degree of inclination for which the elasticities, according to the two directions *a y*, *c d*, ought to be equal: now, this actually happens with respect to the plate No. 3; and this equality may be proved by cutting in this plate, in the direction of *a y* and its perpendicular, two small rods of the same dimensions: it will be seen, on causing them to vibrate in the same mode of transversal motion, that they produce the same sound. It also follows, because the elasticity in the direction *a y* is sometimes smaller and sometimes greater than that which exists in the direction of *c d*, that the first axis of the nodal hyperbola ought to change its position to be able to remain always perpendicular to that of the lines *a y*, *c d*, which possess the greatest elasticity; thus, in Nos. 1 and 2, *c d* possessing the least elasticity, it becomes the transverse axis of the hyperbola, whilst in Nos. 4, 5 and 6, the elasticity being greater in the direction *c d* than in that of *a y*, the transverse axis of the hyperbola places itself on the latter line. As the ratio of the two elasticities varies only gradually, it is obvious that the modifications impressed on the hyperbolic system ought in the same manner to be gradual: thus the summits of these curves, at first separated in No. 1 by a certain distance (which will depend on the nature of the wood), will approach nearer and nearer, for the following plates, until they coalesce as in No. 3, at a certain degree of inclination, which was 45° in the experiment to which I now refer, but which might be a different number of degrees for another kind of wood. At the point where we have seen that the elasticities are equal in the direction of the axis, the two curves transform themselves into two straight lines which intersect each other rectangularly, after which they again separate; but their separation is effected in a direction perpendicular to that of their coalescence. The sounds of the hyperbolic system follow nearly the same course as those of the system of crossed lines, that is to say, they become higher in proportion as the plates more nearly approach being parallel to the axis of greatest elasticity; but it deserves to be remarked, that the plate No. 3, for which the elasticity is the same in the two directions *a y*, *c d*, is that between the two sounds of which there is the greatest interval: this evidently depends on the elasticity in the two directions *a y*, *c d* being very different from that which exists in the other directions of the plate.

Lastly, it is to be remarked that, in the four first plates, the sound of the hyperbolic system is sharper than that of the system of crossed lines, and that it is the contrary for the plate No. 6, which renders it necessary that there should be between No. 4 and No. 6 a plate, the sounds of which are equal, which in the present case is exemplified in No. 5, although its two modes of division differ greatly from each other. There is another thing remarkable in this plate; its two modes of division can transform themselves gradually into each other by changing the position of the place of excitation, so that the two points *c* and *c′* becoming two nodal centres, are in every respect in the conditions indicated by fig. 4.

The interval included between the gravest and the sharpest sounds of this series was an augmented sixth.

It is almost useless to observe that the plates taken in the directions I, II, III, inclined on the other side of the axis A X the same number of degrees as the plates 1, 2, 3, would present exactly the same phænomena as these latter. This observation being equally applicable to the following series, we shall not mention it again.

SECOND SERIES.—*Plates taken round the axis* A Z *of least elasticity and perpendicular to the plane* C Y A X; figs. 9 and 10.

As in the preceding case, one of the nodal systems of the plates of this series consists of two lines crossed rectangularly, one of which, *a z*, corresponds with the axis A Z; whence it follows that the second may be considered as the projection of the two other axes on the plane of the plate, which, whatever its inclination may be, ought consequently to possess a greater elasticity in the direction *f g* than in the direction *a z*: thus the hyperbolic system of this series cannot present the transformations which we saw in the preceding series, where *cd*, fig. 8, possesses sometimes a less, at other times a greater elasticity than that of *a y*. In the present case, *a z* remaining constantly the axis of least elasticity, the resistance to flexion in the direction *f g* goes on gradually increasing from the plate No. 1 to the plate No. 6 parallel to the plane A X B Z, and the branches of the hyperbola straighten themselves in proportion as the plates more nearly approach this last position. As to the sounds which correspond to each of these nodal systems, it is observed that they ascend gradually from No. 1 to No. 6, and that the sound of the hyperbolic system is sharper in a part of the series than that of the system of crossed lines, whilst they become graver in the other part. There is therefore a certain inclination for which the sounds of the two systems ought to be equal; and this evidently would have taken place in the present experiment for a plate intermediate to No. 4 and No. 5.

The interval between the gravest and the sharpest sound of each series was an augmented fifth.

THIRD SERIES.—*Plates taken round the axis* A X *of greatest elasticity, and perpendicular to the plane* A Y D Z; figs. 11 and 12.

The elastic state of these plates cannot present such remarkable differences as those we have observed in the preceding series; for, being all cut round the axis of greatest elasticity, they can only contain in their plane that of least or that of mean elasticity, or lastly, those intermediate between these limits, which do not vary greatly from each other. Thus it is seen that their modes of division differ very little from each other, and that the sounds which correspond to them present rather slight differences, although they go on ascending in proportion as the plates more nearly approach containing the axis of mean elasticity in their plane. Here, as in the other series, one of the nodal systems consists of two lines crossed rectangularly, one of which, *a x*, places itself always on the axis of greatest elasticity, and this line serves as the second axis to the hyperbolic curves which compose the nodal system. Doubtlessly these curves are not entirely similar in the different plates; but I have not been able to perceive any very remarkable difference between them, unless that it appears that their summits gradually approach by a very small quantity, in proportion as the plates more nearly approach containing the intermediate axis in their plane.

FOURTH SERIES.—*Plates cut round the diagonal* A D, *and perpendicular to the plane* B C Y Z; figs. 13 and 14.

These plates present much more complicated phænomena than those we have hitherto observed. Except for the first and the last, neither of the two nodal systems consists of lines crossed rectangularly, which shows that this kind of acoustic figure can only occur on plates which contain at least one of the axes of elasticity in their plane, since Nos. 2, 3, 4, 5, which are inclined to the three axes, present only hyperbolic lines, whilst No. 1, which contains two of the axes of elasticity, and No. 6, which contains only one, are susceptible of assuming this kind of division.

In this series, neither of the modes of division remains constantly the same for the different degrees of inclination of the plates: setting out from the plate No. 1, one of the systems gradually passes from two crossed lines to two hyperbolic branches, which are nearly transformed into parallel straight lines in No. 6; on the contrary, the other system appears in No. 1 under the form of two hyperbolic curves, the summits of which approach nearer and nearer until they coalesce in No. 6, where they assume the form of two straight lines which cut each other at right angles and this contrary course in the modifications of the two systems is such, that there is a certain inclination (No. 3) for which the two modes of division are the same, although the sounds which correspond to them are very different.

As in the preceding series, and for the same reasons, the sound of each nodal system goes on always ascending in proportion as the plate more nearly approaches containing the axis of greatest elasticity in its plane.

FIFTH SERIES.—*Plates cut round the diagonal* A E, *and perpendicular to the plane r s t;* figs. 5.

Among all the plates which may be cut round the diagonal A E of the cube fig. 5, there are three each of which contains one of the axes of elasticity, and which consequently we have already had occasion to observe; thus the plate No. 3, fig. 8, which passes through the diagonal A B, and through the edge A Y, contains the diagonal A E in its plane; also, the plate No. 4, fig. 10, which passes through one of the diagonals X Y or A C, and which is perpendicular to the plane C Y A X, contains also A E in its plane; and lastly, the plate No. 3 of fig. 12, parallel to the plane A D E X, is circumstanced in the same manner. Thus, if *r s t*, fig. 15, is a plane perpendicular to the diagonal A E, and if the lines 1, 3, 5 indicate the directions of the three plates we have just spoken of, in order to become acquainted with the progress of the transformations which connect the modes of division of these plates together, it will be sufficient to take round A E, the projection of which is in *c*, a few other plates such as 2, 4, 6. The Nos. 1, 2, 3 of fig. 16 represent this series thus completed, and the dotted line *a e* indicates in all the direction of the diagonal of the cube.

The nodal system represented by the unbroken lines consists, for No. 1, of two crossed nodal lines, one of which, *a y*, places itself upon the axis A Y, and the other in a perpendicular direction; it transforms itself in No. 2 into hyperbolic curves, which by the approximation of their summits again become straight lines in No. 3, which contains the axis A Y of greatest elasticity: these curves afterwards recede again, No. 4, and in the same direction as No. 2; they then change a third time into straight lines in No. 5, which contains the axis A Z of least elasticity; and lastly, they reassume the appearance of two hyperbolic branches in No. 6.

The transformations of the dotted system are much less complicated, since it appears as two straight lines crossed rectangularly in No. 1, and afterwards only changes into two hyperbolic branches, which continue to become straighter until a certain limit, which appears to be at No. 3, and the summits of which afterwards approach each other, Nos. 5 and 6, in order to coalesce again in No. 1.

As to the general course observed by the sounds of the two nodal systems, it is very simple, and it was easy to determine it previously. Thus, the plate No. 5, containing in its plane the axis A Z of least elasticity, the two gravest sounds of the entire series is heard; these sounds afterwards gradually rise until No. 3, which contains the axis A X of greatest elasticity; after which they redescend by degrees in Nos. 2 and 1, (the latter contains the axis A Y of intermediate elasticity in its plane,) and they return at last to their point of departure in the plates Nos. 6 and 5.

The transformations of the nodal lines of this series, by establishing a link between the three series of plates cut round the axes, makes us conceive the possibility of arriving at the determination of nodal surfaces, which we might suppose to exist within bodies having three rectangular axes of elasticity, and the knowledge of which might enable us to determine, *à priori*, the modes of division of a circular plate inclined in any manner with respect to these axes. But it is obvious, that to attempt such an investigation it would be necessary to base it on experiments made with a substance the three axes of which shall be accurately perpendicular to each other, which is not entirely the case in wood.

It would now remain for us to examine two other series of plates, one taken round the diagonal A B, and the other round the diagonal A C; but as it is evident that the arrangements of nodal lines which they would present would differ very little from those of the fourth series, we may dispense with their examination.

Such are, in general, the phænomena which are observed in bodies which, like that we have just examined, possess three axes of elasticity: collected into a few propositions, the results we have obtained are reducible to the following general data.

1st. When one of the axes of elasticity occurs in the plane of the plate, one of the nodal figures always consists of two straight lines, which intersect each other at right angles, and one of which invariably places itself in the exact direction of this axis; the other figure is then formed by two curves which resemble the branches of a hyperbola.

2nd. When the plate contains neither of the axes in its plane, the two nodal figures are constantly hyperbolic curves; straight lines never enter into their composition.

3rd. The numbers of vibrations which accompany each mode of division are, in general, higher as the inclination of the plane to the axis of greatest elasticity becomes less.

4th. The plate which gives the sharpest sound, or which is susceptible of producing the greatest number of vibrations, is that which contains in its plane the axis of greatest elasticity and that of mean elasticity.

5th. The plate which is perpendicular to the axis of greatest elasticity is that from which the gravest sound is obtained, or which is susceptible of producing the least number of vibrations.

6th. When one of the axes is in the plane of the plate, and the elasticity in the direction perpendicular to this axis is equal to that which itself possesses, the two nodal systems are similar; they each consist of two straight lines which intersect each other rectangularly, and they occupy positions 45° from each other. In a body which possesses three unequal axes of elasticity there are only two planes which possess this property.

7th. The transverse axis of the nodal curves always occurs in the direction of the least resistance to flexion; it hence follows, that when in a series of plates this axis places itself in the direction at first occupied by the conjugate axis, it is because the elasticity in this direction has become relatively less than in the other.

8th. In a body which possesses three unequal axes of elasticity, there are four planes in which the elasticity is so distributed that the two sounds of the plates parallel to these planes become equal, and the two modes of division gradually transform themselves into each other, by turning round two fixed points, which, for this reason, I have called *nodal centres*.

9th. The numbers of vibrations are only indirectly connected with the modes of division, since two similar nodal figures, as in No. 3, fig. 8, and in No. 3, fig. 14, are accompanied by very different sounds; whilst, on the other side, the same sounds are produced on the occurrence of very different figures, as is the case for No. 5 of fig. 8.

10th. Lastly, a more general consequence which may be deduced from the different facts we have just examined is, that when a circular plate does not possess the same properties in every direction, or, in other words, when the parts of which it consists are not symmetrically arranged round its centre, the modes of division of which it is susceptible assume positions determined by the peculiar structure of the body; and that each mode of division, considered separately, may always, subject however to alternations more or less considerable, establish themselves in two positions equally determined, so that it may be said that, in heterogeneous circular plates, all the modes of division are double.

By the aid of these data, which are no doubt still very few and imperfect, a notion may be formed, to a certain point, of the elastic state of crystallized bodies, by submitting them to the same mode of investigation: this is what we have attempted for rock crystal, in a series of experiments which will be the subject of § III. of this Memoir.

ARTICLE VIII.

Experiments on the Essential Oil of the Spiræa Ulmaria, *or Meadow-Sweet; by* DR. LÖWIG, *Professor of Chemistry at Zurich*[*].

From J. C. Poggendorff's *Annalen der Physik und Chemie*; Berlin,
Second Series, vol. v. p. 596.

WHILST by the examination of plants and vegetable matters we have acquired the knowledge of a great number of oxyacids with compound radicals, with the exception of prussic acid no such hydracid has been shown to exist in organic nature; and of hydracids with ternary radicals, if we except the sulphocyanic acid, we have not the slightest knowledge.

The constancy of the phænomena which oil of bitter almonds presents, have not only led to the positive knowledge of the existence of ternary radicals, but also to the fact that oxyacids exist with ternary bases containing oxygen.

By means of the experiments made with the essential oil of the blossoms of the *Spiræa Ulmaria,* described in the following treatise, the first hydracid with a ternary radical in organic nature has been discovered in a most extraordinary manner, and they give every reason to hope that the radical of the same may also be isolated.

The reader must excuse the circumstances that the experiments are not carried further, and that some of the most important appearances when remarked have not been further pursued, as for all the experiments, there was only a very small quantity of material at the disposal of the experimenter.

If also, on account of the small quantity of the oil which could be subjected to research, each experiment was conducted only on a small scale, especially as regards the analysis, and could only be seldom repeated, the coincidence of each separate experiment may perhaps in part supply the want of repetition. Should however the analytical results experience any small change by later repeated experiments, we may nevertheless be sure that the facts themselves of which this Memoir treats will lose nothing in importance.

By means of this communication the attention of chemists may be drawn to the oil of the *Spiræa,* so that not only a repetition of these experiments, but also a further extension of them may be expected with certainty from other chemists.

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[*] [The Editor is indebted for the translation of this Paper to E.Solly, jun., Esq.]

M. Pagenstuher, an apothecary in Berne, had already drawn attention to the oil and distilled water of the blossoms of *Spiræa Ulmaria* in a treatise which may be found in Buchner's Repertorium, vol. XLIX. p. 337. He there describes exactly almost all the combinations which are the subject of this Memoir; and had he made analyses of the elements with only a few substances, the real nature, not only of the oil itself, but also several decompositions and recompositions which it experiences would not then have escaped him.

M. Pagenstuher had the kindness to give me the oil for all the investigations, and also to communicate to me his observations made up to that time. All his experiments which were of any importance on the present subject are incorporated into this Memoir, so that in many respects the present work may be considered as undertaken conjointly with M. Pagenstuher. To facilitate the general view of the following experiments, a few of the leading results may be first stated.

The oil of the blossoms of the *Spiræa Ulmaria* is a hydracid; it consists of *one* eq. of a radical $= C\,12\ H\,5\ O\,4$, and *one* eq. of hydrogen, which uniting with the radical forms an acid. If the hydrogen, which by uniting with the radical forms the acid, be oxidized by nitric acid, 4 additional eqs. of oxygen are taken up by the radical, thus forming the oxyacid of the same radical. Instead of 1 eq. of hydrogen, the radical can unite with 1 eq. of chlorine, bromine, iodine, or even of a metal. These latter combinations are also formed when the hydracid is made to act on metallic oxides. With ammonia, on the contrary, the hydracid unites without undergoing any change. From these combinations the following compounds result:

$C\,12\ H\,5\ O\,4 + H$
$C\,12\ H\,5\ O\,4 + Cl$
$C\,12\ H\,5\ O\,4 + Br$
$C\,12\ H\,5\ O\,4 + I$
$C\,12\ H\,5\ O\,4 + P$
$C\,12\ H\,5\ O\,4 + O4$
$(C\,12\ H\,5\ O\,4 + H) + N\,H\,3$ or ammonia.

The radical is designated by the name Spiræoyl, or for the convenience of shortness Spiroil. Another name would have been chosen for it had not a similar nomenclature been already applied to another substance nearly allied to it. It is always doubtful policy to derive the name of a vegetable principle from the plant in which it is first discovered, for generally with great probability the same body may be found in other plants. Names which designate any principal peculiarity of the substance are therefore in such cases always preferable. One of the peculiarities of spiroil is its property of forming yellow compounds with oxygen, and with the metals, alkalies, and earths; a name therefore which had reference to this quality would have been very suitable. A name however, is only a sign for a certain expression, and thus considered it is perfectly indifferent what name may be chosen for any substance.

Hydrospiroïlic Acid.

The fluid oil of the blossoms of the *Spiræa Ulmaria* is hydrospiroilic acid. This may be obtained by distilling the flowers with water; about as much water is to be distilled off as was originally employed.

The product of the distillation is however subjected to a redistillation till about $\frac{1}{5}$th is come over in the receiver. A concentrated aqueous solution of the oil is thus obtained, and the oil itself, though only in very minute quantities. The oil is heavier than water, is of a light yellow colour, and possesses the odour of the blossoms in a very great degree. It mixes in all proportions with alcohol and æther, and is slightly soluble in water. It causes a burning sensation on the tongue. The fumes which come over during the distillation of the oil first render litmus-paper green, and then bleach it. The aqueous solution of the oil first of all slightly reddens tincture of litmus, and then deprives it of its colour excepting a greenish shade. It is inflammable, and burns with a shining smoky flame. If the oil be passed through a red-hot tube containing pieces of iron, neither ammonia nor prussic acid is obtained nor can the formation of any sulphuret of iron be detected. The oil does not experience any change either in dry or moist oxygen gas; it volatilizes unchanged. It solidifies at a temperature of $-20°$*. Its boiling-point is about $+85°$*, when it evaporates entirely without leaving any residue.

With the bases of salts, namely, with the alkalies and alkaline earths, it easily combines, forming insoluble or difficultly soluble compounds.

Concentrated sulphuric acid converts the oil into a black carbonaceous mass. Chlorine and bromine decompose it instantaneously, hydrochloric or hydrobromic acid and chloride or bromide of spiroil being formed. Nitric acid, if not too concentrated, immediately forms spiroilic acid; if however the acid be very concentrated and fuming, it immediately changes it into a yellow, very volatile, bitter-tasting compound, having the appearance of butter.

The experiments on the composition of the anhydrous oil, as well as the other compounds, were made in the usual manner with oxide of copper.

0·290 grms. of the oil gave 0·694 carb. acid 191·89 carbon
0·290 ——————— 0·145 water 16·10 hydrogen;

according to which 290 parts of the oil contain

Carbon...............	191·89	or in 100 parts	66·17
Hydrogen	16·10	———	5·55
Oxygen	82·01	———	28·28
	290·00		100·00

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* Probably Centigrade.—Translator.

from whence we may deduce the following eq.

				in 100 parts
12 eqs. Carbon	=	73·56	=	66·92
6 — Hydrogen	=	6·00	=	8·35
4 — Oxygen	=	32·00	=	27·73
1 Hydrospiroilic acid		111·56		100·00

The combination of the oil with copper was also subjected to analysis. This compound was obtained by agitating together an aqueous solution of the oil with freshly prepared quite pure hydrated oxide of copper, taking care that the oil should be in excess; and the green compound thus obtained was dried at + 160°. At this temperature the combination is not decomposed, which is evident, as the oil may again be obtained unaltered on the addition of an acid. By other means, by double decomposition for instance, the combination with copper may be obtained, but not quite pure, as it then contains slight traces of the acid which was united with the copper, even if excess of alkali be employed as a precipitant.

0·174 grm of the compound with copper yielded 0·324 c. a. 89·58 carbon
0·174 ———————————————— 0·054 water 5·99 hydr.

Also by burning the cupreous combination in contact with the air, 0·130 of the compound yielded

0·03719 oxide of copper 29·68 copper.

If now in the cupreous combination the copper is considered to have been in the metallic state we obtain

Carbon	=	89·58	or	51·48
Hydrogen	=	5·99	—	3·44
Oxygen	=	38·71	—	22·20
Copper	=	39·72	—	22·88
		174·00		100·00

which in eq.

12 eqs. Carbon	73·56	or	51·71
5 — Hydrogen	5·00	—	3·51
4 — Oxygen	32·00	—	22·51
1 — Copper	31·70	—	22·27
1 eq. Spiroilide of copper......	142·26		100·00

These experiments confirm the truth of the above-mentioned view, that the oil is a hydracid with a ternary base, and they likewise show that the action of this hydracid with metals is exactly the same as that of those which were before known.

This view receives still further confirmation by the fact that when chlorine is passed over one of the metallic combinations, as spiroilide of copper or spiroilide of silver, chloride of copper or silver, and chloride of spiroil are formed without the slightest trace of muriatic acid.

The most striking proof, however, that the oil is really a hydracid is, that when potassium is brought into contact with the oil over mercury and gently warmed, hydrogen is evolved, spiroilide of potassium being formed, from which latter the oil may again be obtained, possessed of all its original properties, by the action of muriatic acid.

The action of the oil and potassium, which at common temperatures goes on but slowly, is by a very gentle heat so much increased, that during the evolution of the hydrogen the combination of the spiroil and potassium is attended with the evolution of heat and light. At the same time not the slightest trace of carbon or of any carbonaceous matter is deposited, and the hydrogen which is evolved is quite pure. If the oil which is employed for these experiments be not quite free from water, evolution of hydrogen takes place as soon as it comes into contact with the potassium; this however ceases (almost entirely) in a few moments: if however the apparatus be now gently warmed, which may be done by gradually bringing near a glowing coal, the evolution of gas begins again in great quantity and with the same violence as when the anhydrous oil was employed.

Hydrospiroilate of Ammonia.

If a concentrated solution of ammonia is poured upon pure hydrospiroilic acid, the fluid mixture after a few seconds is converted into a solid mass of hydrospiroilate of ammonia, giving out heat and undergoing a considerable increase of bulk during the action: it may be freed from water and excess of acid by washing with alcohol. It possesses a weak aromatic smell resembling a rose, is tasteless, and has a yellow colour. The compound is almost insoluble in water, which nevertheless when left for some time in contact with it, acquires a yellow colour. In common cold spirit of wine the hydrospiroilate of ammonia is only slightly soluble, but on the contrary it is dissolved in great quantities both in hot and cold pure alcohol. If the boiling solution be allowed to cool, hydrospiroilate of ammonia is obtained in transparent delicate tufts of acicular crystals of a light yellow colour. If it be preserved in close vessels in a moist state it is decomposed; after a short time it becomes gradually black, then semifluid, ammonia is evolved, and an exceedingly strong penetrating odour of oil of roses is perceptible.

At the boiling-point of water, hydrospiroilate of ammonia undergoes no change.

At + 115° it is fluid, melting like wax; heated a few degrees above its boiling-point it volatilizes in the form of a yellow vapour without leaving any residue and without undergoing any alteration. If solution of potash or soda be poured over this salt, the ammoniacal odour is not immediately developed, but becomes so after continued contact or the application of heat. This circumstance might lead to the supposition, that in this compound there is a similar relation between the acid and the alkali as in the cyanate of ammonia.

At the same time it must be observed, that acids immediately decompose this compound, the oil being deposited, undecomposed, and a corresponding salt of ammonia formed. 0·213grm. of hydrospiroilate of ammonia obtained in crystals by evaporating the alcoholic solution was decomposed by dilute muriatic acid; the solution thus obtained was evaporated to dryness in a water-bath, and the remaining neutral saline mass again dissolved in water. By precipitation with nitrate of silver, 0·239grm. of chloride of silver were obtained; as these correspond to 0·0288 of ammonia, the above 0·213grm. consist of

Ammonia		0·0288,	or in 100 parts	13·52
Hydrospiroilic acid		0·1850	————	86·48
		0·2138		100·00
1 equivalent ammonia	=	17·18	or	13·38
1 hydrospiroilic acid	=	111·56	—	86·62
1 eq. hydrospiroilate of ammonia		128·74		100·00

Spiroilide of Potassium.

The spiroilide of potassium may be obtained either by gently heating together potassium and hydrospiroilic acid, hydrogen being evolved, or by bringing together either the pure or the watery hydrospiroilic acid and solution of potash. Spiroilide of potassium is difficulty soluble in water.

If the aqueous solution be slowly evaporated, small prismatic straw-coloured crystals are obtained. Left in contact with the air it soon decomposes, absorbing moisture and carbonic acid, like the hydrospiroilate of ammonia. It may nevertheless be kept unaltered for a long time in close vessels. The smell resembling that of roses is likewise perceptible during the decomposition of this substance; at the end, carbonate of potash remains.

1 eq. potassium	39·20	26·87	24·93
1 eq. spiroil	110·56	73·13	75·07
1 eq. spiroilide potassium	149·76	100·00	100·00

Spiroilides of Sodium, Calcium and Barium. These possess similar properties to the spiroilide of potassium, but the two latter compounds are still less soluble in water.

Spiroilide of Magnesium may be obtained by agitating together the watery hydrospiroilic acid and hydrate of magnesia. It appears as a light yellow and almost insoluble powder.

Protospiroilide of Iron. The aqueous solution of hydrospiroilic acid has no action on protochloride of iron; on the addition of ammonia however a deep violet blue precipitate falls.

Sesquispiroilide of Iron. Sesquichloride of iron immediately changes the colour of the aqueous solution of hydrospiroilic acid to a fine deep cherry-red colour, without any precipitate.

If this fluid be exposed to the air it loses its red colour in a short time, and a pure solution of sesquichloride of iron remains, in which a fresh addition of hydrospiroilic acid again causes the cherry-red colour to appear.

Subspiroilide of Copper. Hydrospiroilic acid has no action on the subchloride of copper; a slight addition of ammonia however causes a light brown precipitate in this mixture.

Protospiroilide of Copper. This compound is best obtained by agitating together an aqueous solution of hydrospiroilic acid and newly prepared hydrated oxide of copper. This latter immediately loses its blue colour and becomes green.

If solutions of sulphate of copper and spiroilide of potassium be mixed, a voluminous precipitate falls, which however is but slowly deposited and possesses a distinct crystalline texture.

Spiroilide of Zinc. When oxide of zinc is agitated with the aqueous solution of hydrospiroilic acid, it very soon absorbs the acid from the water, which latter acquires a yellow colour. By evaporation under the airpump, a yellow pulverulent substance is obtained. The aqueous solution of spiroilide of zinc is coloured cherry-red by sesquichloride of iron.

Spiroilide of Lead. When pure oxide of lead is brought into contact with hydrospiroilic acid no spiroilide of lead is formed. Newly prepared hydrated oxide of lead however, when left for some time in contact with the aqueous acid, becomes converted into a light yellow powder, consisting of small shining laminæ of spiroilide of lead.

Spiroilide of Mercury. The aqueous solution of the acid has no action on red oxide of mercury when they are left together in close vessels, even though frequently agitated.

Spiroilide of mercury is however obtained when a concentrated solution of corrosive sublimate is poured over hydrospiroilate of ammonia. A pale straw-coloured flocky voluminous precipitate is formed.

Spiroilide of Silver. Oxide of silver is partially dissolved by the aqueous solution of hydrospiroilic acid.

The solution is of a yellow colour, and has a bitter metallic taste. By evaporation in vacuo a brownish black residue is obtained, which inflames with detonation in the candle, leaving behind metallic silver.

The undissolved portion of the oxide, which has also acquired a brownish black colour, possesses the same property.

The greater number of the compounds of the metals with spiroil may be obtained by double affinity, but for this purpose the spiroilic combinations must be difficultly soluble or insoluble, and very concentrated solutions of easily soluble salts must be employed. For this reason, in order to obtain the combination with lime, solution of chloride of calcium is employed; for the combination with zinc, acetate of zinc; for the magnesian compound, chloride of magnesium; and for the compounds with iron, the proto- and the sesqui-chloride of iron. The best combination of spiroil to employ is the hydrospiroilate of ammonia, over which the concentrated solution of the salt is to be poured. The spiroilide of barium, which is best obtained by saturating baryta water with the acid, may be advantageously employed in the state of a solution for the preparation of several of the compounds of spiroil. The compounds as obtained by double decomposition are seldom crystalline, but are obtained almost always as a fine and soft powder.

Spiroilic Acid.

If hydrospiroilic acid be gently and carefully heated with nitric acid not too concentrated, and care be taken that the acid be not used in excess, the oil is converted under evolution of nitrous fumes into a solid crystalline body; the substance thus obtained is spiroilic acid.

If the gas which is evolved during this operation be conducted into a solution of chloride of barium mixed with ammonia, not the slightest trace of carbonate of barytes will be formed.

The acid is nearly devoid of odour: its taste is at first not striking; afterwards however much irritation in the throat, and a strong inclination to cough are experienced. Spiroilic acid is fusible, and shows strong inclination to crystallize, especially on returning to the solid state after having been heated. In close vessels it may be sublimed; nevertheless by this operation the greater part is decomposed, leaving behind a carbonaceous mass.

In the anhydrous state, as it is obtained by fusion, spiroilic acid is of a pale yellow colour; if however it be exposed to the air it deliquesces and becomes of a deep yellow colour. It is easily soluble in alcohol and æther; only slightly so however in water. The solutions stain the skin and nails permanently yellow. Litmus-paper is stained deep yellow; no reddening effect can however be observed. If the alcoholic solution of the spiroilic acid be left to spontaneous evaporation, the acid is obtained in delicate transparent prisms of a golden yellow colour.

1st, 0·190 fused spiroilic acid gave 0·350 carbon = 96·77 car.
0·190 ditto gave 0·060 water = 6·66 hydr.
2ndly, 0·243 ditto gave 0·450 carb = 124·42 carb. 0·0759 = 8·33 hyd.

From these proportions we obtain the following for 100 parts:

	I.	II.
Carbon	50.92	51·18
Hydrogen	3·50	3·43
Oxygen	45·58	45·39
	100·00	100·00

or reckoned in equivalents,—

12 eqs. carbon	73·56	51·58
5 — hydrogen	5·00	3·50
8 — oxygen	64·00	44·92
1 eq. spiroilic acid	142·56	100·00

From these experiments we may deduce, that in the moment that the one equivalent of hydrogen of the hydrospiroilic acid is oxidized, four more equivalents of oxygen are taken up by the radical. This likewise explains the formation of so large a portion of nitrous acid, even when a very small quantity only of the oil is employed.

It appeared probable that during the oxidation a portion of nitric or nitrous acid was absorbed by the radical, and this opinion was supported by the fact, that spiroilic acid stains the skin and nails permanently yellow,—a property which is likewise possessed by the spiroilide of potassium and several other metallic spiroilides; several experiments were undertaken to detect the nitrogen or the acid, but unsuccessfully with regard to either.

If spiroilic acid be slightly heated with potassium over mercury, a most violent evolution of heat and light suddenly takes place, by which the vessel is always broken with great violence. Even should excess of potassium be employed in this experiment, only a portion of the spiroilic acid is decomposed; a porous carbon is deposited, and a mixture of spiroilide of potassium and carbonate of potash is formed.

The pure alkalies unite very readily with spiroilic acid, forming yellow compounds, by evaporating the aqueous solutions of which small yellow crystals may be obtained.

If spiroilic acid be dissolved in æther, and the solution agitated with solutions of potash or soda, the æther is immediately abstracted from the acid. The alkaline salts of spiroilic acid are also soluble in alcohol.

If ammonia be saturated with spiroilic acid, a deep blood-red solution is obtained. If this be evaporated to dryness a yellow residue remains, which if rubbed with caustic alkali immediately develops a strong ammoniacal odour. If spiroilate of ammonia be subjected to a high temperature in close vessels it is decomposed, some ammonia is evolved, and an oily body comes over, the exact nature of which has not been determined on account of the minuteness of the quantity.

The aqueous solution of spiroilate of soda gives with acetate of lead a yellow, and with salts of copper a green, precipitate.

Sesquichromate of iron is not precipitated by spiroilate of soda, but is coloured deep cherry-red by it, as well as by the spiroilide.

If the salts of spiroilic acid are heated in contact with the air, they detonate briskly, leaving behind either a pure or carbonated base, and a soft powdery carbon.

If spiroilic acid be mixed with an easily inflammable substance, as sulphur, the mixture detonates when heated.

Fuming nitric acid acts very violently upon spiroilic acid; fumes of nitrous acid are immediately evolved, whilst a yellow semifluid mass is formed, which solidifies only after several days. This yellow substance has an intensely bitter taste, and colours the saliva, skin, and nails, &c. deep yellow; it is fusible and may be distilled, and possesses in a striking degree the smell of fresh butter; no oxalic acid is formed. Submitted to distillation with water, it distils over undecomposed with the water, partially dissolved and partly as a yellow powder.

If the residue of the aqueous solution after the yellow body has gone over be slowly evaporated, transparent colourless prismatic crystals are obtained, the nature of which requires further investigation.

An analysis of this substance, which appeared to possess acid properties, was several times commenced; but even by the most careful application of heat, the oxide of copper was always projected into the tube containing the chloride of calcium, whilst part of the substance was often conducted undecomposed into the potash apparatus. Nevertheless the determination of the carbonic acid has several times been accomplished: from the data thus obtained, this yellow substance must be very rich in oxygen.

Chloride of Spiroil.

Chloride of spiroil may be obtained by decomposing hydrospiroilic acid by means of chlorine. In a suitable apparatus and without the application of heat, dry chlorine gas was passed over the anhydrous acid; evolution of muriatic acid immediately commenced: if the chlorine be evolved but slowly, only a slight elevation of temperature takes place. Chlorine is to be passed through this solution so long as fumes of muriatic acid are evolved: the hydrospiroilic acid is entirely converted into a white crystalline mass.

If however the oil is become solid, the chlorine apparatus must be moved, and the chloride of spiroil which has been formed must be sublimed by the lowest possible heat.

The most beautiful crystalline plates of a dazzling whiteness are obtained, which melt at a very low heat, and, as has been before remarked, may easily be sublimed.

If chloride of spiroil be briskly heated, the melted mass gradually becomes darker, and a slight carbonaceous residue remains.

No other products besides muriatic acid and chloride of spiroil are formed.

Pure chloride of spiroil possesses a peculiar and somewhat aromatic odour, which nevertheless has much similarity to the smell of diluted prussic acid. Its boiling point does not appear to exceed that of water. It is inflammable, and burns with a greenish sooty flame.

It is quite insoluble in water.

When it is boiled with water it evaporates entirely; by this operation not the slightest trace of muriatic acid is formed. Neither dry nor moist air has any action on it. Chloride of spiroil is easily soluble in æther and alcohol.

The alcoholic solution gives with acetate of copper a greenish yellow precipitate; salts of lead are precipitated yellow. Baryta water immediately separates chloride of spiroil from the alcoholic solution, and uniting with it a yellow precipitate falls.

Chloride of spiroil forms yellow, neutral, difficultly soluble compounds with the alkalies. The salts of iron are also coloured blueish black by the same.

In the combinations of chloride of spiroil with the metallic oxides and the alkalies, it appears to combine unaltered, as it may again be obtained unaltered when these compounds are decomposed by an acid.

Nitrate of silver causes a scarcely perceptible milkiness in the filtered solutions of the alkaline compounds which have been decomposed by nitric acid. 0·780 grm. of chloride of spiroil treated in the above-mentioned way gave 0·09 grm. chloride of silver, therefore scarcely 0·02 grm. chlorine. This small amount of chlorine was no doubt due to the presence of muriatic acid, as from 0·628 grm. hydrospiroilic acid 0·795 grm. of chloride of spiroil was obtained; therefore at least 0·157 grm. chlorine must have been taken up.

If chloride of spiroil be melted with potassium by the application of a very gentle heat, violent evolution of heat and light suddenly takes place. A portion of the chloride of spiroil is decomposed thereby; carbon is deposited, whilst another portion unites with the potash which has been formed. If the remaining mass be dissolved in water, and the solution decomposed by nitric acid, pure chloride of spiroil is precipitated. If this same fluid be filtered, nitrate of silver gives a large precipitate of chloride of silver. If the neutral solution of the chlorospiroilide be slowly evaporated, yellow tasteless crystals are obtained which are insoluble in alcohol. If these crystals be heated in a platinum crucible, heat and light are evolved long before the crucible is red hot. The mass blackens, and by the continued application of heat is converted into pure chloride of potassium, in the aqueous solution of which not the slightest alkaline reaction is visible. All these experiments seem to confirm the above-mentioned view, that chloride of spiroil combines without decomposition with the bases of salts.

The question may here be asked, whether the acid properties of the chloride of spiroil arise from chlorine, and whether such a chloracid (in the same way as oxyacids) could combine with them?

Till now no such combinations were known, and therefore perhaps it might be simpler to state that when chloride of spiroil is brought together with a metallic oxide, 4 eqs. of a metallic chloride and 1 of a spiroilate are formed. If an acid be added to the solution of these salts, chloride of spiroil is again thrown down and a corresponding metallic salt formed. In the same manner one may imagine double combinations, consisting of a metallic chloride and a salt of a bromacid, from which on the addition of another acid chloride of bromine might be separated.

0·327 grm. of fused chloride of spiroil yielded

0·593 grm. carbonic acid = 162·94 carbon.
0·099 water = 10·98 hydrogen.

0·327 grm. chloride of spiroil dissolved in potash entirely free from chlorine, the solution evaporated, and the dry residue heated in a platinum crucible yielded, after the mass which had been heated was dissolved in water and saturated with nitric acid, 0·306 fused chloride of silver = ·754 chlorine.

Carbon	162·94	49·83
Hydrogen	10·98	3·35
Oxygen	77·68	23·77
Chlorine	75·40	23·05
	327·00	100·00

1 eq. of chloride of spiroil therefore consists of

12 eqs. carbon	73·56	50·38
5 — hydrogen	5·00	3·42
4 — oxygen	32·00	19·36
1 — chlorine	35·47	26·84
	146·03	100·00

It has been stated that 0·628 grm. hydrospiroilic acid contained 0·795 chloride of spiroil. According to the established eq. 111·56 hydrospiroil should yield 146·03 chloride of spiroil:

11·56 : 146·03 = 0·628 : 0·790.

Bromide of Spiroil.

Bromide of spiroil is easiest obtained by pouring bromine upon hydrospiroilic acid in a deep glass: hydrobromic acid is immediately evolved, the mixture becomes perceptibly warm, and at last solidifies into a greyish white crystalline mass. Bromide of spiroil may also be easily obtained by agitating the aqueous solution of hydrospiroilic acid with solution of bromine; it is immediately precipitated in white flocks: the supernatant liquid is colourless, has no smell, and contains hydrobromic acid. In order to free the bromide of spiroil from excess of bromine and hydrospiroil, it must be kept melted in a water-bath so long as acid fumes are given off. Bromide of spiroil is exactly similar to the chloride of spiroil in all its properties; it is quite insoluble in water, and is easily soluble in æther and alcohol. By the spontaneous evaporation of the alcoholic solution it is obtained crystallized; bromide of spiroil melts at a rather higher temperature than the chloride, and like the latter may also be entirely sublimed: when boiled with water it evaporates unaltered. Its behaviour, with regard to the saline bases, is exactly the same as chloride of spiroil, but the alkaline salts are more difficultly soluble: 0·480 grm. bromide of spiroil dissolved in potash and decomposed by nitric acid yielded only 0·02 bromide of silver.

I. 0·510 grm. fused bromide of spiroil yielded

0·690 carbonic acid = 190·78 carbon.
0·510 water = 13·11 hydrogen.

Further, by solution as above-mentioned in pure caustic potash, and combustion of the compound formed, 0·510 grm. bromide of spiroil became 0·485 bromide of silver = to about 0·2036 bromine.

Carbon	190·78	37·41
Hydrogen	13·11	2·57
Oxygen	192·41	20·02
Bromine	203·60	40·00
	510,00?	100·00

II. 0·325 grm. bromide of spiroil yielded

0·457 grm. carbonic acid = 126·00 carbon.
0·081 ...... water = 8·99 hydrogen.

III. 0·305 grm. bromide of spiroil yielded

0·409 grm. carbonic acid = 113·00 carbon.
0·071 ...... water = 7·88 hydrogen.

If the proportions of bromine in I. be taken, we obtain in 100 parts

	I.	
Carbon	38·77	37·05
Hydrogen	2·72	2·55
Oxygen	18·51	20·40
Bromine	40·00	40·00
	100·00	100·00

Reckoned in equivalents:

12 eqs. carbon	73·56	38·80
5 — hydrogen	5·00	2·62
4 — oxygen	32·00	17·10
1 — bromine	78·39	41·48
1 bromide of spiroil =	188·95	100·00

Iodide of Spiroil.

Hydrospiroilic acid dissolves iodine in great quantities, and forms with it a brownish black fluid; the formation of hydriodic acid is however not perceptible. Iodide of spiroil may be obtained by distilling chloride or bromide of spiroil with iodide of potassium; even by rubbing these substances together decomposition commences, and by the application of heat iodide of spiroil sublimes: it is solid, of a dark brown colour, easily fusible, and shows in general the same properties with regard to solubility in water, æther and alcohol, and its relations to saline bases, as chloride and bromide of spiroil.

ARTICLE IX.

Researches relative to the Insects, known to the Ancients and Moderns, by which the Vine is infested, and on the means of preventing their Ravages. By M. LE BARON WALCKENAER.

From the *Annales de la Société Entomologique de France,* vol. iv. p. 687, *et seq.*

Introduction.

General Considerations.—Division of these Researches into three Sections.

WHEN the human intellect began in Europe to emerge from the darkness and ignorance in which it had for many centuries been buried, its progress was everywhere the same, and the same method was adopted for the advancement of knowledge in all the sciences.

Before the invention of printing the ancients were the only sources of instruction; after the discovery of that art their works became more extensively circulated and better known, and as the necessary consequence of the abundance and the perfection of their labours, the admiration which they had excited was augmented, and increased effect was given to the ascendency they had acquired over the human mind. The only ambition of the learned was to understand, to arrange, and to comment upon the notions which they had transmitted to us. A treatise upon any branch whatever of human knowledge was merely a compilation, more or less complete and methodical, of what the ancients had written upon the subject: an addition was sometimes made of what the moderns had thought or observed on the same topics, but these supplements had not the same weight and authority as the rest of the work in the estimation of either the author or the reader. A remark or a proposition was judged of little value to which could not be added *ut ait Aristoteles, ut ait Plinius, ut ait Hippocrates,* or other similar phrases.

Happily for the progress of natural history, the great number of new productions brought into Europe from the countries recently discovered, at the end of the fifteenth and the commencement of the sixteenth centuries, soon rendered apparent the insufficiency of the works of the ancients with respect to this science.

It was perceived that the greater number of objects for the observation and description of which opportunity was afforded were unknown to them, and that they had very superficially observed and very imperfectly described those with which they were acquainted. This conviction was quickly impressed upon the mind with regard to the smaller species of animals, because upon this point their ignorance was the greatest, and the application of the notions which they had acquired to the knowledge of the moderns the most difficult and perplexing.

With regard to insects in particular, it was easy to see that the ancients had treated of only a very small number, and that with great inaccuracy; their works on this class of animals consequently ceased to occupy attention, which was exclusively devoted to the study of nature, and the science soon advanced rapidly.

However, the names that the ancients had imposed upon some classes of insects easily recognised remained, having passed from the ancient languages into the vernacular tongues. The more obscure names, the signification of which was doubtful or unknown, were employed by the modern naturalists for the numerous genera which the progress of science rendered it necessary to establish. Naturalists did not resolve to invent new names until all those employed by the ancients in the classes which they were studying were exhausted; and even then all, excepting M. Adanson, composed the new names from Greek or Latin roots. But even when naturalists gave names employed by the ancients to the genera of insects which they had formed, it was generally without any idea of applying them to the species which they had been employed by the ancients to designate, and without any attempt to aid in the recognition of those species. That a name had been used by some ancient author to designate an insect of some kind, or that there was no certain proof of the contrary, has been deemed a sufficient reason by modern entomologists for the application of an ancient name to a new genus. Our entomological systems contain names employed by the ancients, the signification of which is so entirely lost that it is matter of doubt whether they belonged to an animal or a plant.

It is necessary for the object that I have in view to illustrate this by an example, which is far from being the only one which I could produce.

M. Camus, the translator into French of Aristotle's Natural History of Animals*, remarks with reason in his notes, that commentators are divided with regard to the signification of the word *Staphylinus*, employed by that philosopher. Some have considered it as the name of an insect, others as that of a plant; but, says Camus, on the authority of the "*Dictionnaire d'Histoire Naturelle de Valmont de Bomare*," in which he found the word *Staphylinus*, "The Staphylinus is an insect well known to naturalists, because it has preserved its name as well in the Latin as in the French." From these words we learn that Camus

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* Camus, *Hist. Nat. des Animaux d'Aristote*, 4to, t. ii. p. 783.

was ignorant that the application of the word Staphylinus to a genus of insects of the class Coleoptera, now divided into a great number of genera bearing other names, is not more ancient than the time of Linnæus, who was the first to employ this word in its present signification, without attempting to determine that which it bears in Aristotle, whom he does not quote.

As to the superior orders of animals, such as quadrupeds, birds, fishes, and reptiles, naturalists have been careful to establish, whenever it was possible, the identity of the species which they have described with those mentioned by the ancients; and for this reason, that the latter nave recorded facts that have not since been so well observed, and some that have not been observed at all, and because still they all form part of the science; but this is not the case with insects. Notwithstanding the imperfection of the science of entomology, the most difficult branch of natural history, the moderns have made such progress in it that they have nothing to learn from the ancients upon this subject; if, therefore, we except the domestic bee, the caterpillar of *Bombix Mori*, or the silk-worm, two species of insects as important as the largest animals in the history of man, of commerce, and of the arts, we shall find that the moderns have occupied themselves very little with what the ancients have said upon insects: at the same time, the names that they have borrowed from them prove that they had read their works upon the subject, and that they would willingly have established, by the identity of the objects upon which they were employed, a direct relation between their labours and those of the naturalists who had preceded them in ancient times; but they appear to have considered this to be too difficult, or as impossible to be undertaken with success. This is the reason that the number of dissertations upon this subject is so small; and even of the few that we possess the object is only to discover to what class of insects the ancient name should be applied, not to determine the genus or the species.

If the science of natural history has little to hope from such investigations, they may yet be subservient to the acquisition of a better and more exact interpretation of the ancient texts; and the difficulties with which the subject is attended ought not to induce us to neglect it. With regard to this, as well as all the uncultivated parts of the vast field of erudition, we may say, if this had been easy it is probable that it would not have remained undone.

The above are the considerations which have induced me to write, and submit to the Academy the researches*, to which I was led by a question which one of my learned brethren did me the honour of ad-

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* These researches were read before the Academy of Inscriptions, of which the author is a member, before they were communicated to the Entomological Society.

dressing to me, respecting the interpretation of the name of an insect infesting the vine mentioned by Plautus. The text of this author in the passage alluded to is so explicit that I ventured immediately to give the solution required. To assure myself that I was not mistaken I commenced an examination of what ancient and modern authors had written on the species of insects infesting the vine, and the means of destroying them; but in explaining and arranging the ancient texts, and in afterwards applying them to the observations of the moderns, I found more difficulties than I expected; I however exerted all my efforts to surmount them. Such was the origin of this Memoir. The subject will doubtless appear minute, but as learning, natural history, and agriculture are equally interested in it, I think it cannot be considered either futile or unworthy of attention.

This Memoir will be divided into three sections. The first, which will be in some degree preparatory, will contain a critical examination of the ancient texts relative to the signification of the names of insects designated in them as being particularly injurious to the vine.

In the second section, by means of results obtained in the first, I shall determine which are the species of insects known to the ancients and moderns as being those injurious to the vine: I shall then indicate the means of preventing their ravages.

In the third section this dissertation will be terminated by a concordance of names, that is to say, a synonymy of all the species of insects mentioned in these researches, arranged in classes, which will render its application to use easy to those naturalists and agriculturists who may think proper to have recourse to it.

First Section.

Critical examination of the ancient texts with respect to the signification of the names of insects which are therein mentioned as being particularly injurious to the vine.

I. *Preliminaries.*—This section is, as I have already said, only preparatory to the principal object of the Memoir.

No application of modern names to interpret the ancient texts will here be made; but we shall content ourselves with investigating the signification of the ancient words, by means of the use to which the ancients themselves have applied them. In the second section the circumstances or the particulars of this use will enable us to interpret the ancient names, that is to say, to ascertain the names corresponding to them in the language of naturalists, the only names applicable to the definitions and descriptions proper to determine with precision the objects named. The vulgar names will be only a secondary consideration.

For objects, the differences of which escape superficial observation, the ancient as well as the modern languages furnish only general denominations, common to several objects or species, and are consequently extremely vague, a single word being employed to designate beings of very different natures. Scholars, grammarians, and lexicographers frequently add error to the confusion by their false distinctions, and sometimes the prodigious erudition of modern commentators increases the difficulty still more.

The true method of acquiring a complete and exact idea of the notions represented by each of the names we seek to explain appears to be this; to examine all the texts in which these names are employed, and afterwards to endeavour to remove the obscurity of the various meanings which have been assigned to them when they have been employed in different significations. By this method we can establish our opinions and conjectures upon the ancient texts with security, and without being exposed to the danger, to which we often yield without perceiving it, and sometimes even consciously, of selecting from the ancient authors only such passages as support our interpretations and systems, while we keep out of sight all those which are in opposition to them.

II. *List of the Names of Insects injurious to the Vine mentioned by the Ancients.*—The following are all the names of insects infesting the vine, or mentioned in connexion with it, that I have been able to discover in ancient authors:

1. Thola, Tholea or Tholaat.
2. Gaza.
3. Ips.
4. Iks.
5. Spondyle or Sphondyle.
6. Cantharis.
7. Phteïre or Phteïra.
8. Kampé.
9. Joulos or Julus.
10. Biurus or Bythurus.
11. Involvolus, Involvulus, Involvus.
12. Convolvulus.
13. Volvox.
14. Volucra.
15. Eruca.

III. *List of the Authors in whom the above Names are found, and whose texts are explained in these Researches.*—The following are the authors in whom the names above mentioned are found, and who will consequently receive some explanation in this dissertation:

The Bible.
Homer.
Ctesias.
Alcman.
Aristotle.
Theophrastus.
Plautus.
Cato.
Cicero.
Strabo.
Pliny.
Columella.
Athenæus.
Origen.
St. John Chrysostom.
St. Epiphamius.
Ammonius.
Herodian (the grammarian).
Festus.
Suidas.
Hesychius.
Eustathius.
Philo.

IV. *Thola, or Tholea, or Tholaath.*—This is a Hebrew word, and is found in Deuteronomy, in the part which treats of the punishments with which the Israelites were menaced if they abandoned the law of God*. This verse is thus rendered in the translation of the Greek and Hebrew texts by the pastors and professors of the Church of Geneva†.

"You shall plant vines, you shall cultivate them, you shall not drink the wine of them and you shall gather nothing from them, because the worms shall eat their *fruit*."

De Sacy, after the Vulgate, translates it in the following manner:

"You shall plant a vine and you shall dress it, but you shall not drink the wine of it, and you shall gather nothing from it, because it shall be destroyed by the worms."

In the first of these translations the word *fruit* is printed in italics, because in fact it is not in the Hebrew; but it ought not to be added, for it is useless to the sense, which is complete without it, and it may lead to error; for the insects which injure the vine by cutting the root are not the same as those which knaw the leaves, nor are the latter the same as those which eat the fruit.

The word Tholath in the interlineary version of the Hebrew Bible by Arias Montanus‡ is also translated by *Vermis*. But the Hebrews had also another word for worm, *rimma*. This word is often employed figuratively in the Bible in the same sense as *thola*, to designate a vile being or an animal engendered from corruption.

The word *rimma* is employed several times in this sense in the Book of Job; in Exodus, chap. xvi. verse 24; in Hosea, chap. xiv. verse 11.

The word *tholaat* is also employed in Job, chap. xxiii. verse 6; in Exodus, chap. xvi. verse 20; in the passage already cited of Deuteronomy; in Psalm xxii. verse 17; and lastly in Jonas, chap. iv. verse 7.

But it is necessary for our object to cite the whole of this last passage, and to justify the translation we shall give, which will differ from that of the professors of Geneva and from the Vulgate of De Sacy. In this chapter it is said, that the prophet having quitted the city, and stopped at a place in the east, made himself a shed.

"Then, said the prophet, God caused a *plant* (kikajon) to spring up, which being elevated above Jonas, became a shadow for his head, which pleased Jonas extremely; but at the dawn of the next day God prepared a worm (tholaat), which wounded the *plant* (kikajon) and caused it to wither."

I can easily show that I am right in translating it thus, in preference to adopting any of the three versions that are before me.

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* Deuteronomy, chap. xxviii. verse 29.

† *La Sainte Bible, ou le Vieux et le Nouveau Testament*, traduits par les Pasteurs et les Professeurs de l'Eglise de Genève. Genève, 1805, t. i. p. 276.

‡ Bible of Arias Montanus.

The word that I have rendered "plant" is *kikajon* in the Hebrew, and the sense of the passage shows that it was a plant large enough to have foliage sufficient to form a shade. But what is this plant? No one is acquainted with it. In the Septuagint it is called a gourd, and St. Jerome makes it an ivy, but St. Augustine informs us in a letter to that Father, that the people of Africa were greatly shocked by this alteration, and obliged their bishop to remove the word ivy from the version of St. Jerome. De Sacy, who retains in his version the ivy of St. Jerome because it is the text of the Vulgate, inclines to the idea that it is a vine or a fig-tree. The pastors of Geneva and M. Gesenius* make the Kikajon a Ricinus agreeing with Bochart†, who leans to the same opinion; but he, far from having proved it, brings before us texts which support the contrary opinion.

But if we indulge in conjectures respecting the plant mentioned in this passage of Jerome, we must for the same reason conjecture the species of insect which caused its destruction, and shall thus be liable to give to the word Tholaat a different signification from that really belonging to it. The liability to error is much increased if we translate with De Sacy, "it pricked the ivy at the root," a circumstance which is not mentioned either in the Hebrew text or in the Vulgate, and which would expose us to the danger of drawing consequences from false premises, which would be erroneous in proportion to the regularity and learning with which they were deduced.

I have therefore altered the translation of the text in such a manner as to leave nothing that may not be read with certitude.

From all that has just been said it appears that the words Rimma, and Thola or Tholaath, are often indifferently employed in the Bible, one for the other, in the sense of worm or grub, of an animal born of corruption, vile, and despicable; but with this difference, that the word Thola or Tholaat is twice used in the Bible to designate a worm which preys upon a plant. In the first of the passages alluded to this plant is the vine; we are ignorant of the species of plant intended in the second passage, but we are certain that it is a plant. We know that such an animal, though it possesses the form of a worm, is not one strictly speaking; we are certain that it is either a grub, or a little insect, or the larva of an insect subject to metamorphosis. The word Rimma has never been employed in this last sense, at least not in the Bible. It appears therefore that in this point of view the Hebrew language is richer than our own, since in ordinary discourse we have only one word to designate the worms of the nut, pear, apple, and all other fruits, and likewise the earthworm, though these animals differ not only in genus, but belong to very different orders‡.

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* Gesenius, *Handbuch*, &c., 1828, 8vo, p. 883.

† Bocharti *Hierozoicon*, vol. ii. p. 623.

‡ Vide Cuvier, *Regnè Animal*, t. iii. p. 180, on the third grand division of ar-

V. *Gaza.*—Gaza is another Hebrew word which is once used in the Bible as the name of an insect particularly injurious to the vine, but it is afterwards frequently employed as the name of an insect which devastates all sorts of plants; with several other names of insects which have given occasion to a great number of dissertations, some of which extend to volumes. We have examined the modern names which appear to correspond to the ancient ones of the insects mentioned in the Bible in connexion with the word Gaza; and this examination may perhaps form the subject of another memoir. At present we shall confine our investigations to the word Gaza, because it is the only one among these names employed to denote an insect particularly injurious to the vine; and we shall notice the other names of insects which accompany the word Gaza, only so far as may be necessary for its interpretation. But such is the diversity of opinion among translators, that to obtain clear ideas it will be necessary to produce the passages in which this word occurs, giving our own translation of them, but retaining the Hebrew names.

The following passage in which Gaza is employed as the name of an insect destructive to the vine is in the prophet Amos, chap. iv. v. 9:

"I have smitten you with a scorching wind, and with mildew. *Gaza* has devoured your gardens, all your vines, and all your olive plants and fig-trees, and you have not returned to me, saith the Lord."

We find the word Gaza again in Joel, chap. ii. v. 25:

"I will restore you the fruits of the year, and all that you have lost by *Arbeh, Jelek, Chazil* and *Gaza*, that destroying multitude that I sent to you."

But there is a passage in Joel, chap. i. ver. 4, of still greater importance with regard to the translation of the word Gaza:

"That which the *Gaza* leaves, the *Arbeh* eats; that which the *Arbeh* leaves, the *Jelek* eats; and that which the *Jelek* leaves, the *Chazil* eats."

In all these passages the Seventy have translated Gaza by *Kampé*, and the Vulgate by *Eruca*, that is to say, a caterpillar. The pastors of Geneva and De Sacy have adopted this translation. It has also the approval of Bochart* and Michaelis. But the Chaldee Version applies Gaza to a sort of creeping locust, and the Talmud enumerates ten species of locusts mentioned in the Prophets alone, and among these is the Gaza.

The three other names of insects mentioned in the same verse of Joel, *Arbeh, Jelek,* and *Chazil,* are included in the ten species of locusts enumerated in the Talmud by the Hebrew doctors.

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ticulated animals, in which this naturalist proves that the worms, otherwise called Annelida, ought to be placed at the head of this division, and before the Crustacea, the Arachnida, and Insects.

* Bocharti *Hierozoicon,* part ii. p. 483.

The expositors of the Bible are divided in opinion upon the signification of the words Jelek and Chazil, but are all agreed upon that of the word Arbeh. There can be no doubt that a locust is signified by this word. The Chaldee Version, the Septuagint, and the Vulgate, all agree in their rendering of those passages of the Bible in which this word is found. The Arbeh is the first of the four species of insects, or creeping animals, named by Moses as proper for the food of man; and Forskael informs us that the Arabs at the present day give the name of Arbeh to the species of locust which is used among them for food. Now we know from Joel that what the Gaza leaves the Arbeh destroys; we are therefore entitled to conclude with certainty that Gaza was the name of an insect not only particularly destructive to the vine, but also to plants of every kind; and that to its ravages succeeded those of several species of locusts, which consumed all that was left undevoured by this formidable insect. Several learned expositors have considered this insect to be a caterpillar, while others of equal authority have decided it to be a sort of creeping locust. We shall consider this point upon another occasion; but at present, faithful to the plan we have traced for our guidance, having exhausted what the Hebrews have written upon the insects destructive of the vine, we shall pass to the Greeks.

VI. *Ips, Iks.*—I shall treat of these two words in one article, because, as will be seen, they cannot be separately considered.

The word *Ips* is used in ancient authors as the name of an insect particularly injurious to the vine; but it is also employed by Homer, St. John Chrysostom, and the lexicographers and grammarians of the lower ages, to denote an insect or worm which preys upon horn; and in these two acceptations this word cannot denote a worm properly so called, which has another name in the Greek language.

We will first consider the Ips of Homer.

This word is employed in the Odyssey, book xxi. verse 295, in which Ulysses, who is not yet recognised by his friends, is represented as receiving his terrible bow. The poet says, "The hero takes the bow, examines it with attention, and turns it in every direction, fearing that in the absence of its master the horn might have been injured by the *Ips*."

To ascertain what species of horn was subject to the attacks of the Ips of Homer, we must discover the animal the horns of which were employed in the time of Homer in the construction of bows of the best quality, such as were suitable for a king like Ulysses. Upon this point Homer himself gives us information: in the Iliad, book iv. v. 105, we read that the bow of the divine Pandarus was made of the horns of the *Aigos*, or the Ægagrus or wild goat; that its horns were five feet four inches in length; and that after being polished and united with care by a skilful workman their extremities were gilt.

The horns of the Ægagrus are often nearly three feet and a half in length; they are naturally bent, and when united, as Homer describes, they would form a bow of the dimensions stated by him.

The Ægagrus, or wild goat, is found, though but rarely, in the mountains of Western Europe. One was killed while I was in the Pyrenees, and the horns, which I saw, were two feet and a half in length. This animal is very common in the East: in Persia it is named Paseng. Burckhardt informs us that the Arabs of Syria give it the name of Bidin (Beden), and that the wild goats are found in their countries in herds of forty and fifty. Their flesh is much esteemed, and the horns are collected and sent to Jerusalem to be made into handles for knives and poniards. Burckhardt* saw a pair of the horns of these animals which were three feet and a half in length. We may suppose that the Ips of Homer must be both known and feared by the warriors of that country.

But the word Ips is not found thus applied in the Greek authors who follow Homer; and it is employed in Strabo, Theophrastus, and the writings of the learned agriculturists whom we shall presently quote, to denote an insect or a worm injurious to the vine, consequently a larva which preys upon plants and not upon horn.

We, however, again find the word Ips with the same signification as when employed by Homer in a remarkable passage of St. John Chrysostom, which I shall translate thus: "The same deleterious effects as are produced by copper upon the body, by rust upon iron, by moths in wool, worms in wood, and Ipes in horn, vice produces in the soul†."

But I repeat that in the most learned Greek authors, and those of the highest authority, Ips is an insect which preys upon the vine.

We read in Strabo:

"The Erythræans give to Hercules the name of Ipoctonus, that is, destroyer of the Ipes, insects thus named which prey upon the vine‡."

Theophrastus§, after describing how worms are produced in corn, adds that the Ipes are engendered by the south wind; and in another place he says, "There are, however, places where the vines are not in-

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* Burckhardt, *Travels in Syria and the Holy Land*, 1822, p. 405; Fischer, *Synopsis Animalium*, p. 483; Cuvier, *Règne Animal* (2eme edit.) t. i. p. 275.

† Sanctus Joannes Chrysost. App. vol. iv. p. 669, E. St. John Chrysostom employs the word *Scolex* for the worm which preys upon wood. *Scolex* signifies the earth-worm, the true worm; in fact, in the grammarians of the lower ages, according to the same authorities Scolex also means the worm infesting the ox, an intestinal worm, or the larva of an insect altogether different to the former. The Scolex of St. John Chrysostom, or the worm preying upon wood, can only be the larva of an insect, and in fact Aristotle employs the word with this meaning when he says that every insect proceeds from a Scolex.

‡ Strabo (edit. *Almenoven*) folio, book xiii. p. 613: in the French translation, vol. iv. p. 213.

§ Theophrastus, *De Causis Plantarum*, book. iii. chap. 22; or 23 of Schneider's edit. vol. ii. p. 299. Scaliger translates the word *Ips* by *Convolvulus*, for which we shall see the reason elsewhere.

fested by the *Ips*; this occurs in places exposed to the winds, where there is a free current of air, and no excess of humidity."

In the Geoponics* it is said, "that to prevent the little worms named *Ipas* from attacking the vines, the reeds used for the vine-props should be smoked, because the reeds decomposing in the earth engender little worms, which will otherwise ascend upon the vine."

Galien, cited by Aldrovandus, says that the black earth kills the *Ipes*.

In the Dictionary of Suidas† the word *Ipi* is defined by Worm; but it is remarked that *Ips* is a better expression. That work, however, does not furnish any other information upon the word *Ips*.

But the name *Ips* in a form slightly altered, or another insect under a name differing but little from that, is mentioned by several authors as being very hurtful to the vine.

In a fragment of Alcman quoted by Bochart‡, it is said that "the variegated *Ika* is the scourge of the young shoots of the vine."

The grammarian Ammonius in his Treatise upon Synonyms §, says also, "the *Ikes* are animalcula which destroy the buds of the vine."

Bochart thinks that *Ips* and *Iks* are but one word, according to two different dialects.

Valckenaer in his notes upon Ammonius is of the same opinion: *Ego verisimilam censeo* (says this accomplished critic,) *Sam. Bocharti sententiam qui ab Alcman* Ika, *ex dialecto pro* Ipa *positum sagaciter animadvertit, et ex idoneis auctoribus loca produxit in quibus, qui in vitibus nascuntur vermiculi* Ipes *dicuntur*." Valckenaer concludes with Bochart that *Ips* is the most ancient form of the word.

However in Hesychius, and in another grammarian quoted by M. Boissonade, these two words are distinguished from each other and applied to two different insects.

In Hesychius's Dictionary we find *Iks* as the name of an animalculum (Theridion) which infests the vine; and in the same work *Ips* has this explanation, that this word is employed by grammarians to denote an insect which preys upon horn.

The anonymous grammarian cited by M. Boissonade in his notes to his edition of Herodian ||, enumerating the various name sattributed to the different species of worms or larvæ, according to the substances in which they lodge, or which they destroy, mentions *Iks* as the worm of the vine, and *Ips* as that of meat and horn.

Have these two species of insects been accurately distinguished from each other, and the habit acquired of expressing them by different

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* *Geoponic.*, edit. Niklas, chap. liii. vers. 423.

† Suidas, *Lexicon*, edit. of Kuster, 1705, folio, vol. ii. p. 141.

‡ Bocharti *Hierozoicon*, vol. ii. p. 213.

§ Ammonius, tit. 2, chap. v. *De Differentia adfinium Vocabulorum, nunc primum editum ope MSS. primæ edit. Aldinæ*. Vulgavit Valckenaer, pp. 73, 74.

|| Herodiani *Partitiones*, Lond. 1819, 8o, p. 58.

names? Or is it a distinction erroneously established by grammarians and lexicographers, who of one word slightly varied have made two different words? Whatever may be the fact, the consideration of it is foreign to our present purpose, and will engage our attention at another opportunity. We confine ourselves at present to collecting the facts of the language as we derive them from the critical examination of the texts, without anticipating the consequences which may be deduced from them.

From what has been said we draw the following conclusions:

1st, That in the most learned ancient authors, and in those who have treated *ex professo* of agriculture, natural history and geography, the word *Ips* has never been employed except to denote the larva of an insect very injurious to the vine.

2nd, That in Homer, St. John Chrysostom, and lexicographers and grammarians of the period of the decline of literature, the word *Ips* is exclusively employed to denote the larva of an insect preying upon horn.

3rd, That the word *Iks*, whether it be considered as a different word from *Ips*, or the same in another dialect, is employed by Alcman, and the lexicographers and grammarians of the lower ages, to designate exclusively a variegated insect, which injures the vine, and preys upon its buds.

VII. *Spondyle or Sphondyle.*—Aristotle in his Natural History of Animals*, after describing the mode of coition of flies and beetles, adds that the Spondyle (or Sphondyle), the Phalangium, and other insects agree with them in this respect.

I say Spondyle or Sphondyle, because the editors and translators of Aristotle's work are divided upon this point. In the Greek text of Schneider the word is *Sphondylaï*, in that of Camus *Sphondylaï*: they each represent that it is an insect, because in this passage the meaning is evident; but in another passage of the same work†, speaking of the diseases of the horse, Aristotle mentions cases in which that animal draws up the hip and drags the foot, and says, "the same thing occurs if he devours the Staphylinus. The Staphylinus is of the same size and appearance as the Sphondyle."

M. Camus, in his translation, writes Sphondyle, and agrees with Hesychius, who represents the Staphylinus, and consequently the Spondyle, as an animal. M. Schneider, on the contrary, who this time also writes Sphondyle, considers this word to be entirely different from Spondyle, the name of an animal in the first passage which I have quoted. M. Schneider, adopting the opinion of Scaliger, regards the Staphylinus,

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* Aristot., *Hist. Anim.*, book v. chap. 7, edit. Schneider, vol. ii. p. 181 of the translation; and vol. i. p. 190 of the Greek; and book v. chap. 8. vol. i. p. 219 of the translation of Le Camus.

† Aristot., book viii. chap. 24, Schneider, vol. iii. p. 276.

and consequently the Spondyle mentioned in this last passage, as a plant (the parsnep)*.

M. Schneider in his note does not attempt to prove the accuracy of his translation, but contents himself with citing the authority of Scaliger. I confess that I here lean to the opinion of Le Camus. It is not, however, necessary to discuss the subject; and the circumstance that Aristotle has twice mentioned an insect named Spondyle or Sphondyleis of little importance, since he gives us no information respecting it. In the second passage, indeed, he compares it to the Staphylinus, but we know still less of the Staphylinus than of the Spondyle, and in neither passage is any mention made of the vine. Nor should we have noticed the Spondyle of Aristotle were it he alone who had spoken of it; but Pliny † remarks upon the Aristolochia and the wild vine (*Vitis silvestris*), which vegetates a year in the shade, that no animal infests the roots of these plants, nor of others of which he has treated, excepting the Spondyle, a sort of serpent, which attacks them all. "*Et Aristolochia ac vitis silvestris anno in umbra servantur: et animalium quidem exterorum nullum aliud radices a nobis dictas attingit excepta Spondyle quæ omnes persequitur. Genus id serpentis est.*"

Schneider after quoting this passage adds, "*Ineptè ut solet.*"

Pliny has conceived with genius and executed with ability an abridged encyclopædia of human knowledge; he may perhaps be esteemed the author of the most learned work ever composed; and it certainly is not allowable to speak of a writer of such merit with the asperity and disdain manifested on this occasion by the learned German. However, if the severity of the criticism sometimes occasioned by the difficulties we experience from the gross errors into which Pliny has been led, by the necessity of treating of so many things which he understood imperfectly, can be excused, it is certainly in the editor or translator of Aristotle's Natural History of Animals. Pliny has borrowed extensively from that admirable work; sometimes he is contented with translating it; but even then he generally confuses, by inaccurate or pompously obscure phrases, what Aristotle had explained with clearness and precision, and often mixes with it popular and ridiculous stories, or erroneous and inconsistent notions. But it would have been better if M. Schneider, who combines the knowledge of a naturalist with the erudition of a philologist, instead of allowing himself to use the expression we have cited on this passage of Pliny, had inquired what advantage might be reaped from it; he would then have seen that the error of Pliny will enable us to determine what species of insect is meant by the Spondyle in the first passage of Aristotle, and perhaps also in

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* Schneider, *Arist. Anim. Hist.*, vol. iv. p. 665.

† Plin. *Hist. Nat.* book xxvii. §. 118. (chap. 13.); vol. viii. p. 106 of the edition of Franz.

the second. As it is certain that, in Europe at least, no serpent injures the roots of plants, from the comparison of this passage of Pliny with that of Aristotle we deduce the following facts:

1st. That the larva of the insect named Spondyle by the Greeks was known to the Latins, and that it devoured the roots of plants of every kind.

2nd. That this larva was very large, since it was compared to a small serpent.

We shall see hereafter the consequences deducible from these circumstances.

It may perhaps be said that this long discussion on the word Spondyle might have been omitted, because Pliny speaks only of the wild vine, *Vitis silvestris*, which is not really the vine, nor has it any relation to the plant producing grapes, but which was an annual, like the Aristolochia, as Pliny himself informs us. To this I reply, that the vine is included in the plants mentioned by Pliny as being exposed to the attacks of the Spondyle, and that consequently anything relating to this insect belongs strictly to my subject.

[To be continued.]

SCIENTIFIC MEMOIRS.

VOL. I.—PART II.

Article IX. concluded.

Researches relative to the Insects, known to the Ancients and Moderns, by which the Vine is infested, and on the Means of preventing their Ravages; by M. le Baron Walckenaer, *Hon. Memb. of the Entomological Society of France.*

From the *Annales de la Société Entomologique de France*, vol. iv. p. 711, *et seq.*, and vol. v. p. 219, *et seq.*: read Nov. 18, 1835.

VIII. *Cantharis.*—DIRECTIONS are given in the Geoponics* for preventing Cantharides from injuring the vine: these insects are to be macerated in oil, and the plant rubbed with the preparation.

Another recipe for the preservation of the vine is given in Palladius, for which the Cantharides of the rose are required; they are to be macerated in oil until an unctuous liniment is formed, with which the branches are to be rubbed†.

The name Cantharis occurs very frequently in several Greek and Latin authors without any mention of the vine. Pliny, however‡, says, "*Verrucas Cantharides cum* uva taminia *intritæ exedunt*,"— "Cantharides pounded with the *uva taminia* destroy warts."

The *Uva taminia*, which we translate by wild grape, is, I apprehend, unknown; it is certainly not the fruit of the vine.

It would be superfluous to produce here the numerous passages of the ancient authors in which the word *Kantharis* occurs, because there can be no doubt as to its signification. They all prove evidently that the ancients understood by this word, not the larvæ of insects, but

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* *Geoponica*, edit. Niclas, 1781, 8vo, p. 418. ch. 49.

† Palladius, book i. chap. 35; vol. i. p 43. Bipontine edition.

‡ Pliny, book xxx. chap. 9.

perfect insects; that these insects were all of the order Coleoptera, vulgarly Beetles; and that Cantharis was a general term denoting several species of beetles, but not all the species indifferently. This word is always employed by the ancient authors to denote those species of Coleoptera, or beetles, which are brilliantly coloured and remarkable for their vesicant or venomous properties; but those authors differ greatly from each other with regard to the species which they have in view.

Thus the Cantharis of Aristotle appears to be the same species as that mentioned by Aristophanes*; but it is an insect very different from that with black and yellow bands, which has been so well described by Dioscorides that it is impossible to be mistaken by modern naturalists. To this latter insect must be referred the winged Cantharis of a fulvous colour, to which on account of its malignity and mortal poison Epiphanius compares heresy†. The Cantharis of Origen‡, produced from the larva of an insect which lives in the flesh of the ass, is evidently a different species from that of Epiphanius and Dioscorides, and also from that of Aristotle and Aristophanes, though more resembling the latter.

Pliny mentions several species of Cantharis§, which for want of exact details are difficult to recognise; but when he says (book xviii. chap. 44.), "*Est et Cantharis dictus Scarabæus parvus frumenta erodens*||," we instantly fix upon the small and formidable coleopterous insect to which he here gives the name of Cantharis. Theophrastus, who has also mentioned the little insect engendered in corn, gives it the name of Cantharis.

From what has been said it appears that to find the insect named Cantharis considered by the ancients as injurious to the vine, we must seek for it among the perfect insects of the class Coleoptera; among those which are brilliantly coloured and distinguished by their vesicant venomous quality; and among the largest as well as the smallest species of that class.

IX. *Kampe and Phtheir.*—I class these two words together for an instant, regardless of their different signification, because I find them united in a passage of the Geoponics¶, the only place in which the first is mentioned in connexion with the vine. The author gives a recipe used by the Africans to preserve the vine from the *Phtheirs* and *Kampes* which infest it. Ctesias also mentions the *Phtheirs* which destroy the vine in Greece**.

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* Aristophanes quoted in Aldrovandus *De Insect.*, chap. iii. vol. i. p. 180.

† St. Epiphanius, *Panar. Rom.*, p. 1067, A. edit. Petav.

‡ Origen, *Contra Cels.*, book iv. chap. 57. p. 549, A. edit. Delarue.

§ Pliny, *Hist. Nat.*, book xxix. chap. 30; vol. iii. p. 107. edit. Miller.

|| Pliny, *Hist. Nat.*, chap. 44. or 17. vol. vi. p. 138 of the edition of Franzius.

¶ *Geoponica*, edit. Niclas, chap. xxx. vol. iii. p. 485.

** Ctesias, *Indicorum*, chap. xx p. 253. edit. Baehr. Frankf. 1824, 8vo.

X. *Kampe.*—Aristotle[*] was well acquainted with the metamorphoses of the Butterfly, the larva of which he calls *Kampe*. He particularly mentions that which feeds upon the cabbage.

Theophrastus[†], in his History of Plants, employs the word *Kampe* for an animal which eats the leaves and flowers of all sorts of trees.

Pliny[‡], abridging the passage of Theophrastus alluded to, translates *Kampe* by *Eruca*, Caterpillar.

We have already seen that the word Kampe occurs three times in the translation of the Bible into Greek by the Seventy, twice in Joel, and once in Amos[§]; and that in the Latin translation of the same passages in the Vulgate, the word *Eruca* is always employed, though, as we have already remarked, we are not certain that either *Kampe* or *Eruca* gives the sense of the Hebrew *Gaza*, for which they are used.

St. John Chrysostom, in a remarkable passage, speaks of the *Kampes* as having been an object of worship in the times of paganism[‖]; and this word is with reason rendered *Erucas*, Caterpillars, in the Latin translation. In the Dialogues of Pope Gregory the Great[¶] mention is made of Boniface, Bishop of Ferentum, who enters a garden in which are a very large number of Caterpillars: "*Ingressus hortum, magna hunc Erucarum multitudine invenit esse coopertum.*"

Pope Zachary, translating the same Dialogues into Greek, renders the word *Eruca* by *Kampe*.

But the following passage of Columella leaves no doubt upon the subject[**]:

"*Animalia quæ a nobis appellantur* Erucæ *Græce autem* ΚΑΜΠΑΙ *nominantur.*" "The animals that we (the Romans) call *Erucæ* (Caterpillars) are named in Greek *Kampai.*"

Palladius and Columella, though writing in Latin, always prefer the Greek to the Latin word when they have occasion to mention the caterpillar.

Thus Palladius, giving instructions how to destroy the caterpillars infesting vegetables and the vine, recommends that the stems of the plant producing garlic should be burnt in the garden, and the pruning-knife employed to dress the vine anointed with the garlic, and says: "*Campas fertur evincere qui fusticulos allii sine capitibus per horti omne spatium comburens, nidorum locis pluribus excitavit. Si contra*

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[*] Aristotle, *De Anim.*, book v. chap. 19.

[†] Theophrastus, book iv. chap. 16.

[‡] Pliny, book xii. chap. 24.

[§] Joel, i. 4; ibid. ii. 25; Amos, iv. 9.

[‖] St. John Chrysostom, *Homil.* 2. *in Acta Apostol.*, vol. iv. p. 621, book xiv Eton edit. 1612.

[¶] St. Gregory, *Dialogorum Libri IV.*, book i. chap. 9. vol. ii. p. 396. Paris edition, 1675, folio.

[**] Columella, book xi. chap. 3.

*easdem vitibus voluerimus consulere, allio trito falces putatoriæ feruntur unguendæ**."

Columella having occasion to speak of the destruction caused by the Caterpillar, twice employs the word *Campe.*

> " Nec solum teneras audent erodere frondes
> Implicitus conchæ limax, hirsutaque Campe †."

And afterwards:

> " Non aliter quam decussa pluit arbore nimbus
> Vel teretes mali, vel tectæ cortice glandis,
> Volvitur ad terram distorto corpore Campe‡."

It is therefore evident that it is among the Caterpillars, or the larvæ of the Lepidoptera or Butterflies, that we must search for the Kampes, which, according to the Geoponics, are produced in the vine and destroy it.

XI. *Phtheir.*—This Greek word is known to apply to the parasitic insect peculiar to man, the Louse. We shall have to examine whether Ctesias § and the author of the Geoponics have employed this word to signify all sorts of insects injurious to the vine, which include implicitly the Kampes or Caterpillars; or whether they had in view a particular insect, which being small was for that reason considered by cultivators as the Louse of the vine.

XII. *Julos or Julus.*—Suidas, an author of the ninth or tenth century, says in his Dictionary ||, that the *Julos* is a worm of the vine; that it has a great number of feet, and is also called *Multipede*; that it coils itself up, and breeds in moist vessels.

From these few particulars the most learned lexicographers have not hesitated to establish the identity of the *Joulos* with the Ips, Iki, Convolvulus, and other insects mentioned by the ancients as injurious to the vine.

We shall soon see how many errors are accumulated from thus establishing relations for which there is no authority in any text.

No ancient author has mentioned the *Julos* in connexion with the vine, or as an animal destructive of it.

The Latins have employed the word *Julus* or *Julius* in several of the same senses as were given to it by the Greeks; but I am not aware that they have ever employed it to denote a worm or an insect, or any animal whatever.

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* Palladius, in the *Scriptores de Re Rustica,* Bipontine edit., vol. i. p. 43.

† Columella, *De Cultu Hort.*, ver. 324. vol. i. p. 410. Bipontine edit., 1787, 8vo.

‡ Columella, *De Cultu Hort.*, book x. ver. 366. Gesner in his Dictionary also quotes Sextus Empiricus, t. 14, on the word CAMPE.

§ Ctesias, *Indicorum,* chap. xxi. p. 253. edit. Baehr. Frankfort, 1824, 8vo.

|| Suidas, *Lexicon,* vol. ii. p. 126. Frankfort edit.

Aristotle speaks of the *Julios* in his History of Animals[*]; but all the information which he gives us is, that it is an insect without wings, like the Scolopendra.

Speaking of animals in general, Aristotle distinguishes those which have only four feet from those with a larger number[†], and among the latter he names the Scolopendra and the Bee. It is evident that Aristotle names these two examples as being the extreme limits of the class; one animal having but six feet, only two more than the animals of his first division, or quadrupeds, and the other having a much larger number: one of Aristotle's commentators however, forming his judgement in a similar manner to the lexicographers, makes a Wasp of the Scolopendra. An insect without wings a Wasp!

Aristotle mentions the marine Scolopendra[‡], a different animal, that lives in the sea; he describes it, and says that it is similar to the terrestrial Scolopendra, but of a deeper red colour; that it has a larger number of feet, and those more slender. He remarks upon the terrestrial Scolopendra[§], that if it be divided into several parts, each part has a progressive movement.

Pliny[||], translating this passage of Aristotle upon the marine Scolopendra, says that it resembles an insect of the earth named Centipede; and in another part of his work he thus defines the Centipede[¶]: "The Millipede, which is also named Centipede and Multipede, is a kind of worm of the earth, which crawls upon all its feet, describing an arch, and coils itself up on the slightest touch. The Greeks call it *Oniscos*, and some of them *Tylos*." Further on he says again: "The species of Scolopendra which moves without sinuosities, and is named by the Greeks Seps, and by others Scolopendra, is more venomous."

"*Millipeda, ab aliis centipeda aut multipeda dicta, animal e vermibus terræ pilosum, multis pedibus arcuatim repens, tactuque contrahens se:* Oniscon *Græci vocant, alii* Tylon......*Illam (centipedem) autem quæ non arcuatur* Sepa *Græci vocant, alii Scolopendram minorem perniciosamque.*"

I may here remark, that in this passage Pliny[**] confounds the Julios with another species of Millipede, to which Aristotle gives the name of the polypodous Ass, [Asellus,] *ονος ὁ πολυπος*; and Pliny afterwards appears to give the names of Seps and Scolopendra to the Oniscos,

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[*] Arist., *Hist. Anim.*, book iv. chap. 1; vol. i. p. 129 of the Greek text, and vol. ii. p. 126 of the Latin translation, Schneider's edition; vol. i. p. 171 of the translation of Le Camus.

[†] Arist., book i. chap. 5. vol. ii. p. 16 of Le Camus's translation.

[‡] Arist., book ii. chap. 4.

[§] *Ibid.*, book iv. chap. 7.

[||] Pliny, *Hist. Nat.*, book ix. chap. 43.

[¶] *Ibid.*, book xxix. chap. 6. vol. x. p. 128.

[**] Pliny, *Hist. Nat.*, book xxix. chap. 39. vol. viii. p. 273. Arist., *Hist. Anim.*, vol. v. chap. 25. (*vulgò* 31); Scaliger, 126. vol. ii. p. 224. Schneider's edition.

and says that it is smaller than the Centipede, and moves without making sinuosities. Errors of this sort are common in this author.

Numenius, cited by Athenæus, calls the *Julios* the entrails of the earth. Eustathius, commenting upon this passage, and Theon, a more ancient author, give different reasons for this expression.

Hesychius says that the *Joulos* is like the Polypus, that it lives in moist places, and that it is different from the Onos or Asellus.

Lycophron applies the epithet of *Juliopezos* to a vessel propelled by a great number of oars.

From all these texts we gather the following particulars: that the *Julos* or *Julus* was an apterous insect, or without wings, and furnished with a great number of legs; that it rolls itself up immediately upon being touched; that it describes a curve or sinuosities in moving; that it conceals itself in the earth; that it is found in moist places; and lastly, that Suidas alone informs us that this insect preys upon the vine.

XIII. *Biurus.*—We now arrive at names applied by the Romans to insects destructive to the vine, and we shall commence with a word which, from its etymology, appears to have been derived from the Greek.

The name of *Biurus* employed by Cicero to denote an insect injurious to the vine is only known to us from a passage of Pliny the naturalist. Speaking of different medical prescriptions, and several curious particulars relative to the history of animals, he concludes a chapter with these words: "Marcus Cicero says that there are insects, named Biuri, which prey upon the vines in Campania." "*M. Cicero tradit animalia* Biuros *vocari qui vites in Campania erodant.*"

It has been rightly remarked that this word is derived from the Greek *oura*, and appears to be a synonym of *Bicaudes*, insects with a double tail. This etymology deserves attention, as we learn from it the only peculiarity by which this insect can be recognised. In the most ancient manuscripts the word is written *Biuros*, and we should reject the orthography of *Byturos* adopted by certain editors of Pliny. This latter word has been employed by modern naturalists as the name of a genus of Dermestes*, while they have neglected the true orthography.

XIV. *Involvolus*, *Involvulus*, or *Involvus.*—The words which remain to be noticed are purely Latin; they have, if I may be allowed the expression, a family likeness, and appear to be derived from each other.

We shall commence with the word which the most ancient author has employed, and which is that which has given occasion to this memoir.

Involvolus or *Involvulus* occurs in Plautus.

In the Cistilliarius, act 1, scene 2, verses 455–458, the slave Lampa-

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* See Latreille, in Cuvier's *Règne Animal*, vol. iv. p. 506. [edit. of 1829.]

discus, speaking to his mistress of another slave, says that she imitates a dangerous beast:

> "Imitatur nequam bestiam, et damnificam."

"What is it, I pray?" says his mistress:

> "Quamnam, amabo?"

The slave replies: "The Involvolus, which rolls and envelops itself in the leaf of the vine. In the same manner does she purposely involve the meaning of her speech*."

> "Involvolorum, quæ in pampini folio intorta implicat se,
> Itidem hæc exorditur sibi intortam orationem."

In the Dictionary of Pomponius Festus† we find this definition of the word *Involvus*: "*Vermiculi genus qui involvit pampino.*"

No one can hesitate to recognise the *Involvolus* of Plautus in the *Involvus* of Festus. The word is the same with a very slight alteration. The curious industry of this insect is confirmed by the testimony of two authors; and we learn from Festus that the *bestiola* of Plautus was not a perfect insect, but the larva of an insect.

XV. *Convolvulus.*—Marcus Porcius Cato, in his treatise *De Re Rustica*, gives a recipe to guard the vine from the attacks of the insect named *Convolvulus*, which is there engendered. The directions are, to boil the residuum of oil until it acquires the consistency of honey, and then to anoint the top and the axillæ of each plant with the preparation‡.

"Convolvulus *in vinea ne siet, amurcam condito,*" &c. And at the end, "*Hoc vitem circum caput, et sub brachia unguito,* Convolvulus *non nascitur.*"

Pliny thus quotes this recipe §:

"*Ne* Convolvulus *fiat in vinea, amurcæ congios duos decoqui in crassitudinem mellis,*" &c., &c. And in the conclusion, "*Hoc vites circa capita ac sub brachiis ungi; ita non fore* Convolvulum."

These passages, which are the only ones in which the name *Convolvulus* occurs, contain no information respecting the insect intended by it, excepting, indeed, that it greatly injures the vine. We shall have to examine whether it be the same insect as the *Involvulus* of Plautus, or whether the two words are applied to two different insects.

XVI. *Volvox.*—It will not be necessary to inquire whether the insect

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* It was necessary for my purpose to translate this passage literally; Limiers, *Œuvres de Plaute*, 12mo, vol. iii. p. 293; Levée, *Théâtre des Latins*, 8vo, vol. iii. p. 416; Naudet, *Théâtre de Plaute*, 8vo, vol. iii. p. 187, may be consulted as to the manner in which it has been rendered by various translators.

† Pomp. Festus, book ix. p. 193. edit. of Dair.

‡ M. P. Cato, *De Re Rustica*, chap. 95. vol. i. p. 52, Bipontine edition; vol. i. p. 84. of the *Scriptores Rei Agrariæ*, 2nd edit. of Gesner.

§ Pliny, book xvii. chap. 28, 47. vol. ii. p. 91 of the edit. of Hardouin, folio; vol. v. p. 741. of the edit. of Franzius.

named *Volvox* by Pliny is the same as that which he names *Convolvulus*, for he himself distinguishes them.

After indicating the remedy for the *Convolvulus*, this author informs us that the *Volvox* is another animal which destroys the young grapes; and to prevent its propagation, he recommends that the knife employed to dress the vine should be wiped with the skin of the beaver, and the plant rubbed with bear's blood.

"*Alii* Volvocem *appellant animal prærodens pubescentes uvas: quod ne accidat, falces, cum sint exacutæ fibrina pelle detergent, atque ita putant; sanguine ursino liniri volunt post putationem easdem.*"

XVII. *Volucra.—Eruca.*—We must consider these two words together, because we find them associated in the same passage of Columella; and indeed I am not certain that they ought to be separated from *Volvox*; for it must be stated, that in the passage of Pliny which I have just quoted, several editors read *Volucra* instead of *Volvocem. Volvocem* is however the reading of all the ancient manuscripts of Pliny; and the reading of *Volucra* has only been introduced, because there is a passage in Columella which, though rather different, appears to have been derived from the same source: and as it is impossible to substitute *Volvox* for *Volucra* in Columella,—this latter word being a second time employed in the plural, in a verse which cannot be altered without destroying the measure,—the editors of Pliny have decided upon altering the text to bring it into agreement with that of Columella. Columella's commentator, Gesner, justly censures this alteration, and recommends that the reading of the manuscripts should be preserved in these two authors, and that consequently the word *Volvocem* should be re-established in Pliny.

In his treatise upon trees, Columella*, after mentioning the rats and mice which infest the vine, says: "*Genus est animalis*, Volucra *appellatur, id fere prærodet teneras adhuc pampinos et uvas: quod ne fiat, falces quibus vineam putaveris, peracta putatione sanguine ursino linito...... Vel si pellem fibri habueris, in ipsa putatione quoties falcem acueris, ea pelle aciem detergito atque ita putare incipito.*"

"There is a kind of animals named *Volucra*, which destroys almost entirely the tender shoots of the vine and the grapes. To prevent its ravages, the vine after it is dressed should be frequently anointed with bear's blood, and the pruning-knife rubbed with beaver's skin every time it is sharpened."

Columella in his poem upon the cultivation of gardens, after speaking of culinary vegetables, recapitulates the enemies by which the hopes of the agriculturist are destroyed, viz. tempests, rain, hail, inundations, and, which are still more formidable, the Volucra and Caterpillar, enemies of Bacchus and green willow plats, which envenom the seed,

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* Columella, *De Arboribus*, chap. 15. vol. i. p. 55.

devour the leaves, and leave nothing of the plant but a withered and barren stem.

> "Brassica, cumque tument pallentia robora betæ,
> Mercibus atque olitor gaudet securus adultis,
> Et jam maturis quærit supponere falcem,
> Sæpe ferus duros jaculatus Jupiter imbres,
> Grandine dilapidans hominumque boumque labores:
> Sæpe etiam gravidis irrorat pestifer undis,
> Ex quibus infestæ Baccho, glaucisque salictis
> Nascuntur *Volucres*, serpit *Eruca* per hortos.
> Quos super ingrediens exurit semina morsu,
> Quæ capitis viduata coma, spoliataque nudo
> Vertice, trunca jacent tristi conjuncta veneno*."

Here the *Volucres* and the *Erucæ* are mentioned by Columella as different insects; the first are described as enemies of the vine, the second as destructive to the willow. "*Et quibus infestæ Baccho nascuntur Volucres, glaucisque salictis* (*infesta*) *serpit Eruca per hortos.*"

This interpretation, which does not appear doubtful, suggests a curious remark. It is this, that with the exception of the Latin translation of the Bible—the Vulgate—in which the word *Gaza* has been improperly rendered *Eruca*, the word *Eruca* has never been employed by the Latins, in its Latin form, to denote an enemy peculiar to the vine. Pliny and Columella mention the *Eruca* as the scourge of trees and plants in general, without excepting the vine, but they do not speak of it as its especial enemy; and when Palladius, in the passage which we have cited, gives a specific for the caterpillars infesting the vine, we have seen that he employs the word *Campas* and not *Erucas*.

This observation is not made with the intention of inferring from it, that among the names applied by the Latins to insects infesting the vine there are none denoting Caterpillars, or the larvæ of Lepidoptera; but it suggests the idea that the insects injurious to the vine mentioned under the names *Involvulus*, *Convolvulus*, *Volvox*, and *Volucres* by the Latins, were considered by them as particular species of worms or insects, and not as the larvæ of Lepidoptera, or Caterpillars, or of animals of the same nature as the *Kampai* and *Erucæ*; and that consequently the Latins were unacquainted with the metamorphoses of these species of insects.

In this critical examination I have been careful not to omit any words which are found employed in the writings which remain to us of the Hebrews, Greeks, and Romans to denote insects destructive to the vine. I shall now pass to the second part of this memoir, in which we shall explain the ancient texts by the aid of modern science, and offer such practical considerations as may be useful to the agriculturist.

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* Columella, book x., *De Cultu Hortorum*, ver. 326 to 336.

Second Section.

Determination of the species of insects known to the ancients and moderns, by which the vine is infested, and indication of the means of preventing their ravages.

I. *Preliminary Remarks.*—In the first part of these researches, I examined the ancient texts in which the names of insects injurious to the vine occur, taking the authors in chronological order whenever this plan did not destroy the relations of etymology or derivation existing between the words the signification of which was to be determined. This method appeared to me the only one adapted to the attainment of the end which I proposed.

All languages vary, and, like the people by whom they are spoken, experience the effects of time, revolutions, and custom. Various contemporary writers employ the same words in different senses, either because they do not possess the same degree of knowledge of the things designated by them, or because they differ from each other with respect to the intention with which the terms in question are employed; one writer being required to limit his meaning to one simple, special, or rigorous sense, and another, on the contrary, having in view a figurative sense only, or a vague or general notion.

The examination of all the texts in which the same word is employed, has furnished us with the signification, more or less determinate, which each author attached to the word, and also with the different circumstances and particulars contained in each text relative to the insect named, which consequently may serve as means to distinguish it.

We have been careful to recapitulate the various significations which result from our critical examination of each word; to compare the imperfect notions of the ancients with the more precise knowledge of the moderns; it will therefore only be requisite to recall to our minds the result of each of these examinations, without being perplexed, in this last and difficult investigation, by philological discussions. Should we be forced to commence new inquiries of this nature, it will only be with regard to words which offer matter for curious or useful digressions, and not in relation to those which essentially belong to the subject of which we are treating.

But it will not here be requisite to follow the same order of discussion which we thought it necessary to adopt in our first section. We are not now endeavouring to determine the significations given by each author to a certain word, independently of its real sense, but to ascertain that real sense from the various significations that have been ascribed to the word, and the different applications which have been made of it. Things, not words, are now the subjects under consideration; things must therefore indicate the order to be followed in determining the value of words. We therefore commence with insects which are only slightly connected with our subject, or upon which the ancients have furnished us with particulars from which only vague and uncertain or too general notions can be derived; and we shall pass successively to those insects which are the principal objects of our researches, and for which the texts furnish us with circumstantial details and more precise methods of determination; according to the custom of algebraists, who first eliminate from their equations the parasitic quantities, or those which furnish only imperfect data for the solution of the problems to be solved.

II. *Spondyle, or Sphondyle.*—Scarabæus Melolontha *of Linnæus.—The* Chafer (Hanneton).—*Digression on the various species of Chafers known to the ancients, on several Scarabæi which are allied to that genus, and on the employment of the word* Melolontha *by the ancients and the moderns.*

According to the order which we have marked out, the word Spondyle, or Sphondyle, claims priority.

The conclusions derived from the examination and comparison of the texts are, that the larva of this insect is sufficiently large to have been taken for a small serpent; and that it preys upon the roots of all sorts of plants, excepting that of the Aristolochia, or Wild Vine, *Vitis sylvestris,* which is the Clematis or another plant, but which is not the Vine*.

We are acquainted with only one species of larva which fulfills these conditions; it is the common Cockchafer, so well known and so much dreaded by horticulturists under the name of the white worm. The larva of the *Melolontha Fullo,* or of the *Melolontha vulgaris* of modern naturalists, according to the results we have obtained, is the Spondyle of Aristotle and Pliny.

I find in Aldrovandus† that Agricola said that the modern Greeks give the name of Spondyle to a species of worm of the size of the little finger, with the head of a reddish colour, and the body white, which is found in the earth entwined around the roots of esculent vegetables. This is certainly the larva of the Chafer. Did Agricola receive this information from modern Greeks, and is the word Spondyle still employed by them to denote the white worm?

If the Spondyle of Pliny is the same as that of Aristotle, it follows that the latter naturalist, who designated a perfect insect by this name, was acquainted with its metamorphosis; which will not appear surprising if we remember that Aristotle, as I have already observed, has exceedingly well described the metamorphosis of the Cabbage Butterfly, and that after that description he generalizes the fact, and remarks that the

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* Aristotle and Pliny. See p. 179, *antea.*

† Aldrovandus, *De Insectis*: Frankfort, 1618, p. 225.

greater number of insects come from a worm (*scolex*): "the whole worm grows larger," he says, "and becomes an articulated animal*."

Aristotle has well observed that the Spiders, Grasshoppers, and Crickets are not produced from worms, but from animals similar to themselves. These ideas upon the metamorphosis of insects are very exact, and though Aristotle blends with them a few errors, which it is unnecessary to consider here, they afford proof of the perseverance with which he pursued his observations, and the surprising skill which he possessed for generalizing acquired facts, and for discovering and predicting those not previously observed.

It must not be forgotten that it is in relation to the manner in which coition is effected in insects that Aristotle names the Spondyle; and the Chafer is precisely one of those insects which present themselves most frequently to our notice in coition.

From the text of Pliny and the assertion of Agricola, it appears that among the Latins, and the Greeks of the Lower Empire the name of Spondyle has been retained to denote the larva of the large species of Chafer, with the metamorphosis of which they were unacquainted. That an insect so common as the Chafer, and which acts a part so important to agriculture by the mischief which it occasions, even in the state of a perfect insect, to the leaves of plants and trees, was known to the Latins as well as to the Greeks, cannot be doubted; but we are ignorant whether they gave it a particular name, or whether they included it under the general names of *Scarabæus* and *Cantharis*, so often employed by them to denote all kinds of Coleoptera.

Fabricius, who has detached the Chafers from the genus *Scarabæus* of Linnæus, has given to this genus the name of *Melolontha*, which the Swedish naturalist had assigned as the specific name of the most common species. This name is borrowed from Aristotle, who employs it in the same manner as those of *Cantharis* and *Carabus*, to denote various species of *Scarabæi*, which in our natural systems belong to very different families or to very dissimilar genera. It was from the opinion of the learned of the time of Aldrovandus†, and adopted by Bochart‡, that Linnæus made the Melolontha of Aristotle our common Cockchafer; but, as Latreille has well observed§, from a comparison of the texts of Suidas, Pollux, and the scholiast on Aristophanes, it appears that the name Melolontha was given, among the Greeks, to insects of brilliant colours, a description which does not apply to the common Cockchafer.

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* Aristotle, book v. chap. 19. vol. i. pp. 286 and 287; book i. chap. 4. No. 1. and books v., xii., and xvii. of Schneider's edition, 8vo, 1811, vol. ii. chap. 17. (*vulgò* 19. Scaliger 18), vol. ii. p. 207.

† Aldrovandus, *De Animalibus Insectis*, p. 17.

‡ Bochart, *Hierozoicon*, part ii. book iv. chap. 2.

§ See Latreille's Memoir upon the Insects painted or sculptured upon the ancient Monuments of Egypt, in the *Mémoires sur divers Sujets*, 8vo.

Aristophanes, in the comedy of the Clouds, puts these words into the mouth of Socrates when speaking to Strepsiades: "Let go your thought, like the Melolontha which is launched into the air with a thread around its foot." The ancient scholiast remarks that this Melolontha is an insect of a golden colour, which children hold with a thread and cause to fly*. We know that in modern Greece the children at the present day attach a thread to the foot of the beautiful gold-coloured insect which naturalists call the *Cetonia fastuosa*, which is not scarce in that country, where the children amuse themselves with it in the same manner as those of our climates do with the common Cockchafer; the name of Melolontha should therefore have been applied to the genus *Cetonia*, not to the genus Chafer.

An interesting question in archæology here arises, in connexion with the exact interpretation of a passage of Pliny, which is well worthy of attention. The Roman naturalist, speaking of the different species of amulets used in his time to cure the quartan ague, says that three sorts of Scarabæi are employed for this purpose. "The first," he says, "is the Scarabæus which rolls pills, *qui pilas volvit*, and in consideration of which the Scarabæi are placed among the gods by a great part of Egypt." This circumstance enables us to distinguish, without any doubt, two or three insects of the family of the *Coprophagi*, the *Ateuchus sacer* of Fabricius (*Scarabæus sacer* of Linnæus), or the *Ateuchus laticollis*, and the *Ateuchus Ægyptiorum*, brought from Nubia by M. Caillaud, and recently described by M. Latreille†, who considers it exclusively as the Sacred Scarabæus so often sculptured by the Egyptians upon their monuments, and separately in hard stones of various kinds. But I think that he is mistaken; for I have recently examined all the Scarabæi of Ancient Egypt, sculptured separately, which are in the Bibliothèque du Roi, where an individual of the *Ateuchus Ægyptiorum*, presented by M. Caillaud, is also preserved, and I am convinced that, among the Egyptian stones representing Scarabæi with smooth elytra, a certain number have been sculptured from the *Ateuchus sacer* of Fabricius, and the others (a smaller number) from the *Ateuchus laticollis*; but all those stones which have the elytra striated, or with ribs and longitudinal furrows, have the *Ateuchus Ægyptiorum* of M. Caillaud for their type. Thus the name *Scarabæus*, of the Egyptians, is applicable to three different species, closely allied to each other certainly, and having probably similar manners and habits, but which, notwithstanding, it is easy to distinguish in the sculptured monuments by unequivocal characters‡. The *Ateuchus sacer*, which is black,

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* See Camus, Notes upon Aristotle's *Hist. Anim.*, 4to, vol. ii. p. 478.

† Caillaud, *Voyage à Méroë et au Fleuve Blanc*, p. 172, *Atlas d'Hist. Nat. et d'Antiq.*, pl. 58: Latreille in Cuvier's *Règne Anim.*, vol. iv. p. 533.

‡ Compare Olivier, *Coléopt.*, vol. i. No. 3. p. 150. No. 183. pl. 8. fig. 59. var. B. The pretended var. A. is a different insect; it has a clypeus between

appears to be much more common than the *Ateuchus Ægyptiorum*, which is of a golden green colour, and must be that imitated by the artists of Lower Egypt; while those of Upper Egypt have chosen the *Ateuchus Ægyptiorum* for their model. M. Caillaud found this insect in Sennaar, but not in Egypt. He however discovered the elytra and other remains of them in the mummy-cases in Egypt, which seems to prove that this insect has existed, and perhaps still exists, in that country. As Aristotle and Aristophanes employ the word *Cantharis* to denote the Sacred Scarabæus, I infer that these two authors had in view the *Ateuchus Ægyptiorum* of M. Caillaud.

This first species of Scarabæus of which Pliny speaks is also, according to the view we have taken, the first of the three species of these insects which are mentioned by Horapollo as being held in great veneration by the Egyptians.

The second species of Scarabæus used as an amulet for the cure of the quartan ague, spoken of by Pliny, is employed, he says, by the magicians, but that care must be taken to collect these insects with the left hand. This species has small reflected horns, *cui sunt cornicula reflexa.*

From this indication, Hardouin, and other commentators following him, refer this insect to the *Lucani.* They are mistaken.

The Lucanus, vulgarly called the Stag-Beetle, is one of those insects which Pliny has most correctly described*; and naturalists have therefore allowed it to retain the name which he assigned to it. He gives a good description of its long, indented, and bifurcated horns, which he says are suspended around the neck of children to preserve them from the bite of venomous beasts: "*Cornua prælonga bisulcis dentata forcipibus in cacumine.*" This will not agree with the little recurved horns of the other species of Scarabæus with which it has been identified. This second species of the Scarabæus of Pliny appears to be the second species described by Horapollo; according to this author it has two horns, and the form of the bull; it is sacred to the moon. We are disposed to think that this is the large species of Copris (*Bousier*), with two horns, which M. Savigny brought from Egypt, and named *Midas.* It is sculptured in the temple of Karnak, and according to the observation of Latreille appears to belong to the genus *Onitis,* recently separated from the Coprophagi†.

M. Millin, in his account of the engraved Egyptian stones in the Bibliothèque du Roi, says that an engraving of a sculptured Scarabæus may be seen in the cabinet of antiques of St. Geneviève, which he considers as the *Scarabæus Mimas.* In this he is mistaken, for the *Scarabæus Mimas* is a species peculiar to America; but the

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the elytra, which are also of a different form. Schœnherr, *Synonymia Insect.*, vol. i. p. 18; Caillaud, *Voyage à Méroë et au Fleuve Blanc,* vol. iv. p. 272, *Atlas d'Hist. Nat. et d'Antiq.* ii. 58. p. 10.

* Pliny, *Hist. Nat.*, book ii. chap. 34.

† Latreille, *Mémoires,* pp. 148, 153. Compare *Descript. de l'Egypte,* vol. iii. p. 34.

error of this estimable archæologist is a slight one, since the *Scarabæus Mimas* is a *Copris* as well as the *Midas* of Egypt, which it resembles even almost to its colours. There is therefore reason to think that the Egyptian stone mentioned by M. Millin represented the *Copris Midas* which M. Savigny discovered in Egypt.

The third species of Scarabæus employed, according to Pliny, as an amulet against the effects of the quartan ague, was named the Fuller (*Fullo*). This insect was spotted with white; and the mode of employing it was to divide it into two portions, one of which was affixed to each arm, while the two other species of insects of which we have treated were attached only to the left arm. "*Tertium, qui vocatur Fullo, albis guttis, dissectum utrique lacerto adligant, cætera sinistro.*" All Pliny's commentators are silent upon this remarkable passage, and upon the insect named *Fullo* by the Romans; but naturalists have not been equally careless. Mouffet, whose work appeared after his death in 1634, describing the largest species of Chafer of our climates, which is nearly an inch and a half in length, and is distinguished with facility by the brilliant white spots upon its corselet and elytra, combats the opinion of those who consider the *Fullo* of Pliny as a *Copris* or a *Forficula*, and supposes that by this name the Roman naturalist intended to denote the large species of Chafer with white spots which he (Mouffet) had just described*. Ray, whose History of Insects was published in 1710, is of the same opinion†; and lastly, M. Schœnherr, in his laborious work specially devoted to the synonymy of insects, cites Pliny for his *Melolontha Fullo* ‡.

It is with regret that I contest an opinion apparently so well established by the suffrages of so many eminent naturalists; but my own observations are opposed to it. I have examined a great number of antique stones upon which insects were sculptured or engraved, some of which have perhaps been used as amulets, for they were pierced in a manner adapted for suspension at the neck, and they all represented either Coprophagi or *Cetoniæ* §. Not one of them can belong to a species of Chafer, which may be easily distinguished from the insects previously mentioned by a more lengthened form. The fact is the same with regard to the obelisks, and all the monuments of Egypt of which drawings have been published. I here speak only of the Scarabæi or Coleoptera, and not of the species of Bee or Wasp sculptured upon the obelisks at Luxor‖. M. Latreille, who has been engaged in a similar examination, has arrived at the same conclusion.

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* Mouffet, *Insect., sive minimorum Animalium Theatrum*, 1634, folio, p. 160.

† J. Ray, *Hist. Insect.*, 1710, 4to, p. 93.

‡ C. J. Schœnherr, *Synonymia Insect.*, part iii. Upsalia, 1817, 8vo, p. 164.

§ There are *Coprides*, but no *Cetoniæ* among the Scarabæi at the Bibliothèque du Roi, but I have seen many of the latter in several other collections.

‖ [What insect was really intended to be represented by the sculptures here

It appears therefore that the *Melolontha Fullo* of Pliny should be sought for among the *Coprides*, (*Bousiers*,) or among the *Cetoniæ*, and not among the Chafers.

Pliny says that the green Scarabæus possesses the property of rendering the vision more penetrating, and that engravers upon gems rest their eyes by gazing upon these insects. "*Scarabæi viridis natura contuentium visum exacuit, itaque gemmarum sculptores contuitu eorum acquiescunt**." Marcellus Empiricus, copying Pliny, relates the same fact, but he gives us the additional information that this Scarabæus is of the colour of the emerald, "*Scarabæus coloris smaragdini*." This definition is exactly suitable to the *Cetonia fastuosa*, and to the *Cetonia aurata*, particularly to the former. These two species are of a beautiful golden or emerald green, but the *Cetonia aurata* is distinguished from the other by white spots upon the elytra ("*albis guttis*"); it is nine lines in length, and is frequently found in gardens, upon roses and other flowers. The great Chafer with white spots, the *Melolontha Fullo* of modern naturalists, is, on the contrary, rare, and is found only upon downs and in the vicinity of the sea.

From all these circumstances I conclude that the *Cetonia aurata* was the object of the superstition of which Pliny speaks, and is the insect to which he gives the name of *Fullo*.

To recapitulate: the word Spondyle, or Sphondyle, in the works of Aristotle, denotes the Cockchafer, both the perfect insect and its larva.

As employed by Pliny, who was unacquainted with the metamorphosis of the Cockchafer, Spondyle denotes only the larva of this insect, or the white worm, taken for a small serpent, which in the time of Agricola, in the sixteenth century, was still known to the Greeks by this name of Spondyle.

The "*Scarabæus qui pilas volvit*" of Pliny, which cured the quartan ague and was adored by the Egyptians, is the *Scarabæus sacer* of Linnæus, the *Ateuchus sacer* and *Ateuchus laticollis* of Fabricius, and the *Ateuchus ægyptiacus* of Latreille and Caillaud.

The true Scarabæus of Horapollo, the wings of which form rays when extended, is also the same insect.

The Sacred Scarabæus, named *Cantharis* in Aristotle and Aristophanes, is the *Ateuchus ægyptiacus*.

The "*Scarabæus cui sunt cornicula reflexa*" of Pliny is the *Ateuchus Midas*, the *Copris Midas*, common in Egypt and brought from that country by M. Savigny.

The Scarabæus with two horns, sacred to the moon, of Horapollo, is also the *Copris Midas*.

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alluded to is still we believe a subject of discussion. See London and Edinburgh Philosophical Magazine, vol. iv. p. 170.—Edit.]

* Pliny, *Hist. Nat.*, book xxix. chap. 38. vol. viii. p. 270.

The *Melolontha* of Aristotle and the Greek authors, which served as a toy for children, is the *Cetonia fastuosa.*

The *Scarabæus viridis* of Pliny, which the engravers upon gems loved to contemplate, is also the *Cetonia fastuosa.*

The *Scarabæus Fullo albis guttis* of Pliny is the *Cetonia aurata*, the *Scarabæus auratus* of Linnæus, which has white spots upon the elytra.

As it is proved that the Spondyle of Aristotle and Pliny is the Chafer, we have been right in directing our attention to this word, for the Chafer destroys the leaves of the vine, as well as of all other plants. This genus includes a species, smaller than the common one, which entomologists have named the Chafer of the vine, *Melolontha Vitis,* because it is frequently found upon that plant with the Chafer of Frisch, *Melolontha Frischii,* which is perhaps only one of its varieties*; but this insect is found almost as frequently upon the leaves of the willow and the rose-tree as upon those of the vine; and it is not one of those of which our vine-dressers and cultivators particularly complain, nor did it attract the attention of agriculturists among the ancients.

Before concluding my observations upon the word Spondyle, I must not forget to remark that Fabricius has employed it to designate a genus of Coleoptera which he has formed in the family of the *Prioni*, and named *Spondylis buprestoïdes*, the *Attelabus buprestoïdes* of Linnæus; but this insect, the larva of which lives in the wood of green trees, has no relation to the Spondyle of the ancients, the larva of which attacked the roots of young or annual plants. It was not M. Fabricius's intention, in selecting this name, to assume that any relation existed between them: but what I have said in my preliminary reflections may be applied to this case, and relieves me from the necessity of extending my observations upon this subject.

III. *Joulos, or Julus.—The Juli.*—There is still less reason for the appearance of the name of *Joulos* among those given to insects injurious to the vine than for that of Spondyle, although Suidas has said that the Joulos was a worm of the vine; but this lexicographer is the only one who has so ill defined the insect of which the ancients have spoken under the name of Joulos. From a comparison of their texts, it appears that the Joulos is an apterous or wingless insect, possessing a great number of feet; that it has the lengthened form of a worm; moves in a serpentine manner; coils itself up when touched; and is found in moist places. Modern naturalists cannot have been mistaken with regard to this insect, for which they have retained the ancient name. The name

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* Walckenaer, *Faune Parisienne*, vol. i. p. 185. Olivier, *Entomologie*, genus *Hanneton*, No. 39. pl. 2. fig. 12. *a*, *b*, *c*. p. 34. vol. i. Schœnherr, *Synonymia Insect.*, vol. i. part iii. p. 193.

of *Julus*, given to a genus of insects by the moderns, corresponds exactly to the *Julus* or *Joulos* of the ancients; especially if the modern signification of the word be restrained to the genus *Julus* of Leach*, as defined in his excellent work upon the polypodous insects, excluding the *Polydesmata* and other genera, which have with propriety been removed from it. The *Juli* which the ancients had in view were probably the terrestrial *Julus* and the *Julus sabulosus* of modern entomologists, and the common *Julus* of M. Soavi, erroneously confounded with the former two. These insects are found on the earth under stones; they feed upon the leaves and fruits which fall upon the ground and are there decomposed; but they neither injure the vine nor any other plant. As they are found under the shadow of the vine, as well as in all other dark and humid places, the injuries arising from another cause have been attributed to them.

IV. *Biurus.—Grillo-talpa.—Mole-cricket.*—The word which, after Spondyle and Julios, has the least relation to our subject of those which we have passed in review is *Biurus*. I find it only in an isolated passage of Cicero, cited by Pliny, in which it is said that this animal destroys the vines of Campania. Thus, it is not mentioned as an enemy to the vine, correctly speaking, but as injuring the vines of Campania in particular, by its rapid multiplication. Perhaps also in this passage, which Pliny only quotes incidentally, Cicero was speaking of a particular case in which the *Biuri* were seen to be injurious to the new plantations of vines in Campania, though they would be incapable of injuring them when the roots had acquired sufficient hardness to resist their attacks. Whatever be the fact, the etymology of the word *Bi-Uros*, which, as we have seen, implies an insect armed at its posterior extremity with a double tail, directs us to the Mole-cricket and the larger species of locusts (*Sauterelles*), the only insects so formed that can answer to the particulars specified, from their size and the destruction which they cause, and of ravaging vine plantations extending over a whole country. But the locust having been well known to the Latins under the name of *Locusta*, and to the Greeks under that of *Acris*†, it follows that the name of *Biurus* is applicable only to the Mole-cricket. The probability of this is increased by these circumstances: that this insect is the largest which is known in our parts of Europe, it being not less than an inch and half in length; that it is one of the most singular in its formation, and one of the most destructive; that it cannot be recognised in any of the descriptions of insects transmitted to us by the ancients; and lastly, that, in all the writings which they have left us, the name *Biurus* is the only one that can be applied to it.

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* Leach, *Zoological Miscellany*, 1817, 8vo, vol. iii. p. 32 to 48.

† Vulgate and Septuagint versions of the Bible. Aldrovandus, *De Insectis*, p. 160.

Latreille says that the Mole-cricket was unknown before the time of Mouffet. This is not the case: it is true that Mouffet is the first who published a good representation of it;—the first who gave it the name of Mole-cricket, or rather Cricket-mole, *Grillo-talpa*, a description which applies to it alone*. "*Liceat*," says he, "*hic quæso nobis præ nominum inopia onomatopoiein;*" and he properly rejects the names of Sphondyle and Buprestis, which had been given to it; but this rejection proves that the Mole-cricket had previously attracted and engaged the attention of naturalists. In fact, Aldrovandus had given a good description of this insect before Mouffet, and a representation of it which, though bad, may still be recognised: he named it *Talpa Ferrantis*, because this insect had been previously named Mole, and Ferrante Imperato had figured it: *Neapolitanus diligentissimus aromatarius in naturali sua historia*, book xxviii., says Aldrovandus. Mouffet is therefore indebted to Ferrante for half the name which he gave this insect; for, that he was acquainted with his work is evident from his having borrowed from it the figure which he published of the Tarantula Spider. Ferrante's work was printed in Italian after his death in 1599, and translated into Latin. The original edition is scarce†, and no naturalist of late times, that I am aware of, even including Linnæus, was acquainted with it; at least not one of them has quoted it. They all think that they have done much in ascending to old Aldrovandus; but we have just shown that the history of the Mole-cricket commences before him and Mouffet, and even before Ferrante; for if the application which we have made of the word *Biurus* be, as it appears, exact, we must refer to ancient times for the first mention of this insect.

The Mole-cricket causes great devastation, especially in the southern parts of Europe; it digs holes and constructs subterranean galleries, and cuts and detaches the roots of plants by means of its fore feet, which are shaped like saws; but this it does solely to provide a habitation for its posterity, for it neither eats plants nor their roots, but feeds only upon insects, and destroys a great number of the injurious ones‡. The havoc caused by the Mole-cricket (*Courtillière*) has probably been confounded with the devastation committed by the white worm of the Cockchafer, for, according to a recent dictionary of agriculture§, the name of *Courterolle* has been given to both in several of the cantons of France||.

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* Mouffet, *Insect. Theatr.*, p. 104. chap. 24.

† Ferrante Imperato, *Del Historia Naturale*, libri 28, Naples, 1599, p. 787. *Talpa insecto*. This representation is better than the one given by Aldrovandus.

‡ *Acheta Grillo-Talpa*, Fabr., *System. Entom.*, vol. ii. p. 28. No. 1. Walckenaer, *Faun. Paris.*, vol. ii. p. 282.

§ Baron de Morogue, *Cours complet d'Agriculture*, 1834, 8vo, vol. vii. p. 349, at the word COURTEROLLE.

|| [An elaborate memoir "*On the Anatomy of the Mole-cricket*," by Dr. Kidd, will be found in the Philosophical Magazine, 1st Series, vol. lxvi. p. 401.—EDIT.]

V. *Gaza.*—*Saddle Locust:*—Locusta ephippiger.—*Wingless Locust:*—Locusta aptera.—*Nymph Locust:*—Locusta Puppa.—It will be recollected that from our examination of the name *Gaza*, employed by the prophets Amos and Joel, (p. 174,) we ascertained that it was used as the name of an insect eminently destructive, not only of the vine but of all kinds of plants; and that its ravages were succeeded by those of several species of locusts, which completed the destruction of all that this formidable insect had left undevoured. The word *Gaza* is rendered by caterpillar in the Septuagint and Vulgate, and by creeping, that is, apterous or wingless, locust in the Chaldee version. If it be remembered that in the days of Ptolemy the Jews of Egypt, to whom we owe the Greek translation of the Sacred Books, were very imperfectly acquainted with Hebrew, which was to them a dead language; that St. Jerome, whose translation has served as a basis for the Vulgate, was still more ignorant with regard to the designation of material objects, it will be found that the Chaldee version is on these accounts of higher authority than the two other versions: and if the works of Rosenmüller and Oedmann*, who have discussed this point of criticism with equal sagacity and erudition, be consulted, we shall be convinced, notwithstanding the opinion to the contrary of Bochart and Michaëlis, that the four different names employed by Amos and Joel as the names of insects, all denote locusts. The observations of the judicious traveller Shaw remove all doubt upon the subject. He informs us that in Africa, in the months of March and April, it frequently happens that the locusts driven by the south wind obscure the sun, and augment in density until the middle of May, and that after completing their ravages they remove to lay their eggs, and diminish in number. Then follow, after the interval of a few days, some smaller species, moving like the former in troops, which are in turn succeeded by one or two other species, which complete the devastation.

M. Oedmann thought that completely to vindicate the Chaldee text it was necessary to suppose the *Gaza* to be a locust without wings or elytra, not yet come to its full growth, which was mistaken by the Hebrews for a perfect insect and distinguished by a particular name. But the orientals were too well acquainted with locusts, which from all antiquity had supplied them with food, to allow of our imagining that the Hebrews could have committed such an error. Neither is it necessary to suppose it. We now know several species of creeping locusts which perfectly correspond to the creeping locust of the Chaldee version; a fact of which Oedmann appears to have been ignorant.

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* Rosenmüller, *Handbuch der Biblische alterthume Kunde*, Leipsic, 4th Band, 1831, 8vo, pp. 386 and 388. Oedmann, *Vermischte Sammlungen aus der Naturkunde*, aus dem Schwedischen, uebersetz. von D. Groning, 1787, 12mo, 2nd Heft, pp. 116, 117.

There is in particular one species which has a deeply excavated corselet raised at the back like a saddle; this corselet hides the sonorous and vaulted elytra, which are very short, and do not serve for flying. These locusts resemble nymphæ, but have however arrived at their perfect state, and propagate their kind. This species has been named the *Locusta ephippiger*. There are even other species of which the females at least are without wings or elytra, and which perfectly resemble the larva of the Locust; such are the species named *Locusta aptera* and *Locusta Puppa* by Fabricius. But I am more inclined to consider the Saddle-locust, or the *Locusta ephippiger*, as the *Gaza* of the Bible, than either of the two other species that I have mentioned. Of all the species of creeping locusts the *ephippiger* is that which I have most frequently found upon the vine, though never in sufficient abundance to produce much injury; and it cannot be classed with the true insects of the vine, neither is it mentioned as such in Scripture.

VI. *Cantharis of the Geoponics.—Ninth Cantharis of Aldrovandus.* —Rhynchites Bacchus, *or* Rhynchites Betuleti, *or Attelabus of the Vine.* —Becmar-Diableau.—Lisette, *and* Green Velvet (*Velours vert*) *of the Vine-dressers.—The Coleoptera or Scarabæi which destroy the Vine, and do not answer to the Cantharides of the Geoponics.*—Lethrus Cephalotes.—*Gray Curculiones* (Charansons).—The ancient authors give the name of *Cantharis* to the insects which they employed when pounded as an ingredient of the liniment or unguent with which they anointed the vine to protect it from injurious insects; but it is in the Geoponics alone, when treating of this employment of Cantharides, that we are informed that these insects were engendered in the vine, and were destructive to it; and the author or authors of this compilation only give the recipe of Cantharides macerated in oil as a remedy for the disasters which these insects themselves produce*. We have seen that the word *Cantharis* was employed by the Greeks, as well as by the Latins, as the designation of the Coleoptera or Scarabæi in general; that this name was often applied to the brilliantly-coloured Coleoptera, or those possessing corrosive or vesicating properties; and that it was also used as the name of insects, whether of large or small dimensions, which were rendered remarkable by their destructive effects. Of the first we have noticed the *Mylabris* of the endive, the *Mylabris Cichorii* of modern naturalists, so well described by Dioscorides; and the *Lytta* or *Meloë vesicatoria*, the Cantharides of our apothecaries†. Among the second, or those which are very small, is the *Scarabæus parvus Cantharis dictus* of Pliny which infests corn, which is the *Curculio grana-*

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* Latreille in Cuvier's *Règne Anim.*, vol. v. p. 63. Oliv. *Coléop.* iii. p. 47. pl. 1. Schœnherr, *Synonymia*, 1817, 8vo, p. 31. *Mylabris*, vol. i. part iii. p. 31. Oliv., *Ent.* iii. 47, 7. vol. i. fig. *b*, *c*.

† Latreille in Cuvier, vol. v. p. 67. Schœnherr, *Synonymia*, vol. i. p. 20.

rius or *Calandra granarius* of our modern naturalists; the *Curculio frumentarius* of Linnæus, the *Apion frumentarius* of Schœnherr and Latreille. The former is of a dark fulvous colour; the latter is red and brilliant, and is, I think, that of which Pliny speaks, for it attacks wheat, while the other principally infests oats*. These indications leave us in great uncertainty relative to the *Cantharides* of the Geoponics. But as it was undoubtedly their corrosive or vesicating properties which caused the Cantharis of the ancients to be employed in the liniment which was destined to destroy other insects, it is probable that their Cantharides of the vine were insects of the same nature, or other insects which, from the resemblance of their colour, were confounded with them. Now, as no Coleopterous insect, or Scarabæus, has vesicating properties, as no *Mylabris*, *Lytta*, *Meloë*, or *Cantharis* lives upon the vine, it is evident that the insect of which we are in search must be found among those which from their colour may be confounded or compared with them, especially with the yellow-banded Mylabris of the endive and the brilliant green Cantharis of apothecaries, for we know that these species were employed by the ancients in medicine and agriculture.

We will now pass in review all the Coleoptera or Scarabæi which injure the vine, and that which corresponds the best to these indications must be the *Cantharis* of the vine of the Geoponics. The largest of all these Coleoptera or Scarabæi is the *Lethrus Cephalotes*, which gnaws the young shoots of shrubs in general, but particularly those of the vine, and carries them into its hole.† But this species appears peculiar to Hungary, where it is named *Schneider*, cutter; it is also frequently found in the western parts of Russia; but neither our cultivators nor those of Italy make any complaint of it. I do not find anything relative to this insect in the ancient authors, and if they were acquainted with it, it must have been comprehended by them under the general name of *Scarabæus*.

It is different with the Curculiones (*Charansons*), of which we have several species which infest the vine. The one which I have found most frequently upon this plant is the *Curculio picipes* of Fabricius, which is perhaps the same species as the *Curculio Corruptor* of Host, and the *Curculio Vastator* of Marsham‡. The grey Curculiones, with globular bodies, devour the shoots of the vine as soon as they come out of the bud. They prevent its development and the production of grapes;

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* Schœnherr, *Synonymia Curculionidum*, vol. i. p. 283. No. 75, genus *Apion*. Walckenaer, *Faun. Paris.*, vol. i. p. 237. No. 15. Latreille, *Gener. Crust. et Insect.*, vol. ii. pp. 249 and 271. *Ibid.*, Cuvier, vol. v. p. 88. Oliv., *Entom.* vol. v. 83, 16, 196.

† Latreille, *Gener. Crust. et Insect.*, vol. ii. p. 95. *Ibid.*, Cuvier, vol. iv. p. 542. Fischer, *Entom. de la Russie*, p. 133. xiii. Kirby, *Introd. to Entom.*, vol. i. p. 204. *Ann. des Scien. Nat.*, vol. i. p. 221.

‡ Walckenaer, *Faun. Paris.*, vol. i. p. 249. Fabricius, *Syst. Eleuth.*, vol. ii. p. 540. No. 201. Marsham, *Entomologia Britannica*, vol. i. p. 300. No. 180.

but pear and apple trees are more exposed to their attacks than the vine, and they are more prejudicial in Germany and the South than in our climates.

The Eumolpus of the vine, vulgarly called the *Coupe-bourgeon*, is a third species of the Coleoptera still more destructive than the two which we have just mentioned; but this insect, of which we shall presently treat more at length, like the two preceding ones, possesses but little brilliancy of colour.

It appears then that among all the Coleoptera or Scarabæi which infest the vine, there are only two species closely allied, and which must have been considered as one by the ancients, as they have long been by the moderns, which appear to correspond by their colour to the particulars which we have obtained in our examination of the ancient texts relative to the word *Cantharis*. These two species are the *Rhynchites Betuleti* and the *Rhynchites Bacchus* of modern naturalists; the Attelabus of the vine, or *Attelabus Bacchus*, and the Attelabus of the birch, of their predecessors. These two species, considered as one by the vine-dressers, have received from them in the different dialects and provinces of France, and even in the different districts of the same province, the names of *Becmare*, *Urbec*, *Urbère* or *Urbée*, *Diableau*, *Beche*, *Lisette*, *Velours vert*, *Destraux*, and perhaps others of which we are ignorant. The *Rhynchites Betuleti*[*] is of a brilliant silky green colour, or of an equally brilliant and silky violet blue. The *Rhynchites Bacchus*[†] is of a golden violet purple or of a golden green mixed with purple. These insects cut the petioles of the leaves to cause them to wither and soften so as to allow of their being rolled with greater facility; this they effect with great dexterity, leaving a cavity in which they place their eggs, and thus injure greatly the plants to which they attach themselves. The *Rhynchites Bacchus*[‡] prefers the leaves of the vine and cherry tree; the *Rhynch. Betuleti* those of the vine and the white birch. In the environs of Paris I have most frequently found the *R. Bacchus* upon the vine; but it was the *R. Betuleti* which committed the extensive ravages among the vines of Burgundy about fifteen years ago. M. Silbermann told me, at Strasburg, that the *R. Betuleti* is more destructive than any other insect to the vines of Alsace and the banks of the Rhine, and that the *R. Bacchus* is seldom found there. According to the observations of this able entomologist, the *R. Betuleti* is

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[*] Walckenaer, *Faun. Paris.*, vol. i. p. 235. *Attelabus Betulæ.* Schœnherr, *Synonymia Insect.*, vol. i. p. 222. Panzer, *Faun. Insect. Germ.* xx. No. 6.

[†] Schœnherr, *Gen. et Spec. Curculionidum*, *Rhynchites Bacchus*, vol. i. p. 219. No. 15. Latreille, *Hist. Nat. des Ins.*, vol. ii. p. 85. *Attelabus Bacchus.* Panzer, *Faun. Ins. Germ.*, fasc. 20. No. 5. *Charanson Cramoisi* of Geoff. *Attelabe cuivré* of Olivier.

[‡] Kirby, *Introd. to Entom.*, vol. i. p. 199.

seen in that country as a perfect insect upon the leaves of the vine towards the end of August. The larva rolls the leaf to hide itself, and attacks the young grape, but not the buds, being hatched too late. Schranck, in his *Fauna Boïca**, has placed these two insects in a particular genus, to which he gives the name of *Involvulus*; but the *Involvulus* of the ancients, as we shall presently show, does not belong to the class Coleoptera, but to the Lepidoptera; and I may remark that the genus *Involvulus* of Schranck, being badly constituted, has not been adopted by any naturalist. Though it contains but few species, some of them are distributed by M. Schœnherr among his *Apoderi*, one among his *Attelabi*, and a third among his *Rhynchites*. Aldrovandus was well acquainted with the *Rhynchites Bacchus*; and I am surprised that no naturalist has quoted this venerable father of natural history in modern Europe upon the subject of this small but formidable insect. He places it among the Cantharides, to which he devotes a chapter, thus separating them from the true Scarabæi which occupy another chapter. The following is the description which he gives of this Curculio: "*Nonus numerus significat Convolvulum,* Ιπα *Græcis,* Tagliadizzo *vulgo apud Italos agricolas, corpore cœruleo, pedibus obscure lutescentibus, in vite repertum, ac folia ejus depopulantem. Nascitur ex ovis bombicum ovis similibus magnitudine, colore rubicundis. Hic cum parere vult multa cumulat convolvitque folia (unde forte a Latinis id nominis datum), atque in his sua ova reponit.*" Thus the name of *Tagliadizzo*,—cutter,—given to it by the vine-dressers of Italy; its bluish colours; the injury done to the leaves of the vine, which the insect rolls up and in which it deposits its eggs, all mark with certainty the synonymy of our *Rhynchites Betuleti* or *R. Bacchus* with the ninth Cantharis of Aldrovandus†. But as to the identity of this insect with the *Ips* of the Greeks, and the *Convolvulus* of the Latin authors, which Aldrovandus attempts to establish, the continuation of our researches will prove that it must be rejected.

VII. *Ips.—Iks.—Volucra.—Volvox.*—Eumolpus Vitis.—*Eumolpus of the Vine.—Coupe-bourgeons.—Tête-cache.—Bêche.—Lisette.—Gribouris de la Vigne.*—After having treated of the Cantharides, Aldrovandus devotes an entire chapter to the *Ips* of the Greeks, to confirm his assertion in the preceding chapter that this insect is the Tagliadizzo of the cultivators of Italy; but he remarks that he had only found this insect upon the vine, though the ancient authors say that it preys also upon horn. If Aldrovandus was wrong in maintaining that the *Ips* of the Greeks was the *Convolvulus* of the Latins, he was right in thinking that it belonged to the Coleoptera, and was one of those which the Italian agriculturists class among the Tagliadizzi, or cutters.

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* Schranck, *Fauna Boïca*, vol. i. p. 474. No. 498.

† Aldrovandus, *De Anim. Insect.*, chap. iv. 1638, folio, p. 472.

It appears evident, as has been advanced by Valckenaer, Bochart, and the most learned philologists, that the *Iks* of certain authors, an insect which infests the vine, is the same word as the *Ips* employed by other authors as the name of an insect which also infests the vine, and that *Ips*, *Ipes*, *Iks*, *Ikes*, are only differences of dialect. This agreed, it is evident from our critical examination, that the conclusion to be formed from the information we receive from the Greek authors, including the grammarians and lexicographers of the lower ages, is, that *Ips* is employed as the name of an insect which preys upon horn and meat, and also of one which infests the vine, of which it devours the buds, either in the state of larva or as the perfect insect. We learn from this that the name of *Ips* or *Iks* was applied by the ancients to two or three different species of insects or larvæ of insects. But since the ancients confounded these species, and assigned them but one name, there must necessarily be an analogy between them. There is only one species of the larvæ of the Coleoptera or Scarabæi possessing *trophi*, or organs for manducation, sufficiently hard to pierce horn. The Ips of Homer and of St. John Chrysostom belongs therefore to the Coleoptera, consequently the *Ips* of meat and of the vine must also belong to that class. As we are treating of an insect which preys upon horn and meat, naturalists know that it must belong to Linnæus's tribe of Dermestes, the larvæ of which are so formidable to their collections. They are not ignorant that these insects are found in warehouses of furs, in offices, pantries, and all places which receive animal matters, and that they spare neither horn nor feathers; but our knowledge of them is not sufficient to determine to what genus of modern entomologists those *Dermestes* belong which prey upon old goat's horn, particularly upon that of the Ægagrus, of which the bow of Ulysses was formed, and which is particularly mentioned by Homer. We are well acquainted only with the metamorphoses of the *Dermestes lardarius*, and the *Dermestes Pellio*, the Dermestes of bacon and furs. These insects belong to the numerous family of the *Nitidulariæ* of Latreille*. Degeer† long ago separated from the Dermestes a genus to which he judiciously gave the name of *Ips*; but this name has been since given to genera very different to that which he had created, though they also were formed from the numerous family of the Dermestes. It might possibly be the same larva which infested horn and meat, as is asserted by the grammarian published by Boissonade; it is also possible that the ancients confounded the larvæ of two affinal but different genera. But

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* Latreille, in Cuvier's *Tab. du Règne Animal*, vol. iv. p. 503. Schœnherr, *Synonymia Insect.*, vol. i. part ii. p. 236. No. 25. Walckenaer, *Faun. Paris.*, vol. i. p. 124. No. 2. Panzer, *Faun. Insect. Germ.* lxxxix. 12. Fabr., *Syst. Eleuth.*, vol. i. p. 422.

† Degeer, *Mém. pour servir à l'Hist. des Ins.*, vol. v. p. 190.

certainly the insect designated by the ancients as preying on horn and meat cannot be the same as that whose worm or larva feeds upon the young shoots of the vine. However, to render the same name applicable to them both, they must have belonged to the class Coleoptera, the larvæ of which could not be confounded with caterpillars, or the larvæ of Lepidoptera; the perfect insect which destroys the shoots of the vine must also resemble the Dermestes in form and dimensions. All these conditions meet in the *Eumolpus Vitis*, the Eumolpus of the vine of modern naturalists, which is one of the greatest scourges of that plant. This insect, which is of a black and blood-red colour, belongs to a genus which has been separated from the *Cryptocephali**, and is vulgarly known under the names of the Cryptocephalus (*Gribouris*) of the vine, *Bêche*, *Lisette*, and *Tête-cache*, because its head is covered by its corselet. It feeds upon the buds of the vine, or on the young shoots of that plant which still remain herbaceous, which it cuts in two and causes entirely to perish. It feeds also upon grapes. The great injuries inflicted by this insect upon the vine is an additional reason for considering it as the *Ips* of the ancients. As Strabo observes, we can imagine that the veneration in which the memory of Hercules was held in a country planted with the vine was more on account of his supposed destruction of this plague than of his victory over the Nemæan lion, and why the cultivators were so anxious to obtain and employ recipes for the destruction of these vermin. When the ancients spoke of the *Ips* or *Iks* as a worm which appeared in the spring, they had in view the larva of the Eumolpus of the vine. The larva of this insect is oval; it has six feet; its head is scaly and armed with two small maxillæ†. The insect named *Ips* or *Iks* by the Greeks, was called *Volucra* or *Volvox* by the Latins; but with this difference, that the word *Ips* and *Iks* were applied to the larva of this insect, while *Volucra* and *Volvox* were the names of the perfect insect. This is proved by the use of the word *animal*, and not *vermis*, which Pliny and Columella employ when speaking of the *Volucra* or *Volvox*; while the *Ips* is always spoken of as a worm by the Greeks. The name *Volucra* has probably been given to these larvæ in consequence of the promptitude with which they escape from the hand which endeavours to seize them, for they drop down upon the earth as soon as the leaf in which they are enveloped is touched; and the name *Volvox* is undoubtedly derived from this insect's habit of rolling itself up in leaves. Forcellini, in his dictionary, gives the Italian word *Ritoritelli* as the equivalent of the word *Volucra*; this vulgar name of an insect of the vine in Italy has evidently the same origin as *Volvox*. Nearly all the insects of the genus

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* Buchoz, *Hist. Nat. des Ins. nuisibles à l'Homme*, 1782, 12mo, p. 158 to 163.

† Latreille, *Nouv. Dict. d'Hist. Nat.*, vol. x. p. 358. He quotes Olivier, No. 96. pl. 1. fig. 1; but this figure does not represent the insect of the vine, but is a species from Brazil, the *Eumolpus ignitus*, which is a different insect.

Dermestes counterfeit death when they are touched, and this conformity of habit must have contributed to the error of the ancient authors in confounding together the *Ips* which preys upon horn and that which infests the vine. But there are still stronger reasons which prove that the *Volucra* or *Volvox* of the Latins is the same insect as the *Ips* or *Iks* of the Greeks. Pliny and Columella inform us that the *Volucra* or *Volvox* was a different insect from the *Convolvulus.* This difference between two insects which both infested the vine must necessarily have been complete and radical, since it was remarked by the ancients, who possessed so little information upon this class of animals. We shall show presently that the *Convolvulus* was one of the Lepidoptera or Butterflies; the *Volucra* or *Volvox* must belong to a totally different class. Now among insects there are only the larvæ and the insects of the Coleoptera, and the caterpillars or larvæ of the Lepidoptera, which are very injurious to the vine; the *Volucra* or *Volvox* must therefore belong to the class Coleoptera. Besides, we learn from Pliny and Columella that the *Volucra* or *Volvox* infested both the young shoots and the grapes. Pliny says, "*Volvocem animal prærodens pubescentes uvas*;" and Columella, "*Genus animalis Volucra prærodit teneras adhuc pampinas et uvas.*" These expressions apply solely and entirely to the Eumolpus of the vine and the *Ips* of the Greeks, and not to the Cantharides of the Geoponics, the *Rhynchites Bacchus* or *Betuleti*, which injures the vine by rolling up the leaves and causing them to wither, but which does not attack the fruit. Neither can they be applied, as we shall shortly see, to the various species of the caterpillars or larvæ of the Lepidoptera which attack the vine.

It is therefore proved that the *Ips* or *Iks* of the Greeks is the *Volucra* or *Volvox* of the Latins, and the Eumolpus of the vine the *Eumolpus Vitis* of modern entomologists.

VIII. *Involvulus.*—*Convolvulus.*—*Pyralis of* Bosc d'Antic.—*Vercoquin.*—Procris Vitis, *or* Procris ampelophaga.—*Vine-moth.*—*Grape-moth.*—Tortrix Heperana.—Cochylis Roserana.—From the recipes given by Pliny and Cato to prevent the multiplication of the *Convolvulus*, we learn that it was an insect eminently destructive of the vine; but as they neither give any description of it nor furnish us with any particulars respecting it, excepting that it was a different species from the *Volucra* or *Volvox*, we have no means of ascertaining whether this name applies to the same insect as is denoted by the name *Involvulus* employed by Plautus in the passage which has been quoted. In this uncertainty, the similarity of the roots and the conformity of the onomatopœia, indicative of similar habits and industry, will not allow us to separate these two words, and induce us to presume that they were employed to designate the same object, or rather that they are one name, to which are adjoined two different particles, which do not alter its signification. The description of the industry attributed by Plautus to the *Involvulus*, to the little beast, "*bestiola quæ in pampini folio intorta implicat se*," can be applied only to caterpillars or the larvæ of the Lepidoptera. The caterpillar not only coils up the leaf of the plant in which it envelops itself, like the larva of the Eumolpus or *Coupe-bourgeon*, but it attaches itself to it, and by means of silken filaments which it draws from its own body, constructs for its metamorphosis a web of silk, in which it envelops itself, "*implicat se*." The caterpillars of a whole family of Lepidoptera envelop themselves in this manner in the leaves of plants. To discover the *Involvulus* or *Convolvulus* of the ancients it is therefore only necessary to examine those insects of the numerous family of the *Phalænæ Tortrices* of which the caterpillar attacks the vine. According to the observations of Bosc, the cultivators of the South of France give the name of Vine-moth to one of the Lepidoptera which is seldom found in the environs of Paris. The caterpillar or larva of this moth attacks the grapes when they have attained half of their full growth, and it proceeds from one grape to another by means of a gallery which it constructs*. There is another species named Grape-moth†, which also devours this fruit, and commences its ravages at the same period as the former, but it seldom attacks more than one grape at a time; this species committed great depredations in the vineyards in the vicinity of Constance a few years ago. A species similar to this, or to the preceding one, and of which one or two insects are sufficient to destroy a whole vine, was observed in the Crimea by Pallas‡. This species appears to be the caterpillar of a *Procris* or *Zigæna* (a genus separated from the genus *Sphinx*), and is said to be nearly allied to the *Zigæna Statices*; it is found upon the sorrel and dock in the environs of Paris§. The *Pyralis fasciana*||, which has anterior wings of a dark cinder colour, with a brown line and points of the same colour, has also been mentioned as infesting the vine, or as corresponding to one of the species just alluded to. There is also another species which may be ranked among the insects to which our cultivators have given the names of Vine-moth and Grape-moth, we mean Hübner's *Tinea ambiguella*¶. But

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* Bosc, *Notice sur la Pyrale et autres Insectes qui nuisent aux Vignobles; Esprit des Journaux*, p. 139, and *Bulletin de la Société d'Encouragement*.

† Kirby, *Introduct. to Entomology*, vol. i. p. 205.

‡ Pallas, *Travels in Russia*, vol. ii. p. 241.

§ Walckenaer, *Faun. Paris.*, vol. ii. p. 284. No. 2. Fabricius, *Entom. Syst.*, vol. iii. part i. p. 406. No. 8. Godart, *Hist. des Lépidoptères de France*, vol. iii. p. 158. pl. 22. *Dict. Classique d'Hist. Nat.*, vol. xiv. p. 289, at the word Procris.

|| Fabricius, *Entom. Syst.*, vol. iii. part ii. p. 261. No. 78. Fabricius considers it to be the *Tortrix Heparana* of the Catalogue of Vienna; it is not the *Fasciana* of Linnæus. Compare Friedrich Treitschke, *Die Schmetterlinge von Europa*, vol. viii. p. 28.

¶ Hübner, tab. 22. fig. 153. sect. 64. No. 61 of the text. Treitschke, *Die* to determine the synonymy of the various species of the Lepidoptera more particularly injurious to the vine, which I have found mentioned in the works of naturalists, travellers, and agriculturists, I have had recourse to the skilful and practised eye, and the judicious criticism of M. Duponchel, one of the most accomplished lepidopterists of Europe.

From an attentive examination of this subject we conclude that, with the exception of those which are occasionally found upon the vine, as well as upon other plants, without producing much injury, and of which we shall treat in the following sections, all the species of Lepidoptera which may be considered as particularly detrimental to the vine are reduced to the four following, all producing caterpillars which envelop themselves in leaves, and to which may equally be applied the ancient names of *Involvulus* and *Convolvulus.* In fact we cannot possibly suppose that the ancients made observations sufficiently exact to distinguish differences which the moderns themselves, notwithstanding the extended inquiries lately made upon the subject, have great difficulty in proving.

The first of these species is that which was observed by Bosc, and which he names *Pyralis Vitis*; Fabricius has described this insect under the name of *Pyralis Vitana,* from the specimen in Bosc's collection. For reasons, unfortunately too decisive, which we shall presently allege, we shall not preserve either of these names: we name it *Pyralis Danticana,* from Bosc's second name Dantic, the name Bosc having been employed by Fabricius in his description of another Pyralis which he calls *Pyralis Boscana.* The second species is the *Procris ampelophaga* of Duponchel, Bayle, and Passerini, the *Procris Vitis* of Boisduval. The third species is the *Tortrix Roserana* of Frölich, the *Cochylis Roserana* of Duponchel and Treitschke, and the *Tinea ambiguella* of Hübner. The fourth is the *Tortrix Heperana* of Treitschke and Duponchel, the *Pyralis fasciana* of Fabricius.

The caterpillar of the *Cochylis Roserana,* mentioned by Frölich as causing great devastations in the vineyards near Stuttgard, has not been described by him or any other entomologist that I am acquainted with. There remains then the *Pyralis Danticana**, the *ampelophaga*† of Bayle and Passerini, and the *Fasciana,* the destructive effects of which upon the vine cannot be called in question. The caterpillars of the

Schmetterlinge von Europa, vol. viii. p. 280 and 281. No. 8. *Cochylis Roserana alis anticis argenteis ochroleucis nitidis, fascia media intus angustiore fusca.*

* *Pyralis Vitana, alis fusco virescentibus; fasciis tribus obliquis fuscis marginalibus.* Bosc Dantic, *Mém. de la Société d'Agriculture,* 1786, for the summer quarter, p. 22. pl. 4. fig. 6. *Pyralis Vitis,* Fabricius, *Entom. Syst.,* vol. iii. p. 2. pl. 249; A. J. Coquebert, *Illustratio Iconographica Specierum Insect. quæ in Musæis Parisinis observavit,* J. C. Fabricius, duas 1. tab. 7. fig. 9.

† *Procris ampelophaga,* C. Passerini, *Memoria sopra duo Specie d'Insetti nocivi. Zigæna ampelophaga,* Bayle-Barelle, *Degli Insetti nocivi al Uomo, alle Bestie, al Agricoltore*; Milano, 1824, pl. 1. fig. 7 to 12.

first two species are the only ones upon which we have continued observations; these we proceed to mention. The larva or caterpillar of the first of these two species, the *P. Danticana**, according to Bosc, is comprehended with other species in the environs of Paris under the collective name of *larvæ or worms which injure the vine*; in Burgundy and the vine provinces it is called *Ver-coquin*, a denomination which is also sometimes given to the white worm of the Cockchafer, the Spondyle of Pliny. This caterpillar of the Pyralis of the vine is, shortly after its birth, a centimetre in length; its head is black and its body green, and it has a yellow spot on each side of the neck. Its first appearance is about the end of May, but its greatest devastations are made in the middle of June. It cuts the petioles of the leaves in halves, which causes them to wither, and enables the insect to roll them with greater facility. When the leaf first attacked withers, in consequence of the wound which it has made in the petiole, it proceeds to attack another; and thus one of these caterpillars will destroy several leaves, weaken the vine, and prevent the grapes from becoming large and sweet. This insect does not attack the fruit, but destroys the peduncle of the bunch, which, if it do not wither, remains small and without flavour. When the greater part of the leaves are infested, all the bunches are soon in the same condition, because they grow at the bottom of the stem, and it is there that this caterpillar commences its ravages. The butterfly or Pyralis of this caterpillar is of the size of the nail of the little finger; its wings are of a green fulvous colour, with three oblique bands of brown. These Pyralides are most abundant in the month of July. During the day they remain clinging upon the stems, under the leaves, whence they fly upon the slightest approach of danger. Towards the decline of the day, in the dusk, the male seeks the female; but those which leave their retreats at an earlier hour become the prey of the swallows and other insectivorous birds.

I have remarked that Bosc identified the butterfly which he described under the name of *Pyralis Vitis* with a new species that Fabricius names *Pyralis Vitana*. This species, as I have said, was described by Fabricius at Paris from a specimen in Bosc's collection; and he adds five or six lines of technical description. M. Coquebert, of Reims, published at the same time four fasciculi of insects, drawn, engraved and coloured from the specimens observed and described by the Danish naturalist in the collections of Paris, and among the number is the *Pyralis Vitana* or *Pyralis Vitis* of Bosc. It would appear that no insect ought to be better known than the one we are treating of; this however is not the fact. After a most attentive examination, Duponchel finds the descriptions of Fabricius and Bosc too short, and insuffi-

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* Bosc, *Nouv. Dict. d'Hist. Nat.*, vol. xxxv. p. 392.

cient for the recognition of the insect and the determination of its species; he considers Coquebert's figure of it as too coarsely drawn to throw any light upon the descriptions. This is also the case with the descriptions of Bosc, and the figures by which his memoirs are accompanied. The German authors, Frölich, Treitschke, and others, who in latter times have particularly devoted themselves to the study of the smaller species of Phalænæ, or Moths, are of the same opinion as Duponchel, for not one of them mentions the *Pyralis Vitana* of Fabricius. This species is not mentioned in their voluminous works specially devoted to these insects; or if it be mentioned, it is without their being themselves aware of it. If in the numerous species which they have described they had discovered the *Pyralis Vitana* they would not have failed to cite Fabricius, whose works are in the hands of every entomologist. In this difficulty Duponchel has had recourse to Bosc's collection, which now forms part of the collection at the Museum; and he has found there, under the name of *Vitana*, a Pyralis which is figured and described by the German authors under the name of *Pillerana*. Now, according to them, the caterpillar of this Pyralis lives upon the *Stachys Germanicæ*, a plant too entirely distinct from the vine to allow of it being easily admitted that it lives indifferently upon the two vegetables. But besides, Fabricius has also described the *Pyralis Pillerana*, and the description which he gives of it differs essentially from the *Pyralis Vitana*; the latter is marked with three bands, the *Pillerana* has only two; the colour of the ground in the *Vitana* is of a brownish green, that of the *Pillerana* is of a golden green. From these circumstances M. Duponchel thinks that Bosc has committed the error of labelling one species for another; or, which is more probable, that the label of the *Pyralis Vitana* has been displaced in his collection, which is in great disorder. Duponchel has compared the description given by Bosc of the caterpillar of the *Pyralis Vitana* with those of all the caterpillars of the *Pyralides* or *Tortrices* mentioned in the authors who have treated of this family, and has not found one which appeared to apply to it. I however maintain, and remarked to him, that even if we could suppose that Bosc had been deceived with regard to the butterfly proceeding from the caterpillar, he was not so with regard to the existence of the caterpillar itself, and the curious observations which he had made upon it; and that being myself, two years ago, at Braubach on the Rhine, in the state of Nassau, I remarked a cultivator (the innkeeper of the place) engaged in pulling off such of the leaves of his vines as were coiled up, and he told me it was to destroy an insect which made great havoc in them. I opened several of these leaves, and found in them a very small caterpillar, which I examined with a lens; I perceived that it was the caterpillar described by Bosc, and which I had also previously observed in the environs of Paris. I expressed my surprise to

M. Duponchel that after the progress which had been made by the united efforts of French and German naturalists in this branch of entomology, we could not recognise a butterfly which had been twice drawn and described by skilful naturalists, and which must be common, since its caterpillar was so. To this M. Duponchel replied that he thought I was mistaken in supposing myself certain of having distinguished the caterpillar described by Bosc, because the description given of it by this naturalist in his memoir is so far from precise that it may be applied to all the caterpillars of this genus which have green bodies and black heads, but which differ in other characters of which he does not speak, such, for example, as the colour of the verrucose points with which all the caterpillars of this group are decorated. As to the butterfly, the description and figure by Bosc, the description by Fabricius, and Coquebert's figure, drawn from the individual described by Fabricius in Bosc's collection, may equally be applied to the four following species of Phalænæ : the *Cerasana* and *Riberana* of Treitschke, and the *Corylana* and *Fasciana* of Fabricius. The last approaches more nearly than the others to Bosc's description ; but this species is also described by Fabricius, and Bosc has not recognised it as his own. Still more, after saying that Réaumur had not anywhere mentioned the caterpillar which was the subject of his memoir, he adds : " It appears to be equally rare in other climates, for neither Linnæus, Fabricius, nor Scopoli has described the Phalænæ which it produces."

From these researches and explanations it appears that if the *Pyralis Danticana, Pyralis Vitana* of Fabricius, has not been confounded by him and Bosc with the *Fasciana*; that if it be not the same species as the latter, it must be considered as a species still unknown, and which cannot be well known until we have bred all the caterpillars found upon the vine which resemble the one described by Bosc. To deduce this deficiency in science is almost to acquire the certainty of its being speedily supplied. Although the silence of the Italian naturalists relative to this caterpillar be not a decisive reason for thinking that it is not found in Italy, and did not receive from the ancients the name of *Involvulus*, yet this is more especially true with regard to another caterpillar to which the names *Convolvulus* and *Involvulus* appear more peculiarly applicable. More attentive observations have been made upon this caterpillar than upon that described by Bosc, and its butterfly is well known as the *Procris ampelophaga*, or Procris of the vine so much dreaded by all the cultivators of Tuscany. This caterpillar sometimes injures considerably the buds and young shoots of the vine. In Piedmont it sometimes devours half the vine-plots. It is five or six lines in length, and two lines or two and half in width ; its colour is a brown gray, and the hair is disposed in stars in four longitudinal rows in semi-globular relief towards the anterior part. The inferior surface of the abdomen is smooth and of a yellow white; it attains its full growth towards the end of May, and it is then that it destroys the leaves of the vine. It attaches itself to the upper part of the leaf, and if the branch upon which it is found be shaken, it bends itself in the form of a bow by resting upon the two extremities of its body, and drops down upon the earth. The greatest number of these caterpillars that are to be found upon one vine amounts to about ten; they are generally much fewer. Between the 20th and 30th of May this caterpillar spins a cod of long white flocks, in which it remains motionless, and is transformed into a chrysalis from the 5th to the 10th of June. The chrysalis is at first yellow, with black points upon each segment, but at the moment of transformation the colour increases in intensity and is changed into a deep azure blue. The transformation of the chrysalis into the butterfly generally commences on the 19th of June, and is not concluded till the 25th. This butterfly is the *Procris Vitis,* or *Procris ampelophaga* of modern entomologists. Its wings are of a dark colour, approaching to black, and changing into a sombre green; the body is of a blueish green. The *Musca brevis* often introduces its eggs into the body of the chrysalis of this butterfly; the larvæ of the fly feed on the substance of the chrysalis without altering its exterior, and the chrysalis thus appears to be metamorphosed into a fly instead of producing a butterfly. Each female of this Procris lays about three hundred straw-coloured eggs, of so small a size that they are scarcely visible to the naked eye. Towards the 3rd of July these eggs produce small whitish caterpillars, which are transparent, and covered with almost imperceptible hairs. The caterpillars of this second race are metamorphosed about the 26th of August.

I have myself verified in part the observations made upon the caterpillar of the *Pyralis Danticana* by Bosc. The habits of the *Procris ampelophaga* are only known to me from the memoir of M. Passerini. But if the first species be as abundant in Italy as the second, I shall be induced to think that it is to it that the ancients more particularly applied the names of *Involvolus, Involvulus, Involvus,* and *Convolvulus.*

IX. *Kampe.—Eruca.—Caterpillars of the* Sphinx Elpenor, *or Sphinx of the Vine,—of the* Bombyx purpurea, *or Ecaille mouchetée* (Spotted Tiger-moth),—*and of the* Sphinx Porcellus, *or Sphinx with red bands.*—The other caterpillars that are found upon the vine, and which may occasionally injure it, as well as plants of every other kind, do not belong to the tribe of *Tortrices,* or *Pyralides,* nor to the genus *Procris.* The species which I have most frequently had occasion to remark, are the *Bombyx purpurea* of Fabricius, the *Arctia purpurea* of modern naturalists, and the *Ecaille mouchetée* of Geoffroy, which lives also upon the common broom, the elm, and twenty other plants*. The *Sphinx Elpenor*, or the Sphinx of the vine of Geoffroy, (this is not the *Sphinx Vitis* of modern entomologists, an American butterfly which does not live upon the vine,) is frequently found upon the vine, but it is also met with not less frequently upon the Epilobium, the Salicaria, the balsam, and the convolvulus †. Lastly, the *Sphinx Porcellus*, or the Sphinx with red indented bands, the caterpillar of which is sometimes found upon the vine, but still more often upon the honey-suckle, lavender, and more especially upon the yellow bed-straw, *Galium verum* ‡. The last two species have caterpillars as large as the little finger, and as they keep upon the summit of the shoots they may be easily removed.

These are the caterpillars or larvæ of Lepidoptera which the Greeks and Latins, when speaking of insects infesting the vine, designated by the general names of *Kampe* or *Eruca*; but they did not confound these larvæ with worms, and they knew that they underwent metamorphoses.

X. *Phtheir.—Tholea or Tholaath.*—Coccus Vitis.—*Kermes of the Vine.*—Coccus Adonidum.—*Greenhouse Coccus.*—The *Phtheir* or louse of the vine, which Ctesias mentions as an insect which causes the vine to perish, and which in the Geoponics is classed with the caterpillars among that plant's greatest enemies, can correspond only to the *Coccus Vitis*, to the Cocci, or the Kermes of the vine §. We know that the Cocci or gall-insects, or the Cochineals, with the Aphides, are the insects which, from their small size and their rapid multiplication, are the most similar to the louse; their females also, like lice, are apterous, or without wings. The Cocci cover so completely the bark of the trees that it has a scurfy appearance. When the female has deposited her eggs, her body dries up and becomes a solid crust, which covers the eggs, and its squamous surface is not unlike fat nits. These insects do harm by piercing the wood with their sharp proboscis, which is formed of a sheath having numerous joints, and three bristles or darts of great tenuity. With this tube they suck the sap and cause it to flow.

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* *Arctia purpurea*, Fabr. *Entom. Syst.*, vol. iii. part 1. p. 466. No. 185. Walckenaer, *Faun. Paris.*, vol. ii. p. 291. Godart, *Papillons nocturnes*, vol. i. p. 339. No. 105.

† *Sphinx Elpenor*, Fabr. *Ent. Syst.*, vol. iii. p. 372. No. 51. Walckenaer, *Faun. Paris.* vol. ii. p. 276. No. 6. Godart, *Crépusculaires*, p. 46.

‡ *Sphinx Porcellus*, Fabr. *Ent. Syst.*, vol. iii. p. 373. Walckenaer, *Faun. Paris.*, vol. ii. p. 279. Godart, *Crépusculaires*, p. 51. Duponchel, *Iconographie des Chenilles*, tribe of *Sphingidæ*, pl. 5. fig. 1, *a*, *b*.

§ Ctesias, *Indicorum*, cap. 21. p. 253. edit. Baehr, Frankf., 1824, 8vo. Ctesias speaks of a red insect which in India destroys the trees producing amber, as in Greece the *Phtheir* destroys the vine: Larcher, p. 341. vol. vi. of his translation of Herodotus, has badly rendered this passage.

Our cultivators do not complain of these insects, and know but little of them, because the annual pruning which the vines undergo prevents their multiplication, as the Cocci can only live upon young wood, while its epidermis is still tender. They are however sometimes very abundant upon neglected vines; and in countries where the vine is only cultivated in greenhouses, they multiply extremely, whilst the other enemies of the vine are there unknown*. But the vines in greenhouses are not attacked by the same species of Cocci as they are exposed to in the open air. In the former situation they are attacked by the *Coccus Adonidum*†, not by the *Coccus Vitis*. If, as has been asserted, this insect originally came from Senegal, it is not among the species treated of by the ancients, who also could never have distinguished from each other the various species of the Coccus, which is as much as can be effected by the practised eye of the modern entomologist, aided by a powerful lens, even since the beautiful and recent work of M. Boyer de Fonscolombe upon these insects. This skilful naturalist remarks with truth that there are no well-established limits between the Kermes and the Cocci, between the Gall-insect and the Progall-insects of Réaumur. He therefore makes but one genus of the Coccus and the Kermes; but this he subdivides into several sections, and the Coccus of the vine‡ belongs to the section which is composed of species which at the time of laying have naked bodies, without any trace of rings or members, and rest upon a very cottony nest. The *Coccus Adonidum*, or Kermes of the greenhouse, is also remarkable for the white and downy substance which transudes through its skin, and which gives it a mealy aspect.

The interpretation of the word *Thola*, *Tholea*, or *Tholaath* employed in the Bible, which we considered at the commencement of these researches, applies to the name *Phtheir* given to the Gall-insect by the author of the Geoponics. It will be recollected that the result of our long discussion upon this subject was, that *Thola* is employed in the Bible to signify not only a worm, vermin, an insect or larva of an insect, or an animal vile and despicable, but also an insect or larva of an insect which infested the vine, and another plant, the name of which we are unacquainted with, but which we know to have been a large tree, because it gave an extensive shade. Indications so vague would not

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* J. Major, (Landscape Gardener,) A Treatise on the Insects most prevalent on Fruit Trees and Garden Produce, 1829, 8vo, p. 112.

† *Coccus Adonidum*, Fabr. *Syst. Rhyngotor.*, 1803, 8vo, p. 307. No. 4. Major, as just referred to, p. 144, the Mealy-Bug.

‡ *Coccus Vitis*, Boyer de Fonscolombe, *Ann. de la Soc. Entom.*, vol. iii. p. 214. No. 14. Réaumur, *Mem. Insect.*, vol. iv. p. 62. pl. 6. figs. 1 to 7. Fabr. *Syst. Rhyngotor.*, p. 310. No. 4. *Coccus vitis viniferæ.*

lead us to any probable conjecture upon the subject of the *Thola* or *Tholea*, if this word, which in the Bible is employed separately, were not elsewhere frequently found in conjunction with the word *Dibaphi*[*] to denote the insect that the Arabs term Kermes, and which, when heated with vinegar, produces a fine red colour, in a word, the Cochineal insect. The species of cochineal which produces this colour in Europe are the *Coccus Ilicis*, which attaches itself to the green oak[†], and which consequently may be the insect mentioned in the Bible as the destroyer of a tree giving shade; and the *Coccus Polonicus*, which adheres to the roots of the annual Scleranthus and other plants[‡]. The Coccus of the vine does not produce this colour, but the resemblance of these insects, and their generic affinities, must have caused them to be confounded with the other *Coccus*, or the *Tholaath Dibaphi*, or at least comprehended under the same denomination. Thus we say, and with much less propriety, the worm of the apple and the nut, though these are the larvæ of insects of very dissimilar genera. The word *Thola* or *Tholaath* in the Bible was employed for vermin, louse, a small, insignificant, vile, and contemptible insect, as the *Phtheir*; but the epithet *Dibaphi*, designating the Kermes or insect useful in dyeing, which was sometimes added to the word *Thola* or *Tholaath*, indicated sufficiently, by the similarity of the species, the nature of the insect or vermin intended by the word, and which was productive of so great injury to the vine and certain trees.

XI. *Means which are to be employed to destroy the Insects which infest the Vine.*—The recipes of Pliny and Columella for the protection of the vine from the insects which attack it appear to prove that the Cocci committed greater ravages upon the vines in ancient than in modern times. Their directions were to rub the stems and branches of this plant with greasy substances, such as oil or bear's fat, to which was also added the use of vesicating substances. Modern cultivators, as I have said, protect the vine from the Coccus by pruning it. But other methods must be employed for the destruction of the Weevils (*Becmares*) and Coupe-bourgeons, the *Rhynchites Bacchus* and *Rhynch. Betuleti*, and the *Eumolpus Vitis*. The best of all is to choose the moment when these insects have undergone metamorphosis and begin to copulate, and to place under each vine a kind of basin made for the purpose in the form of a deeply recurved crescent, so as to surround the stem or branch

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[*] Bochart, *Hierozoicon*, p. 22.

[†] *Coccus Ilicis*, Fabr. *Syst. Rhyngotor.*, p. 308. Réaumur, *Insect.*, iv. tab. 5. Garidel, *Plantes des Environs d'Aix*, p. 250. pl. 35. Boyer de Fonscolombe, *Ann. de la Société Entom.*, vol. iii. p. 210.

[‡] *Coccus Polonicus*, Fabr. *Syst. Rhyngotor.*, p. 310, No. 26. Frisch., *Insect.*, 56. Walckenaer, *Faun. Paris.*, vol. ii. p. 363.

under which it is placed, and then to shake the branches and make the insects fall into it. The substitution for the basin of a very wide tin funnel with a bag at the extremity, into which the insects fall, has been proposed; also that of linen twisted into the same form. The same means may be applied for the caterpillars of the butterfly or moth as for the Coleoptera, especially when they have arrived at a certain size. The devastation is then indeed almost completed, for the leaves are decayed and partly devoured; but the repetition of the evil in the following years may be precluded by thus preventing the reproduction of the insects. To this method may be added another, which is particularly adapted to the destruction of the Pyralis of the vine, the *Procris ampelophaga* of Passerini, and in general to that of all the small species of Phalænæ which attack the vine: it is that of lighting fires at the commencement of the night in a direction opposite to the wind. The insects come in crowds to the fire and are burned. These fires must be renewed for ten or twelve days in succession, but not when there is much rain or wind; for not only the flame will not burn, but the butterflies in such weather remain obstinately fixed to the leaves to which they have attached themselves. The most effectual method of destroying all the larvæ of the Lepidoptera and Coleoptera which attack the vine is to remove, one by one, the coiled leaves in which these insects have deposited their eggs, and to throw them into a furnace and burn them. This method is the most tedious and expensive, but it is also the most certain; and I have seen it pursued with great patience and care in the state of Nassau by the cultivators on the banks of the Rhine.

Third Section.

Synonymy of all the species of insects which have been mentioned in these researches.

We shall present in this section one of the principal summaries of these investigations by giving the synonymy of all the insects of which we have had occasion to treat; but to adapt it to the end in view we must proceed in an order the inverse of that which we followed in the preceding section; that is, we must first give the synonymy of the insects which are most detrimental to the vine plants, and then proceed to those which only injure them occasionally, and conclude with those which the ancients have erroneously designated as the enemies of the vine; carefully conforming, with regard to each of these three sorts of insects, to the classification most generally adopted by modern naturalists. Finally, we shall conclude by giving a list of insects which do not injure the vine, but the synonymy of which has been incidentally determined in these researches.

I. *Synonymy of the Insects most injurious to the Vine.*

COLEOPTERA.

Ancient Names.	*Names of Modern Naturalists, Latin and French.*	*Common Names.*
1. *Greek.* Ips (Vitis). Iks.	Eumolpus Vitis (the larva).	*French.* Gribouri de la Vigne (the larva).—Coupe-bourgeon.— Ebourgeonneur.—Couturière. — Ver de la Vigne.
2. *Latin.* Volucra.	Eumolpus Vitis (the perfect insect). Eumolpe de la Vigne.	Gribouri de la Vigne (the perfect insect). Coupe-bourgeon, &c.
3. *Latin.* Volvox.	1. Rhynchites Bacchus (the larva). 2. Attelabus Betuleti (the larva). Attelabe de la Vigne. Charanson de la Vigne.	Urbie.—Béche. — Lisette.—Diableaux.—Destreaux. *Italian.* Tagliadizzo.
4. *Greek.* Kantharis.	1. Rhynchites Bacchus (the perfect insect). 2. Rhynchites Betuleti (the perfect insect). Attelabe de la Vigne. Charanson de la Vigne.	Becmare.—*Eng.* Weevil. Velours vert.
5. *Greek.* Kantharis. Melolontha. *Latin.* Scarabæus.	Lethrus Cephalotes.	*German.* Schneider (Cutter).

ORTHOPTERA.

1. *Hebrew.* Gaza.	1. Locusta Ephippiger (Sauterelle à selle ou à cymbale).	*English.* Saddle Locust.
	2. Locusta aptera (Sauterelle aptère).	Wingless Locust.
	3. Locusta Puppa (Sauterelle-Nymphe).	Nymph Locust.

HEMIPTERA.

1. *Hebrew.* Thola, Tholea, or Tholaath. Tholaath Dibaphi. *Greek.* Phtheir.	1. Coccus Vitis; Cochenille de la Vigne.	*English.* Mealy-bug (Punaise farineuse). Kermes of the vine.
	——— Adonidum.	Cochenille des Serres (Green-house Coccus).
	——— Ilicis.	Cochenille du Chêne vert.
	——— Polonicus.	Cochenille de la Scleranthe.

LEPIDOPTERA.

1. *Latin.* Involvulus or Involvolus. Involvus. Convolvulus. Campe. *Greek.* Kampe.	Pyralis Danticana (the caterpillar)? Pyralis Vitis. *Bosc Dantic, Mém. de la Société d'Agric.* 1786, p. 22, pl. 4. fig. 6. Pyralis Vitana. Pyralis Fasciana. *Fabric., Entom. System.*	*Fr.* Ver-Coquin. Teigne de la Vigne. *Eng.* Vine-moth.

Ancient Names.	*Names of Modern Naturalists, Latin and French.*	*Common Names.*
2. *Latin.* Convolvulus. Involvulus.	Procris ampelophaga (the caterpillar). *Duponchel, Suppl. à l'Hist. des Lépid.* t. 2, p. 92, pl. 8, fig. 2.	*Fr.* Teigne du Raisin. Ver-Coquin. *Ital.* Ritoritello. *Eng.* Grape-moth.
	Procris ampelophaga. *Bayle-Barelle, Dei Insetti nocivi*; Milano, 1824.	
	Procris ampelophaga. *Passerini, Mem. sopra due Specie d'Insetti nocivi, nelle Mem. dell' Accad. dei Georgifili,* 1830, p. 4, t. 1, fig. 1 e 14.	
	Sphinx ampelophaga. *Hübn. Suppl.* t. 24, fig. 153 et 154.	
	Atychia ampelophaga. *Treitschke,* t. 10, *Suppl.* p. 100.	
	Sphinx Vitis. *Freyer, Beytr.* ii., Bd. xii., Ht. 5, 69, tab. 68.	
	Procris Vitis. *Boisduval, Icones Historiques des Lépidoptères nouveaux,* t. 2, p. 79, pl. 56, fig. 2 et 3.	
3. Involvulus. Convolvulus.	Cochylis Roserana. *Duponchel, Hist. des Lépidopt.* t. 9, p. 418, pl. 257, fig. 8.	Teigne de la Vigne. Rouleuse. Tordeuse.
	Tortrix Roserana. *Frœlich, Enumer. Tortricum Würtemb. indig.* p. 52, No. 511.	*Eng.* Leaf-roller. Small Brown-bar.
	Tinea ambiguella. *Hübner,* tab. 22, fig. 153 (fœm.).	
	Cochylis Roserana. *Treitschke,* t. 8, p. 280.	
4. Involvulus. Convolvulus.	Tortrix Heperana (the caterpillar).	Chape-Brune. Teigne du Lilas.
	Tortrix Heperana (the caterpillar). *Duponchel, Hist. des Lépidoptères de France,* t. 9, p. 67, pl. 238, fig. 7.	Teigne du Raisin. Teigne de la Vigne.
	Tortrix Heperana. *Wien, verz.? Illiger, Schranck, Gotze et Treitschke,* t. viii. p. 58, No. 8.	*Eng.* Dark Oblique-bar.
	Tortrix Padana. *Schranck, Faun. Boica,* ii., 32, Abth. 5, 78, No. 1755.	
	Tortrix Carpiniana. *Hübner,* tab. 18, fig. 16 (fœm.).	
	Tortrix Pasquayana. *Frœlich, Wien, Verz.* p. 36, No. 55	
	Pyralis Fasciana. *Fabricius, Ent. Syst.* iii. 2, 348, 24.	
	Lozotænia Carpiniana. *Stephens, Syst. Cat. of British Insects,* p. 169, No. 6852.	
	La Chape-Brune. *Geoffroy,* t. 2, p. 169, No. 118.	
	Phalène Chape-Brune du Lilas. *Degeer,* t. 1. *Mém.* 13, p. 403.	

II. *Insects which only occasionally injure the Vine.*

COLEOPTERA.

Ancient Names.	*Names of Modern Naturalists, Latin and French.*	*Common Names.*
1. *Greek.* Spondyle.	Melolontha vulgaris. — Le Hanneton vulgaire.	Le Hanneton.—*Eng.* Cockchafer; May-bug; Oak-web.
2. *Latin.* Spondyle genus serpentis (Plin.).	Melolontha vulgaris (the larva).	Ver blanc. Turc.
	Melolontha Vitis (the larva).	Man. Courterolle. Petit Hanneton d'été, ou Hanneton vert (the grub).

ORTHOPTERA.

1. Biurus.	Acheta Grillo-Talpa (Fabr.). Talpa Ferrantis (Aldr.).	La Courtillière.—*Eng.* Mole-cricket.

LEPIDOPTERA.

1. *Greek.* Kampe. *Latin.* Eruca.	1. Arctia purpurea (the caterpillar). L'Ecaille mouchetée.	Chenilles de la Vigne. Vine Caterpillars.
	2. Sphinx Elpenor (the caterpillar). Sphinx ou Papillon rouge de la Vigne.	*Eng.* Elephant Moth.
	3. Sphinx Porcellus (the caterpillar). Sphinx ou Papillon à bande rouge dentelée.	*Eng.* Small Elephant Moth.

III. *Insects erroneously described by the ancients as injuring the Vine.*

POLYPODA.

1. *Greek.* Iulios. *Latin.* Centipedes. Millipedes.	1. Julius sabulosus, Jules des sables.	Mille-Pieds. *Eng.* Galley-worm.
	2. Julius terrestris, Jules terrestre.	
	3. Julius communis, Jules commun.	

COLEOPTERA.

1. *Greek.* Kantharis. *Latin.* Cantharis.	1. Mylabris Cichorii, Mylabre de la Chicorée.	Mouches-cantharides.
	2. Lytta vesicatoria, la Cantharide.	*Eng.* Spanish Fly.
2. *Greek.* Ips (Homer).	Dermestes (the larva).	Ver.—*Eng.* Leather-eater.

IV. *Names of Insects mentioned by the Ancients, which do not injure the Vine, but the modern names of which have been determined in these investigations.*

1. *Greek.* Melolontha. Kantharis. *Latin.* Scarabæus. Cantharis.	Coleoptera of Linnæus. Eleutherata of Fabricius.	Scarabées. Escarbots. *Eng.* Beetles, Chaffers, Dors, Clocks, and Bobs.
2. *Greek.* Kantharis.	*Latin.* 1. Ateuchus sacer. Scarabæus sacer.	Le Pillulaire.

Ancient Names.	*Names of Modern Naturalists, Latin and French.*	*Common Names.*
Latin. 1. Scarabæus qui pilas volvit (Plin.).	2. Ateuchus Ægyptiorum. Scarabée sacré. Bousier sacré.	*Eng.* Tumble-dung Beetle.
3. 2. Scarabæus cui sunt cornicula reflexa (Plin.). Bousier of Horapollo, which has two horns and resembles a bull.	*Latin.* Onitis Midas. *French.* Bousier à deux cornes.	Le Pillulaire.
4. 3. Lucanus cui sunt cornua prælonga bisulcis dentata forcipibus in cacumine (Plin.).	*Latin.* Lucanus cervus. *French.* Lucane Cerf-volant.	Le Cerf-volant.—*Eng.* Stag-beetle. Pinch-bob.
5. 4. Scarabæus Fullo albis guttis (Plin.).	*Latin.* Cetonia aurata. *French.* Cétoine dorée.	*Eng.* Green or Rose Beetle.
6. 5. Ips of Homer, of St. John Chrysostom, and of the grammarians of the middle ages.	Larva of the Dermestes Pellio, of the Dermestes Lardarius; the larva not yet known of a species of Dermestes which is related to these two species, and which gnaws the horn of the Ægagrus, or wild goat.	
7. *Greek.* Kantharis. *Latin.* Scarabæus parvus Cantharis dictus (Plin.).	*Latin.* 1. Curculio granarius, Calandra granaria. *French.* La Calandre, ou le Charanson des Grains. *Latin.*2. Curculio frumentarius, Apion frumentarius. *French.* Charanson du Froment.	Corn-weevil.

V. *Summary of the Synonymy of the Insects mentioned in these researches, arranged according to their natural order.*—To accommodate agronomers and the learned, we thought it necessary in the preceding paragraph to divide the synonymy of the insects which have been mentioned in these researches into three sections. For the use of naturalists it must be repeated according to the natural order, and without any distinction of those which injure the vine much, or little, or not at all. For the sake of brevity we shall be satisfied with designating the insect by the name which it bears in our best systems; it will be immediately followed by the French or common name most generally in use, and lastly by the ancient names, printed in small capitals.

MYRIAPODA.

1. Julus sabulosus, Jule des sables.
   JULIOS, CENTIPEDES, MILLEPEDES.
2. Julus terrestris, Jule terrestre.
   JULIOS, CENTIPEDES, MILLEPEDES.
3. Julus communis, Jule commun.
   JULIOS, CENTIPEDES, MILLEPEDES.

COLEOPTERA.

1. Dermestes Lardarius,
   Dermestes Pellio, aut species proxima; (the larva).
   Le Dermeste des fourrures ou de la corne (the larva).
   IPS of Homer.

2. Ateuchus sacer, le Bousier sacré, le Pillulaire.
   CANTHARIS, SCARABÆUS QUI PILAS VOLVIT (Plin.).
3. Ateuchus Ægyptiorum, Bousier Egyptien.
   CANTHARIS, SCARABÆUS QUI PILAS VOLVIT (Plin.).
4. Onitis Midas, le Bousier à deux cornes.
   SCARABÆUS CUI SUNT CORNICULA REFLEXA.
5. Lethrus Cephalotes, Schneider (Cutter).
   KANTHARIS, MELOLONTHA, SCARABÆUS.
6. Melolontha vulgaris, le Hanneton ordinaire.
   Spondyle (the perfect insect).
   Sphondyle genus serpentis (Plin.) (the larva).
7. Cetonia aurata, la Cetoine dorée.
   SCARABÆUS FULLO ALBIS GUTTIS (Plin.).
8. Lucanus Cervus, le Cerf-volant.
   LUCANUS.
9. Mylabris cichorii, Mylabre de la chicorée.
   KANTHARIS, CANTHARIS.
10. Lytta vesicatoria, la Cantharide.
    KANTHARIS, CANTHARIS.
11. Eumolpus Vitis, Gribouri de la Vigne (the perfect insect). Ver-Coquin (the larva).
    IPS (the larva). VOLUCRA (the perfect insect).
12. Rhynchites Bacchus, Attelabe de la Vigne, Becmare, Tagliadizzo.
    VOLVOX, CANTHARIS.
13. Rhynchites Betuleti, Velours-Vert.
    CANTHARIS.
14. Calandra granaria, la Calandre, Charanson des grains.
    SCARABÆUS PARVUS CANTHARIS DICTUS (Plin.).
15. Curculio frumentarius, Charanson du Froment.
    SCARABÆUS PARVUS CANTHARIS DICTUS (Plin.).

ORTHOPTERA.

1. Acheta Gryllo-Talpa, le Grillon-Taupe, la Courtilière.
   BIURUS (Cicero, Plin.).
2. Locusta Ephippiger, Loc. aptera, Loc. Pupa. Sauterelle à Cymbales, Sauterelle aptère, Sauterelle Nymphe.
   GAZA (*Hebrew*).

HEMIPTERA.

1. Coccus Vitis, Coc. Adonidum, Coc. Polonicus, Cochenille de la Vigne, Cochenille des Serres, Cochenille de la Scléranthe.
   THOLA or THOLAATH (*Hebrew*). PHTHEIR (*Greek*).

LEPIDOPTERA.

1. Arctia purpurea, l'Ecaille mouchetée.
   KAMPE, ERUCA (the caterpillar).
2. Sphinx Elpenor, Pap. rouge de la Vigne.
   KAMPE, ERUCA.
3. Sphinx Porcellus, Papillon à bande rouge dentelée.
   KAMPE, ERUCA (the caterpillar).
4. Pyralis Danticana, P. Vitana, Chenille ou Teigne de la Vigne, Ver-coquin, la Chenette.
   CAMPE, INVOLVULUS, INVOLVUS, CONVOLVULUS.
5. Procris ampelophaga, Atychia ampelophaga, Procris Vitis, Teigne du Raisin, Ritoritello.
   CAMPE, INVOLVULUS, INVOLVUS, CONVOLVULUS (the caterpillar).
6. Cochylis Roserana, Tortrix Roserana, Tinea ambiguella, Teigne de la Vigne.
   CAMPE, INVOLVULUS, INVOLVUS, CONVOLVULUS (the caterpillar).
7. Tortrix Heperana, Pyralis Fasciana, Lozotænia Carpiniana, Tort. Padana, T. Pasquayana, Chenille de la Chape-brune, Teigne du Lilas, Teigne de la Vigne.
   CAMPE, INVOLVULUS, INVOLVUS, CONVOLVULUS, (the caterpillar).

Thus there are thirty-six species of insects known by the moderns, of which we think we have determined the corresponding names in Hebrew, in Greek, and in Latin.

VI. *Conclusion.*—In France at the present day, 800,000 *hectares* [1,976,914 acres] of land are planted with the vine, the fruit of which, converted into wine, yields an annual produce of 760,000,000 francs, [30,158,730*l.* sterling.] The consideration therefore of the insects destructive of a plant which is the source of so much wealth does not appear superfluous; and to lessen my regret at having so long occupied the time devoted by the Academy to researches of more importance, I would at least persuade myself that these minute inquiries are not devoid either of interest or of utility.

Article X.

The Kingdoms of Nature, their Life and Affinity; *by* Dr. C. G. Carus.

From the *Zeitschrift für Natur und Heilkunde,* Band 1. Hefte 1. Dresden, 1819.

WHEN man awakes from that state in which he is but the passive recipient of impressions from the external world, and when therefore, instead of reposing in the consciousness of his increasing in strength and stature and exhibiting a reciprocity of bodily action of various kinds with surrounding objects, he feels the spirit, the infusion of the breath of God, in motion within him, he is powerfully impelled to endeavour, by bringing the relations between the spirit within and the phænomena without into a clear point of view, to obtain a clearer knowledge of himself. This desire has its origin in a most distinct conviction that without such knowledge no real harmony, no true internal equilibrium can be conceived to exist in man, and that nature and he must therefore stand as two eternally separated beings. But a feeling that things are separated which at the same moment exist in and through each other, is totally incompatible with that internal repose which, as we ourselves are *one,* is to be found not in the sense of separation but in the consciousness of unity. In this fact we clearly see what it was that gave birth to those speculations, by means of which it was sought for so many ages, sometimes with more and sometimes with less sincerity and freedom, to ascertain the relations between the phænomena of nature and the laws of mind. In those speculations, however, we have occasion to observe, how frequently that which stands forth in us most plainly and undisguisedly, and which for that reason should be supposed discernible and known at the very first, was exactly the least heeded and last discovered. It was, no doubt, owing to this circumstance, that many a truth which presented itself almost unveiled to the pure and unsophisticated feeling of the genuine children of nature remained a hidden mystery to the sages of mankind.

In order to avoid such errors it is particularly important that we should give a general and exact definition of the terms *proof* and *explanation.* Now to *explain* is but to consider a phænomenon in the clearness of a superior light, and to *prove* is but to trace a subordinate proposition up to a higher, or rather to a primary truth. The *supreme* and *one,* which is alike the foundation of nature and mind, can therefore no more be proved or explained than the splendour of the sun can be increased by means of some terrestrial light. On the contrary, the immediate consciousness of a supreme and eternal unity is the primary standard by which we distinguish the *just*, the *true*, and the *beautiful*. Without this principle we should indeed be incapable of pursuing any general inquiry and of forming any judgement, so that demonstration and science can exist for those only who recognise a positive and supreme principle. We hold, therefore, that the true end of scientific inquiry (so far as it is to furnish explanation) is not to define and demonstrate the highest principle, but to trace other truths up to this, to show the harmony which exists between nature and mind, or to discover a unity of law in the multiplicity of phænomena.

It is hoped that these general remarks may be sufficient to indicate the guiding principle of the following inquiries, which, being designed to lead to a clear conception of Life in general, and its single forms in particular, are here recommended by their author to the friendly attention and examination of medical men and naturalists, previously to their being, perhaps, at some future time, presented by himself or by some one else, in that strictly scientific form which is found so indispensably necessary to all who would penetrate the essence of nature, and obtain, instead of the vague and negative notions which commonly prevail, a distinct and positive knowledge.

If, with this view, we direct our attention to one only of the endless variety of forms which life assumes; if we observe, for instance, how a plant through internal instinct and under external relations unfolds itself from an obscure and insignificant seed, how its parts multiply, and how their organization becomes progressively more and more refined, until it reaches its acme in the flower, where the plastic power again concentrates itself into a seed, and thus closes the circle of its being in that form out of which it had first issued, we find throughout this chain of phænomena an internal pervading principle, a certain determinate succession, a regularity which compels us to expound all these movements, changes, and developments as parts of a whole, as the operations of one internal universal cause in which all others are comprehended. It is evident that this internal, this essential and efficient principle can be no single thing, such as the body of the plant, the chemical change of its substance, or the circulation of its sap, and still less the effect of external influences, but rather all these together— a something in which all these inhere as their common cause, and which we characterize as a unity by the generic appellation *life*. Hence it is easy to perceive how erroneous it would be, for instance, to suppose the plant first organized, and life then added to it as an attribute and consequently as something extrinsic, nearly in the same manner as we should conceive of a machine as a thing consisting of several parts put together and possessing, at first, no inherent power of acting, but having this power imparted to it when it is completed. On the contrary, life is necessarily the original principle, and the body one of its particular phænomena, conceived therefore not as permanent but as perpetually changing; and this idea of it is conveyed in the term *formation*, inasmuch as it signifies a thing not only formed but forming itself. We know, for example, that the human body after a series of years is a very different thing from what it was at an earlier period; nay, that the body of the adult does not contain even a single atom of that which constituted the fœtus; and nevertheless, that the internal, the living principle, the *man*, as every one's consciousness undeniably assures him, is still the same, nothing being changed but the phænomena of life, among which, as we have already shown, the body is to be included.

As it follows from the foregoing observations that life is not a single isolated reality, we shall be obliged to define it generally as the constant manifestation of an ideal unity through a real multiplicity, that is, the manifestation of an internal principle or law through outward forms. This view of the subject will indeed derive additional light from the analogous character of that inward principle which we call *soul*, inasmuch as this also consists not in this or that particular thought, or in the mere succession of our thoughts, or anything else of the sort, but in the whole spiritual life in general, that is, in the constant revelation and manifestation of an internal unity—of the deepest consciousness of the individual identity through an infinite variety of sensations and ideas.

If we now cast a look on that universal nature which surrounds us, the endless multiplicity of its phænomena is indisputably manifest; and as it would be an absurdity to imagine a highest number to which another number cannot be added, we can fix the limits of nature nowhere, either in the great or in the small, because the infinite divisibility of each would lead again to infinity. These infinities are nevertheless included in the comprehensiveness of the *whole*; there is but one *whole*, (the word has no plural form in our language,) and the idea of this necessarily contains at the same time the internal multiplicity, or rather infinity; for it would be a manifest inconsistency to conceive of a real *whole* as a unity, while in its strict *reality* it implies rather the idea of an infinity of individuals. Thus we find in fact the idea of life, that is, the constant manifestation of unity through multiplicity, exhibited by universal nature; and are therefore bound to consider nature collectively as one vast and infinite life, in which, though the extinction of any one of its various modifications, or the merging of a single external form of life in the universal life, is possible, an absolute and proper death is inconceivable.

Proceeding from this general view to the consideration of single beings, we perceive that all those individuals, so far as they are integrant parts of universal nature, must partake more or less of its essential properties,—that whatever is essential to the one must be partially repeated in the other. Every natural being must therefore appear, like nature in general, partly as a unity (in which light only it is an individual), and partly as a multiplicity, in which light it is infinitely divisible*, and its action and reaction upon the different other individuals may also be infinite. At the same time it still further appears that such an individual approaches more nearly to general nature in proportion as the multiplicity manifesting itself in its unity is more comprehensive and striking. A substance therefore (a geometrical body, for instance,) which is merely multiform and infinitely divisible in space but immutable in time, has far less claim to this affinity than a body, such as that of a plant or an animal, which changes in time also because of its continued growth and progress toward an independent life. Although, as has been shown already, the idea of life is in full and perfect accordance with universal nature, and consequently no natural body can, in this general view, be accounted anything more than a living member of the whole, yet there is a vast difference perceptible between individuals, inasmuch as the collective idea of life, a life proper to themselves, manifests itself in some, while others are less independent and can be recognised only as necessary parts of other individuals.

Now it is clear that the idea of life and that of an organism are essentially the same; for any unity that continually develops itself inwardly and outwardly into a real multiplicity is named—so far as it produces means, that is to say, instruments or organs suited to its own development—an *organism* or *organized body*, and everything belonging to it is termed *organic*. Its action is therefore named *organic* life, and that which is generated in space by this living action the *organic* body. Universal nature is consequently to be considered as the highest, the most complete, the original organism; and *in* nature those individuals only are to be called organisms which, as unities under certain external conditions, that is in their relation to other natural unities, continually develop themselves inwardly and outwardly into a real multiplicity. Among such organisms the most prominent are those bodies which, including our planet, constitute the system of the universe, and display themselves in continual motion and formation; those on our planet consist of plants and animals.

Now, as in an animal a piece of bone, muscle, or skin, and in a plant a fragment of the wood, leaf, or fruit, may be considered as organic, but cannot be called an organism, all substances, except plants and animals, observable in and upon the body of the earth, so far indeed as they are parts of the terrestrial organism, are to be regarded as organic and as parts of a living thing, but not as organisms possessing an independent life.

According to this view we must include among those things which do not as unities develop themselves into multiplicity,—that is, among

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* [An investigation of this part of the subject will be found in a paper "*On the Origin and Production of Matter, and on its alleged Infinite Divisibility,*" in the Philosophical Magazine, First Series, vol. lxii. p. 360, *et seq.* See also vol. lxiii. p. 372.—EDIT.]

Inorganisms,—1st, all substances which, though infinitely divisible, being but mechanically so, are incapable of being developed into various parts and of maintaining at the same time their individual existence; and therefore all elementary bodies, such for instance as oxygen, hydrogen, carbon, the metals, sulphur, &c. 2nd, All substances whose resolution or development into their elements annihilates their individual existence; as, for instance, water, which, as soon as it is decomposed by the influence of galvanism into oxygen and hydrogen gas, ceases to be water,—widely differing in this respect from the plant, which, when it develops itself into leaves, branches, flowers, and fruit, remains still the same plant, or rather becomes then for the first time completely a plant. To these we must add the acids, salts, &c., nay, the constituent parts of organic bodies themselves, which being resolved into their elements are, as organic bodies, utterly destroyed. 3rd, All bodies which owe not their existence and multiplicity to spontaneous development, but are composed by nature or by art out of materials already prepared; for instance, floating islands, buildings formed by animals, all automata, machines, &c.

But as we find in real organisms single subordinate parts or organs, which in a certain degree reproduce the idea of the whole; nay, as we see that in less perfect organisms that bond of unity which holds the developed parts together is yet so feeble that if it is separated the part appears to be really a whole, (for instance, the shoot of a plant separated from its parent often becomes a new plant, and the parts of a polypus become new polypi,) so do we not unfrequently observe the idea of the living thing to which they belong reproduced to a certain extent by natural bodies which, so far as they are parts of a greater organism, have not the appearance of being organisms themselves. Of this fact we have an instance in the formation of a water-drop, which, as manifesting a certain force of gravity or tendency to internal unity, is essentially analogous to the spherical formation of the heavenly bodies; and in crystallization, the growth of metals, &c. we see a repetition of the process by which the earth was formed out of fluids. If we turn our attention to these intimations of individual life in unorganized bodies, the idea of the living principle pervading all nature presents itself anew and more distinctly to our minds, and we are forced to admit the relations of the unorganized to organized bodies, which could exist only in this connexion and under their other relations to universal nature. From all this we are finally led to infer the universal connexion, the combination, the never-ceasing action and reaction of all the powers of nature, sometimes in sympathy, sometimes in antipathy, as necessary to the production of an immeasurably vast and magnificent whole,—an action and reaction which would be impossible, were not all originally pervaded by one living principle, were not all in this respect similar and allied to each other.

From these general conclusions we proceed to a survey of the different kingdoms of nature, in order to submit to a closer examination the peculiarity of their life and their mutual relations. For this purpose we must define more exactly the boundaries of each separate kingdom. Here we must first make a distinction between the celestial and the terrestrial bodies. To the former belong the solar systems, including the earth considered as a planet. The idea of terrestrial bodies comprehends all the different single objects perceptible by the senses in and on the earth. Now, terrestrial bodies, according to their appearing or not appearing as independent organisms, form two principal classes, and this leads to a second division of bodies into the organized and unorganized. We divide the inorganic bodies likewise, as far as we consider them members of the planet, into the constituent parts of the body of the earth, and the constituent parts of the atmosphere, viz. 1. Fossils and liquids; 2. Gases and vapours. The organic bodies are divided into vegetable and animal bodies. We have therefore four kingdoms of nature, and four different departments of natural philosophy belonging to them. The kingdom of the earth (Geology); the kingdom of the air (Atmospherology); the vegetable kingdom (Phytology, Botany); the animal kingdom (Zoology).

Of the Inorganic Kingdom.

The great elementary masses of the earth are formed and governed by many powers, among which we may distinguish those which relate to the individual preservation of the planet, from those which originate in other heavenly bodies. Of the former the most remarkable is Gravitation, which manifests itself as the immediate principle of internal unity, the *sensible* tendency of all parts of the earth to a common centre, and therefore to an *ideal* unity, since according to Euclid no point can be represented materially. But another effect presenting itself in the *visible* relation of the earth to other heavenly bodies, is that which we perceive under the form of Light. These two powers, when united, produce other phænomena; for instance, heat, which results from the opposition between the rays of light and the direction of gravity: wherefore we observe the heat of the earth to be more intense, the more the heated body is found to be in a straight line between the illuminating and the illuminated object, that is, between the centre of the sun and that of the earth. To these also belongs the phænomenon called magnetism, as the effect of the gravitation of the earth, and its position with respect to the other planets, i. e. the direction of its axis. In heat the predominant principle is light; whereas the predominant principle in magnetism is gravitation. To these we find new powers still added, among which the mechanical and the chemical appear to be allied to gravitation and magnetism, (for the laws of mechanics are essentially connected with those of gravitation, as the chemical laws include the compositions and decompositions, the attractions and repulsions of ponderable bodies,) while electricity and galvanism, on the contrary, being more connected with light and heat, are found less inherent in terrestrial substances. A body which is an electric or galvanic conductor can be conceived to exist without electric or galvanic power; whereas no earthly substance can be imagined without the chemical effects proper to its composition, and the mechanical operations proper to its form. It is moreover worthy of remark how all this series of powers, which constitute in their totality the life of the planet, is found also in its single parts constantly and in the most various forms; for we find in every object a proportionate gravitation of the mass toward its centre. This fact explains the mutual attraction of two bodies floating in a fluid, the formation of a drop of water, and the nature of the globular form in general as one in which all the radii, or the relations of the periphery to its centre, are equal. It explains also the mutual illumination of single terrestrial bodies; the production of heat as the result of the collision of different bodies; the manifestation of electricity, not only in the stormy atmosphere, but also in resin and glass; and the manifestation of terrestrial magnetism in the smallest bar of iron. These objects, for the complete examination of their endless variety and eternal regularity, require a full development of the laws of chemistry and natural philosophy,—a development which would exceed the limits of this treatise as much as it does the powers of the author, and which, in its full and scientific comprehensiveness, is still a desideratum.

But our purpose demands a particular examination of the relation of water to the other atmospherical and terrestrial substances, more particularly because it forms, as we shall show, the most essential link between organized and unorganized bodies, or rather the constant source from which the former arise. Water considered in its *threefold* form, as solid, fluid, and gaseous, presents a true middle and connecting member between the planet and its atmosphere. It may be considered as the *indifference* of both,—on which fact depends its decomposition into a combustible element (hydrogen), and an element promoting combustion (oxygen), nay, it is in its purity *really* indifferent in respect to the other terrestrial as well as to atmospherical substances. But the manifold in nature, however far back in point of time we trace its origin, will be found constantly issuing out of the simple and indifferent; and on this very account water, as far as it appears an *indifference*, becomes the germ and source of an infinity of other forms; indeed it is a question whether we are not already justified in supposing, and whether further inquiries will not establish the fact, that both the planet and its atmosphere are but different developments of one and the same original fluid. Several of the older chemists (Leidenfrost, Wallerius, Markgraff) have attempted to show, that even now, during certain chemical processes, particles of water are changed into earth; and though Lavoisier has sufficiently refuted that opinion, he has not demonstrated the impossibility of the decomposition of an *original* fluid into water, air, and earth[*]. That water is of the utmost importance in the general formation of the earth, has been proved beyond doubt by the excellent experiments of the immortal Werner; and we are justified in continuing still to believe in its importance to the preservation and life of the planet, when we take into consideration both its quantity and its continual motion. In regard to its quantity, we find that of the sum total of the surface of the globe (9,000,000 square miles,) the water occupies nearly 6,500,000 and the land only 2,500,000[†]. The water is so deep also that several points of the sea are unfathomable, although latterly it has been fathomed to a depth amounting to 4600 feet. The motion of the water, on the other hand, depends partly on gravitation, as in the running of rivers and streams; partly on the attraction of other planets, (viz. the sun and moon,) as in its ebbing and flowing in the tides[‡], and in its ascending and descending between the earth and sky in the form of vapour, dew, rain, snow, &c. Comparing animal with planetary life, we are therefore led to conclude, that as a homogeneous fluid, in continual circulation, the blood, is the source in which all forms and reproductions of the organism originate, so is water one of the members most important to the life of the earth. This internal life of the fluid becomes indeed more evident when we consider the individual formations of the solid to which it gives birth. The most striking illustration of this is the process of crystallization, which exhibits a near approach of the inorganic to the organic life; for we cannot deny, even to the crystal, a certain inward peculiar life at the moment of its formation. The only difference between an organic body and a crystal is, that the life of the latter, the principle of action and reaction, terminates as soon as its formation is accomplished. One might be tempted to say that the crystal lives only to form itself; for as soon as it is formed it dies; while true organisms, on the contrary, form themselves only in order to live, and it is only when they are perfectly formed that their life is truly and properly evident. But the formation of the crystals, as a process nearly allied to organic life, is not the only phænomenon remarkable in them. The very forms of the crystal are, in their approximation to the form of the organized being, well worthy of a closer attention. We find in all earthy, as well as in many metallic or combustible fossils, the purely geometrical form of the crystal, which, in proportion as it is more compact, and presents a more limited coincidence

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[*] See the experiments of J. F. W. Otto's System in an Universal Hydrography of the Earth. Berlin, 1800.

[†] See Kant's Physic. Geograph., edited by Rink, Pt. I. p. 61.

[‡] See Otto's Universal Hydrography, p. 520—550.

of surface with other forms, approaches more nearly the spherical form, as that which is perfectly compact, thoroughly symmetrical, and therefore fundamentally organic. The icosahedron, for instance, approaches the spherical form more nearly than the octahedron does; it is also important to observe that the most precious crystals, and especially the diamond, (which being pure carbon, is therefore, from its composition, most closely allied to the organized bodies,) are those wherein we observe the most compact crystallization, at least that which approaches most nearly to the sphere and is therefore in nowise columnar; wherefore the diamond, particularly on account of its power of refraction, has a closer resemblance to a solid drop of water*. This view, by showing how crystallization may be examined, from the three-sided pyramid and the cube upwards to the most many-sided forms, or those which approach nearest to the sphere, may place the theory of crystallization on a more natural and therefore a more philosophical basis. On the other hand we must also take into consideration the copies, or rather the prototypes, of the form of really organized bodies which occur in the solidification of the fluid. It is by no means without a cause, nor to be regarded as a mere *lusus naturæ*, (a very unmeaning expression,) that pure water in its crystallization assumes forms which correspond most closely with those of inferior organizations: thus the flakes of snow represent the forms of Polypi, Asteriæ, and Medusæ; we find in the ice on windows the forms of many vegetable substances, leaves, stems, flowers; the earth too and some metallic substances present, when melted or united with water, similar types, in which we see the condition under which Dendrites and the manifold forms of native ores originate. In all this the moving creating life of the original fluid cannot pass unnoticed, and becomes still more evident if we examine the history of the origin of organized bodies, in which the fluid appears as the basis both of animal and vegetable life; and thus the very germ of individual organisms is intimately connected with the life of the planet. Indeed this is partly true of the solid parts of the earth; for it is easy to show, even in the fossil kingdom, a transition partly to animal and partly to vegetable life; so much so, that a philosophical inquirer, Henry Steffens†, has been led, from a comparison of several facts, to establish two very probable propositions relating to this subject:

"1. In the whole silicious series (of fossils),—which constitutes the chief mass in the oldest and principal mountains of our earth, which goes through all periods, and in its bituminous substances exhibits the remains of an extinct vegetation, yet connected as a living member with the whole existing vegetation by the marsh-turf,—carbon and hydrogen (the essential elements of the vegetable kingdom) are the principal

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* On the formation of the water-drop, see p. 229.

† See *Beitrage zur Naturgeschichte der Erde*: pp. 58 and 69. Freyberg, 1801.

characteristics. 2. In the whole calcareous series,—which begins in the oldest mountains of our earth, proceeds through all periods, is of the greatest magnitude in those of the latest formation, presents in the petrifactions the relics of an extinct animalization, and is connected as a living member (in the coral-banks) with the existing animal world,—nitrogen and hydrogen (the essential elements of the animal world) are the principal characteristics.

The connexion between inorganic and organic life, however, is shown more immediately and more clearly in the production of organisms from pure water; to which we must refer both the origin of the Infusoria obtained by the pouring of water over mineral substances, as observed by Gruithuisen, and still more, the origin of the so-called *green matter*, the history of which has been so admirably traced by Priestley and Ingenhousz. These show more than all other experiments, that in the purest water, under the free influence of air, light, and heat, beings are formed which, oscillating as it were between the animal and the plant, exhibit the primitive germs of both kingdoms. The succession of the changes which take place in the formation of the Infusoria is of such importance that we cannot avoid considering them more in detail, and therefore select from the acute G. R. Treviranus * the following passage in reference to those cases.

"If we expose spring water † to the sun in open, or even in closed but transparent vessels, after a few days bubbles rise from the bottom, or from the sides of the vessel, and a green crust is formed at the same time. Upon observing this crust through a microscope, we discover a mass of green particles, generally of a round or oval form, very minute, and overlaid with a transparent mucous covering, some of them moving freely, whilst others perfectly similar to these remain motionless and attached to the sides of the vessel. This motion is sometimes greater than at others. The animalcules frequently lie as if torpid, but soon recover their former activity.

"As the corpuscles constantly become more numerous, the crust increases likewise. After a few weeks the latter acquires a certain thickness and consistency. If we examine it in this state, it appears exactly as described by Priestley. It looks in fact like a slimy sediment of the water, which has become green under the influence of the sun without presenting any trace of organization. The green particles, which were visible at the time of their formation, are now so crowded together, and perhaps so changed in their organization, that the most attentive observer, unless he had closely followed their metamorphosis step by step, would hardly be able to discover the traces of their primitive form.

"A few weeks later, when the crust has assumed a still greater con-

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* *Biologie*, vol. ii. p. 302.

† Similar results are obtained from distilled water, though far more rapidly when mixed with organic substances.

sistency, it appears to have become a confused mass, or an indurated green mucus. When the mass is broken and observed through a good microscope, the original green corpuscles appear again, but changed in form, enveloped in a slimy matter, and interwoven with small transparent threads resembling slender colourless glass tubes, and show irregular yet visible movements. They approach each other, return again to their former position, become entangled with each other, and again disentangle themselves. If observed at the instant when such movements occur with the greatest energy, these little filaments have all the appearance of diminutive eels; in fact they are in some degree similar to the small vermiculations observed in vinegar. We may often discover in them even peristaltic motions. The white colour and the motion of these filaments last but a certain time. After a few weeks more, the crust becomes more solid, uneven, and raised here and there into irregular protuberances. The threads (or filaments) become more distinct; they are green, and scattered about without order, chiefly on the most prominent part of the crust, without however rising over its surface, which remains smooth and rather hard to the touch. The crust itself presents scarcely any traces of the original animalcules.

"If the crust be left undisturbed, and the water be now and then, but seldom, renewed, the unevennesses of the crust increase and rise in a pyramidal form. As soon as the pyramids are formed, the green threads, winding irregularly through the unevennesses of the green crust, rise also, become developed, and dispose themselves along the pyramidal bodies, toward the upper parts of which they become particularly visible; the rest is of a gelatinous substance, of a sufficient consistence to maintain its form as long as it remains under water. If these productions belong to the class of zoophytes, they must be ranked among the Tremellæ."

Some have indeed denied the actual production of organized from unorganized matter, since distilled water over quicksilver does not produce any green matter. But in the first place it is not easy to see why a metamorphosis should not be regarded as such because it occurs only under certain given circumstances; in the second place, it is also very possible that in a process so little favoured even by pure water, the quicksilver, on account of its property of counteracting production (a property which renders it so useful as medicine), may destroy or prevent the infusorial fermentation, as it has been called.

We think therefore that we are not in error when (combining the consideration of these important changes with our general inquiries into unorganized matter,) we recur to the proposition we have before laid down, viz. that the multiplicity of the phænomena of nature rests upon one *unity*; that nature therefore nowhere presents either an absolute difference (for such changes would then be inexplicable), or an absolute identity; and consequently, if we give the name of *sub-*

stance to the real, or that which is the condition of the phænomena of nature, this eternal substance causes by a continual metamorphosis the appearance and disappearance, the perpetual change of natural objects; a real creation and annihilation being as inconceivable as a limit to universal nature.

Of the Organic Kingdom.

The animal stands in the same relation to the vegetable kingdom as organized bodies in general do to the unorganized, that is, as unities unfolding themselves into multiplicity; for as in the activity of individual terrestrial organisms we observe not merely a power peculiar to them as organisms, viz. organic life, but likewise that activity which appertains to them as parts of universal nature, viz. physical life, gravitation, chemical properties, &c.: so also we find in the animal kingdom, besides the life peculiar to animals, the properties peculiar to vegetation. But further, according to our previous inquiries, so little difference can we trace between the unorganized and the organized in their essence and their various relations, that the organized merely presents the unorganized body in higher power, and in closer unity, and in more perfect independence. In like manner the absolute and essential difference between an animal and a plant is so little, that the animal is to be considered only as a plant which has attained a more complete unity, independence, freedom, and power; which will be more satisfactorily proved in the following pages, where we intend to submit the life of plants, as well as that of animals, to a closer examination.

The Vegetable Kingdom.

Speaking of the crystal, we stated that it forms itself by an inward *living principle*, but that when formed it appears deprived of individual life; whilst organisms, on the contrary, (though to be considered as in a state of continual transformation and growth,) first manifest their real life when they are completely developed: In the same manner we may say of the plant when compared with the animal, that though the plant be in one view formed in order to live, yet even when developed it strives only after a progressive organic formation and real development as the highest aim of its life; whilst, on the other hand, the whole end of the activity of animal life is not mere organic formation, but also free self-determination and ideal development. A proposition which may be also thus briefly expressed: If in universal nature, and in every individual that forms a part of the universe, we must distinguish between the internal unity or law, and external multiplicity or sensible phænomena, we find that in the plant the multiplicity overbalances the unity; in the animal, on the contrary, the unity overbalances the multiplicity. But since a body which possesses less unity is thereby more precisely marked as an integrant part of a superior whole, and, on the contrary, a body possessing greater internal unity appears to be on that account more a whole in itself, hence we know why the plant is from necessity more closely connected with the organism of the earth than the animal; considered in which point of view, the principal peculiarities of vegetable organization are capable of a general explanation.

As the first consequence of the above fundamental peculiarity, we have to consider the division of the plant according to the direction of the two principal properties of the terrestrial organism, that is, in its tendency to inward unity (gravitation), and in its relation to the higher natural bodies (light). In this point of view, the plant must be regarded as consisting of two parts, the terrestrial and the aërial, the former consisting of the roots and stem, the latter of the leaves and flowers. From the division, or dualism, thus characterizing the plant, there follows also as a second consequence the want of internal unity in the formation of the plant in its relation to space. Moreover, while we see the animal endowed with different systems of organization, the one for absorption, assimilation, and secretion [*Stoffwechsel*], the other for sensation and motion, and the first system inclosed within the second in the form of intestines; the plant, on the contrary, wants the intestines properly so called, and possesses nothing to correspond with the absorbing and assimilating intestines of the animal, but that which we call the root; so that while the animal, as a unity in relation to space, exists one half *within* the other, the plant, on the contrary, as a duality in relation to space, appears one half *upon* the other. Hence we may moreover infer the original homogeneity of both halves; and this circumstance renders the reversion of their functions possible, so that the branch is converted into a root, and the root into branch, leaves, flowers, &c., as is proved by experiment. A third consequence is, that as the union of two points appears as a line, the line is the archetype of the plant; while, on the contrary, (as we shall show hereafter,) the globular form is the archetype of the animal body. The root, being subject to the law of gravitation, strikes downward toward the centre of the earth; the stem, the leaves, and the flowers, on the contrary, follow the light, and rise in the opposite direction, so that the whole represents a perpendicular line. The experiments instituted by Count Buquoi, in order to ascertain the constancy of these directions under unusual external circumstances, are in this respect well worthy of attention *. Seeds were put into a layer of mould lying loose at the top and bottom; but, though placed closer to the lower surface, instead of growing out of this, they pierced through the far stiffer part of the layer, so as to grow out of its surface. Plants which were set upside down in a flowerpot always bent their flower-stem around the edge of the pot, and grew upwards. A fourth consequence of that fundamental property of the plant is its

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* *Skizzen zu einem Gesetzbuche der Natur.* Leipzig, 1817, p. 315.

fixing itself upon a given spot; while, on the other hand, locomotion is the characteristic of the animal kingdom. For there is no comparison between plants taking root, and the adhesion of some animals, corals, and oysters, to the ground by means of their shells. In the latter case there is not, as there is in that of the plant, an active dynamical intrusion into the maternal bosom of the earth for the sake of nourishment and life, but a mere mechanical hold of the surface. A fifth consequence is the more marked dependence of vegetable life on the life of the earth. Whether the vegetative organization awakes and develops itself, or sleeps and dies, depends accordingly on the position of the planet with respect to the sun and other heavenly bodies, as well as on the peculiar development of the soil. Though these circumstances affect animal life also, it is not to be denied that they do so in a far inferior degree, and that the progress of animal organization imparts an independence of which the plant is utterly incapable. As the sixth and last consequence arising from the less perfect unity of the plant, we are to consider not only the dualism already mentioned, but the peculiar nature of every bud; and every internodium may be considered as a whole in itself, or in some measure an individual plant; wherefore a bush or a tree is more properly compared to an aggregate of animals (a coral bank) than to a single animal. In this way we shall easily comprehend the various modes of propagating plants, in which a bud (an eye) and the shoots that issue from it renew the parent organism, and that which we see in the bud is exhibited likewise as tubercles in the root or also (as in the genus Allium) near the flower, or as the bulb, and always possessing the power of reproducing the whole plant out of itself; nay the very seed is but an improved and more perfectly compact picture of the bud.

If we closely examine the structure and composition of plants, we find that, like the organism of the earth itself, they contain solid, fluid, and gaseous elementary particles. We see that in the plant, as well as in the earth, the fluid contributes to the formation of the solid parts, and that the finer and therefore more destructible organization of the plant is composed of chemical elements, namely, the carbonic, hydrogen, and oxygen gases. The transition of the fluids into solids, and consequently the history of the formation of the proper body of the plant itself, is evident in its primary structure, that is, in its cellular tissue. If we call to our recollection the history of the primitive formation of the rudiments of organic bodies in the green matter of Priestley, and see in this the conditions of this formation,—whilst, under the influence of light and gravitation, some particles of the original fluid attain the nature of individual beings, as well as a tendency to internal unity, and consequently a globular form,—it becomes clear that this development cannot occur without a separation of those particles from the rest, without an *individual limitation* in form of a spherical surface; so that the rudiment of animal life appears to be a hollow globular body. Consequently when the effort to attain a higher unity or a more perfect organization presses several of these globuli, possessing solid though weak surfaces, one toward the other, the surfaces, by mutual pressure, are necessarily modified into different geometrical angular bodies*. In the most imperfect plants we observe, as a consequence of an imperfectly developed internal structure, that in their single cells, which press each other but slightly, the globular form prevails, although on account of the linear direction peculiar to plants (see p. 235) it is elongated into the ellipsoid; while in the more perfect plants, on the contrary, the single cells of their tissue appear, in consequence of the mutual pressure, in the form of regular dodecahedrons. Looking upon the cellular formation as the basis of the whole plant, and considering that the plant itself in its primitive destination is dependent on its relation to the planet and its unity (gravitation), we are fully entitled to identify the anatomical system of the cellular tissue†, as the proper reproductive system of the plant, with terrestrial gravitation and the planetary body itself, inasmuch as that principle may be considered the basis of the whole organism of the earth. But since, in the organism of the earth, light and air, as constituting a second integrant part, stand opposed to gravitation [*der Nachtseite*], and since the plant bears a relation not only to gravitation but to light also (see p. 235) when its formation is complete, it will necessarily present a second anatomical system, namely that of the spiral vessels, which have been very justly considered of late as the organs that perform in plants the functions of nerves. The lower plants, which want no light for their development, are not provided with spiral vessels; in the more perfect plants, on the contrary, the spiral vessels are as essential a part of the organization as the cellular tissue. In fine, between both these systems of the cellular tissue and the spiral vessels (the earth and water system, and the light and air system, as they are called by Kieser†,) the epidermis stands as a binding and connecting member, whose vessels appear to be the more perfect intercellular ducts, and its pores the orifices of these vessels‡.

As the anatomical systems of plants are therefore but very few, the multiplicity of their external organs, which unfolds itself in the most beautiful progression and regularity, is so much the more important. Whilst the root, penetrating more deeply in the direction of the earth, spreads itself with uniformity, the plant elevates itself more and more into the light, and attains a more delicate and perfect organization; in which process it is a fact deserving most particular attention, that this perfectibility does not manifest itself in the production of new organs

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* Kieser's *Grundzüge der Anatomie der Planzen.* Jena, 1815, p. 9.

† Ibid., pp. 16—19.

‡ Ibid., p. 19.

entirely different from the former, but in a continually progressive transformation of the original types, a succession of metamorphoses, on the nature of which we have received the most interesting information in the excellent observations of Goethe*. It appears that while the first rough type, as it were, of the whole plant is contained in the coatings or leaves of the seed (cotyledons), which abound with a gross and yet unelaborated sap; the same type is manifested more plainly in the successive divisions of the stem (internodia), and in the leaves, in which, when we compare the upper with the lower leaves which surround the stem, its progressive improvement becomes very distinctly evident. As soon as the plant has formed its leaves, which perform the functions of the organs of respiration and secretion, and has thus purified its fluids, it goes on to produce the flower, which is its most complete organ, entirely under the influence of the light. Even this transition is not performed suddenly, but is prepared by the formation of the calyx, wherein the leaves of the stem begin to contract themselves, while they unite in greater numbers around a common axis in the same plane: this formation shows itself most evidently in the collective calyx of flowers belonging to the class Syngenesia, in which the pappus performs the function of the calyx of single flowers. Moreover, the calyx itself constitutes the most evident transition to the corolla, the functions of which it often performs; and the corolla is only a finer calyx for the organs of generation, which, as the most compact and perfect organs, issue forth from their last organ of development and preparation, as from a covering which they have last thrown aside. It is a remarkable fact, and one which places the correctness of these views beyond doubt, that too rich a nourishment, and the accumulation of too many fluids not yet properly purified, may cause a retrograde organization of these parts; the organs of generation may be transformed into flower-leaves (as in double flowers), the leaves of the calyx may be changed back again into leaves of the stem (as is often the case in the calyx of the rose), and instead of the organs of generation, a new shoot or internodium, bearing a new flower, may appear (as in the proliferous roses or Rose-kings†). When, after such successive progression, the plant has reached the highest point of polarity between root and flower (gravitation and light), between which the stem and leaves may be considered as mere connecting links, similar in their function to that of the epidermis between the cellular system and the system of the spiral vessels, the same opposition appears once more under the form of male and female stamina; the latter of which, as containing the germ of a new plant (the seed), belong more to the reproductive system, and stand more under the influence of the earth. Wherefore the inferior plants, such as

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* *Versuch die Metamorphose der Planzen zu erklären.* Gotha, 1790. Reprinted in the *Hefte zur Naturwissenschaft und Morphologie*, 1817, i.

[† *Rosenkönigen*, Germ.]

mushrooms, ferns, &c., produce their seeds immediately without the aid of the male stamina, and this circumstance accords with their texture, which is merely composed of cellular tissue. On the other hand, the male stamina, containing a generating life-imparting principle, that is, the operation of light, come nearer to animal nature. This view is in perfect accordance with the power of motion which is often to be observed in these parts, as well as with the very probable hypothesis, that the cause of the scent and of the colour of flowers may be traced to the elements of the male pollen*, which is contained in their leaves. We have already stated that the seed itself being an *indifference* emanating from this highest polarity, contains the most concentrated image of the bud. As it has thus within itself in idea the whole organism of the plant, it is capable of reproducing in reality the whole plant out of itself.

Proceeding from this short survey of the principal phænomena of the development of plants to a further examination of their active manifestation of life, we shall find that even in this respect the vegetable kingdom, as a part of universal life, is connected with inorganic nature. It has been already observed that the life of the plant consists chiefly in the formation of its organs; whence it follows, that its most essential and fundamental activity manifests itself in the process of assimilation and secretion, as well as in the circulation of the sap, which is nothing but a repetition of the chemical attraction and repulsion observed in unorganized matter. But since the circulation of the sap is not effected by any independent peculiar organ of circulation, (such, for instance, as a kind of heart,) we must suppose this movement to be, like the ebbing and flowing of the tides, the effect of a certain attraction, partly originating in the structure of the plant, and partly in its external relations; unless we should prefer ascribing it entirely to the motion of fluids in capillary vessels, that is, in other words, to the laws of capillary attraction. But the laws of capillarity have surely but a limited influence in this case: capillarity may indeed enable us to explain the phænomenon of the rising of fluids, but not their progressive motion, and still less the flowing off of the sap when the plant is cut or injured; because a capillary tube never can overflow, and that for the very cause which makes fluids ascend, namely, their adhesion to the inner surface of the vessel. Hence, although capillary attraction has some share in the circulation of the plant, it is evident that this depends upon some higher cause. It has been already shown that the polarity of the plant between root and flower, which depends on the elementary polarity between gravitation and light, is also visible in the relation of the functions of both those parts, the root being particularly adapted to attraction and absorption, but less fit for secretion, and the

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* Goethe's *Morphologie*, p. 23.

leaves and flowers being particularly capable of secretion and expiration, but less fit for absorption. Polarity is therefore the cause which brings the sap into motion by reciprocal attraction and repulsion from the root to the leaves and flowers, and from the leaves and flowers again to the root: a motion on which, moreover, the physical powers—which, as the condition of both these parts, we have named vegetative poles, namely the powers of light and gravitation—must have a most decided influence; since, for instance, it is a well-known fact, that the perspiration of plants is very different according to the degrees in which they are exposed or withdrawn from the light of the sun. But besides those active properties which contribute to the organic formation of plants, some of them possess a peculiar mobility, which seems to arise from real sensibility, and at the first glance presents a perfect line of demarcation between the vegetable and inorganic bodies. In order to have a clear insight into this fact, it is necessary to fix our idea of the word *sensibility*, as that which we would be understood to convey most correctly, if we say that it consists *in the change operated by outward or inward circumstances in the feelings of a being conscious that it exists as a unity*; consequently if we deny sensibility to the stone or the mineral, it is not because such a body is not subject to the most various agitations and changes, but because it is merely a member of a higher unity, and in itself is to be considered as an individual, not as a true unity. In regard to the plant we may say that it has become an organic unity; but on account of the dualism (see p. 235.) prevailing in its totality, and its being therefore bound as it were to the external world, we may with safety deny that it is conscious of its own unity; for in order to have self-consciousness, or an internal perception of unity, there must be, not merely that *ideal* unity which belongs to organized beings in general, but that *real* manifestation of unity which arises from the continual action and reaction of all the organs and an organic centre. But such action and reaction are not to be found in the plant, in which each bud may be considered as a whole; so that this real unity, as we shall hereafter see, is possible only in the animal, in which the organs are connected with a unity by means of the vascular and nervous systems. But if we cannot suppose plants to be possessed of sensibility, how can we account for their movements towards the light, the shrinking of the sensitive plant from the touch, the closing of the Dionæa by mechanical irritation, or the inclining of the stamina towards the stigma, and the regular embracing of extraneous bodies, and in definite directions, by the creeping plants, &c.? In our opinion all these phænomena are to be accounted for in the same way as the rotation of the earth, the motion of falling bodies, the oscillation of the sea in its ebbing and flowing, the attraction and repulsion of the cork balls in the electrometer; that is to say, we think that they are entirely the effects of external disposing causes, and therefore the consequence of a susceptibility or capacity to be effected by those external causes. This property, so far as it is conducive to the excitement of organic activity, has been named *irritability*, but always without annexing to the word a sufficiently precise idea. We think it therefore not superfluous to illustrate it by some examples; when, for instance, a body is to be put in motion by an impulse, it is necessary that it possess mobility, or the capacity to be put in motion. It is the same with chemical operations: in order that a body be acted upon and decomposed by another body, it is requisite that the former be susceptible of this chemical action. The case is the same if an organism as a whole, comprehended in an enduring form, is to be affected by external influences; except that we must here distinguish whether the activity called forth by this influence appears as a change in its physical properties, for instance in its extension; or as a change in its own organic activity, its formation; or in the mutual relation of its single parts. In the former case we name this property a physical receptivity, in the second irritability, and the exciting power a *stimulant*; from which it is clear that the same influence can act both as a mere physical power and as a stimulant: heat, for instance, can expand a body, and at the same time quicken its organic formation or growth; in the latter case it acts as a stimulant. Hence it follows that the irritability of the plant stands in the same relation to animal sensibility, as its own physical receptivity stands to its irritability. While the plant therefore, from being indebted for its own movements to the influence of external causes, approaches more nearly to universal nature, and is therefore further separated from animal life, to which it approximates again in the inclining of the stamina towards the stigma, this movement, though independent of its own will, originates in an attraction inherent in the plant itself.

Having hitherto been occupied in considering the influence of the organization of the earth upon plants, it necessarily follows that we should consider the influence which the vegetable kingdom exercises upon the life of the earth; for even though we should not be inclined to consider that the origin of the vegetable kingdom in general necessarily marks an important epoch in the formation of the earth,—as for instance, in the development of the plant, the production of a single organ (as the flower) from a particular influence is to the whole plant,—yet the transformation of vast masses of vegetable substances into strata of pit and Bovey coals, into strata of turf and of vegetable mould, and particularly the influence of living vegetation upon the surface of the earth and atmosphere, are objects too striking to pass unnoticed. In the latter point of view it is particularly worthy of remark, that the origin of brooks and streams is owing to the existence of woody mountains, and their greater attraction of atmospheric vapours; wherefore we often see streams dried up, on account of the destruction of the forests in which their sources lay; a circumstance to which modern travellers ascribe the present dry desert state of Greece, in which several streams celebrated by the ancients have totally disappeared, leaving behind a dry and barren soil, because the woods which contained their springs were wasted and destroyed through barbarism and neglect. If, lastly, we consider how essential an influence the course and deposition of rivers have upon the surface of the earth; how far all countries have been produced by their rivers (as, for instance, Lower Egypt by the Nile, or the regions of America, towards the lower part of the Mississippi, by the alluviations of this river); we find here again the bond of mutual relation and affinity which connects organized and unorganized terrestrial bodies by means of vegetable life manifested with sufficient distinctness.

The Animal Kingdom.

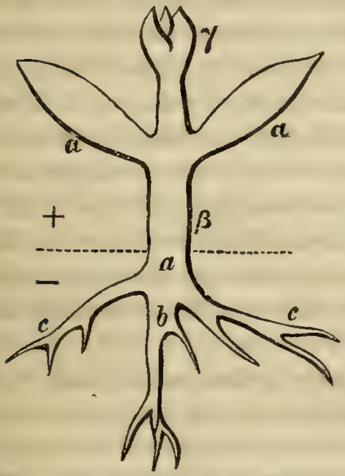
As the plant may be considered a crystal continually developing itself in a constant change of its matter, in like manner the living animal body so nearly represents a plant which has reached a higher unity and faculty of self-determination, that although the animal still remains a part of a higher unity, and is closely bound to the earth by the necessities of life, yet this hold taken of the animal as compared with that taken of the plant, is even less in degree than that which we observe in the plant as compared with the unorganized body. For this very reason, the animal presents, among natural bodies, the most perfect idea of an organism (see p. 226); and as we can prove mathematically that there are only three fundamental numbers (which are continually repeated in all forms of perception, namely, unity, its division into duality, and the reunion of the unity and duality in trinity), which are exemplified in our conception of space through the threefold dimension of length, breadth, and thickness,—in like manner the threefold succession of inorganic vegetable and animal life exhibits the members which together afford the idea of an organism, viz. multiplicity, development, and unity.

Since the addition of the idea of unity constitutes the perfect idea of an organism, just as thickness, added to length and breadth, constitutes the idea of a body, it is evident that the unity of the animal body presents and affords in reality a perfect idea of an *individual* organism. We have already observed that the peculiarities of vegetable life may very well be collectively ascribed to its want of inward self-independence; in a similar way we may deduce all the peculiarities of the animal organism already alluded to, from the idea of *perfect unity* which is characteristic of animal life.

Consequence the first.—If the plant, exposed alike to gravitation and light, is divided into root and stem, into a terrestrial and an atmospheric part, the animal, being more independent, is less bound to the organism of the planet to which it originally belongs, and is consequently more under the influence of light; wherefore, though sometimes fettered to the earth, it is by no means fixed or rooted in it, to which circumstance it owes its faculty of locomotion.

A further consequence which flows from the above consideration is, that the animal cannot, like the plant, draw its nourishing juices from the soil, because its whole organism has a tendency to inward unity; its very root-organs are turned inwards and formed into intestines; from which we are able to show most evidently the origin of the excretory canal and of the absorbent and circulating vessels. Let us suppose for instance a plant A, living with the atmospheric part + aboveground, and with its terrestrial part – underground; let us now detach it from the ground so that all the fibres of its root, *a b c c*, having struck back into the internal parts of the stem and of the leaves, may be reversed inwards; we shall then have the figure B, in which the part subjected to the light perfectly encompasses that subjected to the earth; and *a b* appears as the alimentary duct; *b*, as the cavity of the stomach, and *c c* as the vessels for distributing the sap.

A.



B.

C.

Here we observe how very much in this metamorphosis the plant has assumed the type of the animal body, such as we observe it among the lower classes of animals. In this way we may now see why in the Medusa, the Sea-star, the Echinus, and other inferior kinds of animals, the aperture of the mouth is turned downwards, and the alimentary duct upwards*; or in this way we may see that the lower classes want the opposite or posterior opening of the excretory ducts (anus), or that (as is likewise particularly evident in the Medusa†) the vascular organs branch out immediately from the cavity of the stomach, and that the leaf-formed parts B, *α α*, furnish an explanation of the appearance of a kind of exter-

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* Carus, *Lehrbuch der Zootomie*, p.327.

† Ibid., p. 578.

nal respiratory organs or gills (nearly as in the Clio* and the Cleodora); or lastly, we may see that the germ γ grows opposite to the mouth, at the hinder extremity of the body, as the generator of a new internodium, that is (in the plant) as the producer of a new whole, or the organ of the plant's propagation; and we are thereby enabled to account for the usual place assigned by nature to the organs of generation. In this way we shall always recognise that which has hitherto escaped the attention of the observer, namely, the analogy that exists between the body of the animal and that of the plant as above metamorphosed, as well as the manifest derivation of the former from the latter.

A third consequence of the above is, that the animal being the realization of the abstract idea of unity, in which all parts must relate to a common centre, the sphere must be of necessity the original type of animal organization, the globe being that which tends to its centre with perfectly equal relations (radii). But, so far as the animal is not merely the upper part of the plant become detached, but likewise contains in itself the organs of the root, the globe must be hollow, and contain within itself the intestines, which can be most clearly pointed out in the lower animals. Because, without taking into account that the Infusoria appear merely as so many living hollow globules or cells (see the history of the green matter of Priestley, page 232, &c.), this kind of structure is evident in the bladder-worm (Cysticercus), in which (see fig. C.) the absorbing proboscis *b* (therefore called the head) is in reality curved inwards into the cavity of the body *a*, exactly in the manner described in the hypothetical metamorphosis of the plant into an animal. Similar forms are also exhibited by the Echinus tribe, to which we must add, that microscopical observations show most clearly that the whole of the organic mass of higher animals is composed of minute globuli.

The fourth consequence is, that as unity is the characteristic peculiarity of an organism, there must exist, because of the greater multiplicity of its form, a bond which, while it again unites that multitude, exhibits that relationship to a common centre which accords with its organization. However, since the animal presents two sides, a higher one, turned toward the outer world, and peculiarly animal, and a lower one, turned into itself, destined for reproduction, and so far purely vegetative, the above bond must likewise of necessity be twofold, and bear a particular relation to each side.

Fifth consequence.—No reciprocal action can take place between two bodies, except in two directions, (just as the organism itself appears essentially as body and life under two forms only), namely, in its tendency to produce a change and combination of particles, or to a reciprocal transmission of power. Inasmuch as in the animal the change

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* Carus, *Lehrbuch der Zootomie*, p. 482.

of particles belongs properly to its internal vegetative side, its external side (for the very reason that it is turned immediately to the outer world) will appear as a perceptive and reciprocating activity*. But since we may constantly recognise in every mutual relation of body as well as of action a threefold *momentum*, viz. action, reaction, and connexion or the indifference of both, these three elements must therefore be found also in each of the two sides of the animal organism. In fact we find them in the external (animal) side, as perception, motion, and the connexion of both through the nerves; in the internal vegetative side, as assimilation, secretion, and the connexion of both by circulation. But whatever is true of the bond which in these two spheres gives every form its centricity, the same must naturally be found in the so-called connecting nervous and vascular systems. Yet these two systems must not perhaps be considered as occasioning a real *dualism* in the organization, but as equally subordinate and reduced to a *unity*, namely the nervous system, as being the higher, because it belongs to the animal which includes the vegetative sphere.

Sixth consequence. As the plant is not merely occupied with its own change of particles and continual transformation, but when it has attained its perfect development produces the seed, as the representative of all its properties, the true reproduction of the species, we find in the animal likewise a similar reproduction of the species, in so far as even the animal is but a more perfect vegetable nature. The system thus established in respect to the animal, viz. the sexual system, possesses in its nature a polarity similar to that of the plant; for we find in the more perfectly organized animals, a female reproductive and a male impregnating organ. But, on the contrary, we find the lower animals, like the inferior plants (see p. 239), endowed with female organs only. The activity of this system, manifesting itself especially in assimilation, secretion, and (as the basis of these two *momenta*) in vascular action, belongs to the sphere of vegetation; and there is nothing to be compared with it in the animal sphere, except that activity by virtue of which, in the most perfect animal organism, that is the human, the idea of nature is reproduced by means of spiritual power, and truly developed through science and art.

We have now further to consider the composition and internal formation of the animal body, as well as the nature and direction of its active faculties of life. In the first point of view, we observe that it contains, like the organism of the earth and of the plant, a combination of solids, fluids, vapours, and gases, among which the fluids are again the sources of the rest. Its ultimate elements are principally hydrogen, oxygen, and

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* It is only in this way that the origin of the sensible side is capable of a scientific construction.

carbonic acid gas, with the additional one of nitrogen, which peculiarly belongs to this kingdom, and the volatile nature of which perfectly agrees with the rapid merging of animal bodies in the universal life of nature, as soon as their individual life is extinct; to which passage, however, the earthy parts, such as bones and shells, offer somewhat longer resistance. In regard to the internal formation of the solid parts of the animal body, it has been already remarked (see p. 244) that the spherical type, in so far as it is peculiar to animal forms in general, is also visible in the basis of all animal matter, so that the molecular substance is the basis of the collective animal body. If we now reflect likewise how in the Infusoria and Priestley's matter, the rudiments of the animal kingdom appear as so many animated globuli, we shall thence perceive that the largest animal bodies themselves must be viewed as an innumerable aggregate of Infusoria, but at the same time united into a living whole. It is moreover worthy of remark, that very essential differences present themselves in respect to the primary formation of the animal body in its different systems. It is also a remarkable fact, that in the organs exclusively proper to the animal,—for instance, in those of sense, motion, and the nerves,—this molecular mass is clearly discernible, partly as the marrow of the nerves (a peculiar grey substance), partly as developed nervous and muscular fibres; while in the organs which are more immediately borrowed from the plant (the vascular system, the skin, and the intestines), we remark again a very decided tendency to cellular formation.

We have now before us two modes of perceiving in its true nature the further formation of the primary animal mass into the single organs of the animal body; either that of attentively watching the development of one complete animal organism in its different stages, or that of observing the succession of the species according to the order of animals in the development of their animalism. Of these two we shall give only the principal outlines of the first series of formation*, in which we shall find a great analogy to the development of the plant, but more especially a manifold confirmation of that which we have advanced in regard to the metamorphosis, or rather elevation, of vegetable into animal life. But in tracing the development of the individual animal body through its several stages, we shall take as the main object of our observations the human organization as the most perfect; and thus we shall have occasion to recur to that of other animals in those cases only in which the observations made on the human being itself are deficient in regard to its first rudiments.

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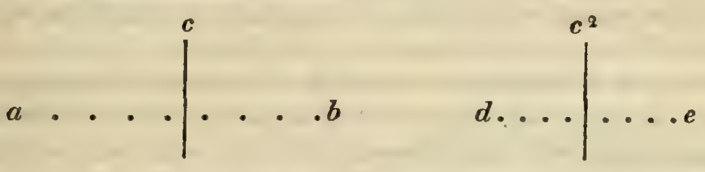
* The observation of the development of organization in the series of animals, is the idea upon which Carus's Manual of Zootomy is founded, to which he here refers his readers.

One thing, however, we know with certainty, namely, that the human body, like that of other animals, has its origin in the egg. But the egg itself must be considered as the original animal, as the infusorial creature, appearing in the globular or primary form of animal life, and, like the seed of the plant, containing *ideally* within itself the whole animal, which, under given external circumstances, it develops *really* out of itself. Recent observations have shown that the first rudiment of the embryo is formed on the inner surface of the hollow globe of the egg, by folding or turning inwardly a part of the integuments of the egg; which reminds us distinctly of the purely vegetative bladder-worm (Cysticercus, see p. 244), in which the so-called head, or the absorbing orifice, is turned inwards. It is plain therefore that we must consider the egg in this first period of development also as a plant with a root turned inwards (see p. 243); and we find this moreover confirmed by the functionary attributes of its parts, since the first introversion of the integument forms the cavity of the stomach and the intestines, as the first rudiment of the embryo. On the other hand, the external covering of the egg being somewhat similar to the green (breathing) surface of the plant, performs the breathing function of the embryo, and contains also (as the latest observations of Pander have shown), in the external envelop of the cuticle, the origin of the organs of sensation. In a further stage of development we see the activity of formation concentrating itself more in the point turned inwards, and thus producing new opposites. The rudiment of the embryo repeats the above-described form, and the higher animal organs, which commence with the spinal marrow and the vertebral column, originate above the germ of the intestines of the cavity of the stomach. It is now in particular plainly seen how the animal is as it were transformed from a plant into an animal in the manner above described (p. 234). For at the very first the intestines are only attached to the fœtus, that is, the gut lies as yet in the umbilical cord, just as at first the root is *attached to* and not *in* the plant; but the intestines are soon afterwards drawn more and more inwards, and by degrees are completely enveloped by the animal organs, which grow simultaneously forwards on both sides out of the vertebral column; and here for the first time the fœtus presents the appearance of an independent animal organism. The vascular system, however, is much more slow in connecting itself perfectly with the fœtus; since it is rather a general connecting medium in the vegetative sphere, and therefore performs the function of a bond of union between the integument of the egg and the fœtus so long as it remains inclosed in the egg. Even at this period of development the relation of the parts of the egg reminds us of the plant; as the fœtus, by means of the umbilical cord, is united to the placenta and integuments of the egg, held in organic connexion with the maternal organism, just as the flower (which it resembles in respect to the development of the organs of generation,) is united by means of the stem to the root, which is held in connexion with the organism of the earth. It is also remarkable, that in the fœtus the direction of the development from the insertion of the umbilical cord is upwards, as we see in the plant that the flower is directed upwards from the insertion of the stalk. For instance, the anthers are never turned back toward the stem and the fruit-germ, or directed downwards, but on the contrary are invariably and wholly directed upwards, unless the flower-stalk and fruit-germ are turned downwards, in which case they also are directed downwards with them. This explains the development of the vertebral column into the head, in which the flower of the collected animal organization appears as completely as the flower of the egg does in the entire fœtus. This fact is still more clearly observable in the central structure of the vertebral column, viz. the spinal marrow, the fibres of which we see more perfectly developing themselves as they ascend upwards, till they terminate in the perfect and noble formation of the brain.

This direction of development in the fœtus, the truth of which is most clearly established by many physiological as well as pathological observations, is also indicated by the position of the entire fœtus, in which we find the head usually turned downwards, but the lower extremity turned toward the insertion of the umbilical cord; in the same manner as the flower of the plant, and the head of the more perfect animal, rise upwards from the ground. Moreover, that the greater weight of the head which occasions it to sink downwards in the uterus is not the only use of that position of the fœtus, is evident from the parturition of the quadruped mammalia, since although their standing on four feet must prevent the operation of such a cause, they nevertheless bring forth their young with the head (nay even with the face) forwards.

We can here give but general outlines of the further development of the different systems and organs formed in the fœtus; and with respect to the systems belonging to the animal side of the animal body, and which corresponds to the light side of the plant, we observe that from their constituting the parts originally turned toward the outer world, they are endowed with a tendency to develop their structure in a direction radiating outwards from an internal centre; for which reason we see the nervous system form itself as the radii of a central mass (the brain and the spinal marrow), which develops itself with a perfection continually increasing in proportion as the radiation outwards increases; we see also the ends of the nerves forming themselves either into the organs of sensation, or, as being destined to re-act upon external objects, inserting themselves into the molecular mass of the animal; so that these molecules, disposed into muscular fibres, are drawn, sometimes more and sometimes less strongly, towards that insertion of the nerves, according as the action of the nerves is increased or diminished by its centricity; and it is thus that they determine the muscular contraction, and all animal motion. The following figure will explain the contraction and relaxation of the muscle: *a b* represents the relaxed muscle, and *c* the nerve in its usual state; *d e* the contracted muscle, and c^2 the nerve in increased action. In the second figure, the contraction of the muscle by means of a diminution of volume is explained by the closer approximation of the points to the centre.



But besides this twofold termination, the nervous system finally branches out also into the vegetative sphere of the body, thus partly determining both sensation and motion, and partly constituting the bond of unity between the different organs of the vegetative system. In the latter point of view the very form of the nervous fibres is remarkable, since they all have an evident tendency to inclose the intestines and vessels in a kind of network. In the same point of view it becomes clear also that, as the inclosing of the lower system in the higher one is characteristic of animal organization, and the rudiments of the nerves always show this peripherous type, the higher animals, and man in particular,—in whom the spinal nerves encompass the common cavity of the trunk, somewhat similarly to the bending of the ribs,—possess a system of nerves appropriated to the vegetative structure, and perfectly analogous to the nervous system of the inferior animals, namely, the ganglionic system, or that of the great sympathetic nerve.

The osseous system is developed uniformly with that of the nerves, and issues out of the vertebral column, as the nervous system does out of the spinal marrow; whilst the vertebral column forms at first but an isolating sheath around the latter, as an earthly substance most powerfully attracted by the nervous system, acting like the sun, and for the most part antagonistically upon the other parts of the body. In a somewhat similar manner, among falling bodies of different specific gravities, the heaviest will always lie undermost and nearest to the attracting centre of the earth. Where the osseous system has acquired its most perfect structure (in the skull) it presents also the original type, the spherical form; on the contrary, like the radii of the nervous system, it branches out more and more in the extremities, a fact which is clearly seen in the increasing number of the bones in each member, from their root in the trunk to the ramification in the fingers and toes.

In examining the origin of the individual members of the vegetative sphere, of which we have already observed that the formation from without toward the internal parts, namely from the umbilical cord into the cavity of the stomach, presents the organs of digestion (as the internal nourishing root of the animal) and the bowels,—we find that in the progress of organization new organs are formed for the functions of respiration and secretion, to correspond to those of nutrition and absorption. The function of respiration indicates the connexion of the individual organism with the atmosphere, as on the other hand the absorption of grosser matter indicates its connexion with the earth by the root.

As long as the fœtus is inclosed in the placenta it can have no immediate connexion with the terrestrial organism, but maintains rather a reciprocal action with the maternal organism, as is seen in its manner of breathing, which is originally performed through the integuments of the egg (see p. 247). But since it is necessary that an independent organ of respiration be prepared for the time when the fœtus leaves the maternal body and the integuments die away, we find the external surface of the fœtus itself (the skin) developed with its continuation (the internal organs of respiration). The vascular system (hitherto the connecting medium between the fœtus and the integuments of the egg, see p. 247), then becomes the connecting medium between the organs of digestion, respiration, and secretion. The organs of secretion, however, may in a certain respect be considered as a repetition of the organs of respiration, since the evaporation and secretion of gases form a prominent part in the process of respiration. The plant develops, besides the organs of general assimilation and secretion, those of generation: a similar development takes place in the animal also, and precisely at the point where the germ of the fruit is developed in the plant, at a point which is therefore analogous to the insertion of the stamen in the flower; in other words, at the insertion of the umbilical cord into the abdominal cavity, which in the first stage of formation, where we observe the embryo with its pointed lower extremity attached to the inner surface of the egg, is the basin that incloses the genitalia as the calyx does the fruit-germ.

After having considered the transition of the form of the plant into that of the animal, it remains for us now to examine the peculiarity of the active living principle of the animal, in order to ascertain how far this is derived from the active living principle of the plant. But we must first carefully observe, that if we were right in considering (page 239) the chief end of the active living principle of the plant to be its formation, the first active living principle in the animal likewise must be a tendency to acquire individual existence. The whole animal body, as far as regards nutrition, growth, secretion, and its being engaged in a continual change of matter in the parts already formed (the vegetative as well as the higher and properly animal parts), lives absolutely the life of the plant. We likewise observe, that here also the fluid parts are the sources from which the several solid parts are formed by different attractive affinities, for this reason, that the external nourishing substances first enter into the fluids, but at the same time the secretions which are given off from the fluid parts of the body are carried away in the form of liquids, vapours, or gases. However, to perform this attraction and repulsion, one condition is requisite which is equally necessary in the life of plants, and that is the circulation of the fluids. This circulation we have seen performed in the plant through the polarity between the root and the flower, and therefore in a linear direction from one end to the other. But since the animal is a plant the root of which is turned inwards, a similar motion of the fluids, from the absorbing to the exhaling pole, instead of being directed linearly from one extremity of the body to the other, must take a centrifugal and centripetal direction, inasmuch as it is but an action and reaction between external and internal organs. We have observed that the circulation of the fluids in the embryo is between the embryo as a centre and the integuments of the egg as the periphery; and that the circulation is not confined to the embryo itself until it has attained its full maturity*, at which period it is observed alternating between the heart (as the centre of vegetation) and the periphery; and in the higher animals and man, partly toward the whole bodily structure and its common integument the epidermis, partly toward the epidermis turned inwards in the organ of respiration, and *vice versâ*. Hence it follows that this movement of the sap or blood is not properly a circular motion, and has therefore no true resemblance to the rotatory motion of the heavenly bodies; because these have an intermediate movement which is the result of attraction and repulsion; whereas the movement of the blood appears as an *alternation* of attraction and repulsion, which is most analogous to the ascending and descending movement of the sap in the plant. This also leads to the conclusion that both this movement and its direction are originally the mere effect of the polar attraction and repulsion of the fluid itself. Wherefore this circulation may take place without needing any other mechanical aid, as for instance the pressure of the vascular surfaces, of which aid many animals are for the most part destitute. Indeed, the *original* unimportance of such aids is rendered fully evident by the fact that the vessels, like all other solid parts, are formed only by the circulating fluids,—consequently, that

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* The essential difference between the earliest and the later form of the circulation in the egg during the process of incubation has been recently demonstrated in the beautiful experiments of Pander.

the fluids exist before the vessels, and that the direction of the vessels is the necessary *consequence of the direction of the fluids.*

Whilst through these general considerations we see how the movements of the original fluids maintain reproduction in all parts of the body, a due attention to the different polar directions of vascular activity, according to the different natures of the several parts of the body, will enable us also to perceive their differences of assimilation as well as secretion, and the laws to which they are subject; but these inquiries we cannot here pursue further. We now turn our attention to animal *sensation*, in order to observe the difference between it and the receptivity of the plant. The animal, containing within itself the organism of the plant, its organic functions must, like those of the plant, be liable to be modified by external influences. This property in the plant we have termed *irritability*, because irritation *immediately* causes and calls forth re-action. In the animal, however, the nervous life (as the expression and type of unity, which embraces all the parts of the organism and forms them into a whole,) stands between irritation and re-action; and as each local irritation is communicated through the nervous life to the whole organic unity, to the consciousness of the animal, the perception of the irritation rises to a *sensation*, and thus it depends more on the free will of the animal whether or not irritation be followed by re-action. The more intense this consciousness is, (it being the unity out of which the multiplicity of organic phænomena is developed,) the purer and more varied must the sensation be, (because a more marked individuality necessarily causes a more varied relationship with external objects,) and the more free the action toward the external world; i. e. the less it will depend on external influence, and the more it will be determined from within. We find therefore that the lower animals, and indeed even the parts of the human body which are less closely connected with the system of nerves proceeding from the brain and spine, exhibit in a greater degree the irritability of the plant; while, on the contrary, the higher nervous life of the human organism presents the flower (or perfection) of individual activity in psychical life, in self-consciousness, the manifestation of which has often been separated from the other branches of organic activity, no less erroneously than its essence (according to the notion of the materialists) has been considered as the sum or result of a certain corporeal mechanism*.

We have already observed in our introductory remarks on the idea of life, that the inward unity, or the highest idea of an individual organism, is by no means the effect or the result, but the ground and cause, of external multiplicity, and this is also the case with the psy-

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* [Rather,—as the sum or result of the powers and properties, physical, vital and sentient, with which the Creator has endowed the human frame.—Edit.]

chical life*—(wherefore the doctrine of Stahl, that the soul forms its own body, *if properly understood*, is very *admissible*,—and we hope soon to have the opportunity more fully to develop these views, for which we have not space here, and to confirm the above propositions by more convincing proofs. For the present we have only to observe in general, having already spoken of the manner in which animal re-action depends on muscular motion (see p. 237), how far the more precise independence and the more certain self-consciousness of the animal give rise to the individual forms of sensation.

In the plant, in which irritation causes re-action at the point where it acts, and the single parts of which are independent, but not the whole, irritability (belonging to all the parts) must be general; and this general irritability (raised into a sensation only through the relation of each irritating action to the whole,) is possessed by the animal in common with the plant, and it is therefore included in the comprehensive term *feeling*. But since in the animal the sensation of each individual part is related to the whole, this sensation can be concentrated and particularly developed, on *certain individual points*, without injury, or rather with advantage to the whole; wherefore we see that the different sides of perception turned toward the outer world, correspond in number with the different organic systems turned toward the outer world, and with the qualitative influences of various kinds acting upon the organism; so that if mere Feeling gives us only a knowledge of the state of our own organism, the *individual* senses of hearing, seeing, smelling, tasting, touching, &c. afford us a clearer consciousness of the external world, through a local alteration of our own condition.

If in closing these observations, intended to show the progressive development of animal life out of the life of the lower kingdoms of nature, we look to the changes which animal life operates upon them, facts present themselves worthy of the most serious consideration. We have seen how the vegetative life is nourished by inorganic life, and how vegetation in its turn operates changes in many ways upon the surface of the earth, and even on the atmosphere. So again we find that the animal kingdom maintains the most active relation with the vegetable life and with the elements of the earth and of the air. We see coral rocks and islands raised from the bottom of the sea by animated beings apparently insignificant, which, existing before the creation of Adam, now elevate their lofty tops as mountains of the continent; we see the animal kingdom penetrating into parts of the earth seemingly impenetrable to all living creatures†; moreover, we observe that here also, where, according to the eternal laws of nature, the highest is

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* [*psychischen Leben*, Germ.]

† See on this subject the observations which G. R. Treviranus (*Biologie*, vol. ii. p. 7) has collected from the instructive reports of other naturalists.

connected with the lowest, and the human organization itself falls at last into inorganic dust, the form and culture of the land, the course of the rivers, vegetation, and population, along with different animal species, are in various ways changed by the activity of man. If therefore we compare the condition of countries which have once flourished and exhibited the activity of human industry, with the desert state which they now present, when, after the fall of these nations, they are deprived of the care and culture of man, we shall be convinced that, as a modern writer* expresses himself on this subject, "Not only does man need the *earth* in order to live and be active, but the *earth* also stands in need of *man*."

We may hope now to have attained the object of the present essay, if, by throwing some new light upon certain aspects of infinite nature which have hitherto remained less observed, we have awakened a new attention to the indissoluble union as well as the beauty and regularity of the phænomena surrounding man and existing within him; and as the contemplation of these must necessarily stimulate us, not only to penetrate more deeply into the mysteries of science, but also to conform our own inward life to that harmony and purity which are presented by universal nature; for what would be the value of all scientific knowledge, did it not manifest itself in ennobling and elevating the human mind?

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* I. F. Koreff, *De Regionibus Italiæ aere pernicioso contaminatis Observationes.* Berol. 1817. 4.

NOTE.

[In some of his reasonings the Author will, perhaps, be thought to deal with abstract terms as if they were real essences, or to employ them in a sense somewhat peculiar. Whatever difference of opinion may, however, exist with regard to the speculative parts of this memoir, it will, it is presumed, be acceptable and interesting to many readers, as showing the manner in which physiological subjects are viewed by some distinguished writers on the Continent.—EDIT.]

Article XI.

Researches on the Elasticity of Bodies which Crystallize regularly; by Felix Savart.

(Read to the Academy of Sciences of Paris, January 26th, 1829.)

From the *Annales de Chimie et de Physique*, vol. xl. p. 5, *et seq.*

[Continued from p. 152.]

§ III. *Analysis of Rock Crystal by means of Sonorous Vibrations,*

Rock Crystal most ordinarily occurs under the form of a hexahedral prism, terminated by pyramids with six faces (fig. 1. pl. IV.). Although this substance does not admit of cleavage by the ordinary means, it is assumed, from analogy, that its primitive form is a rhombohedron, like that which would be obtained if the crystal were susceptible of cleavage parallel to the three non-adjacent faces of the pyramid, such, for example, as $a\mathrm{X}b$, $e\mathrm{X}f$, $c\mathrm{X}d$, and their parallels $a'\mathrm{Y}b'$, $e'\mathrm{Y}f'$, $c'\mathrm{Y}d'$. The accuracy of this induction is besides confirmed by a very simple experiment, which consists in making a prism of rock crystal red hot, and suddenly cooling it; an operation which determines its fracture, and which most frequently, gives as the result pieces of crystal which have the form of rhombohedrons.

Setting out with these notions with which mineralogy furnishes us, it is obvious that circular plates taken parallel or perpendicular to the axis, parallel to a face of cleavage or of non-cleavage of the pyramid, &c. ought to present different phænomena with respect to sonorous vibrations, since the cohesion and elasticity are not the same in these different directions. Consequently, to simplify as much as possible the examination of these phænomena, we have had cut, from different pieces of rock crystal, a considerable number of circular plates, at first taken in different azimuths of a plane perpendicular to the axis, fig. 2. and fig. 2, *bis*; then, according to the azimuths of a plane perpendicular to two parallel faces of the hexahedron, and passing through its axis, fig. 3. and fig. 3, *bis*; lastly, according to the different azimuths of a plane passing through the axis and two opposite edges of the crystal fig. 4. and 4 *bis*.

As it was necessary to support this general disposition of the experiments by facts, it was indispensable to ascertain first, that the elastic state of the crystal is the same for all the planes parallel to the natural faces of the hexahedron, and next, that it is also the same for all the planes perpendicular to the preceding and passing through the axis, although it be different in the latter from what it is in the former; lastly, it was necessary to verify whether the plates cut parallel to the faces *a*X*b*, *e*X*f*, *c*X*d* of the pyramid were really susceptible of assuming the same modes of division, and whether these modes were different from those of the three plates cut parallel to the faces *b*X*c*, *d*X*e*, *a*X*f*, these latter being besides similar to each other. Experiment having shown the affirmative of these positions, it is evident that all the series of plates perpendicular to a plane normal to any two parallel faces of the prism, and passing through its axis, ought to present identical phænomena for the same degrees of inclination, and that the same ought to be the case for the series of plates perpendicular to any plane passing through two opposite edges of the hexahedron. All the plates we have employed are 23 or 27 lines in diameter and 1 line in thickness; they have been cut with great care and are polished, in order that the phænomena they exhibit with respect to light might be compared with those they present relative to sonorous vibrations. Lastly, although they have been taken from five or six different crystals and from different countries, it may be supposed that they belong to the same piece of quartz, because, whenever it was necessary to pass from one crystal to another, the precaution was taken of causing to be cut in the new specimen a certain number of plates, for the sole purpose of repeating the experiments already made; and by this process we may assure ourselves that crystals of very different appearance, such as those of Madagascar and of Dauphiny, do not however present remarkable differences in their structure.

Before proceeding to the description of the phænomena which are related to each series of plates, we shall observe, that in all the figures the line *x y* represents the axis itself of the crystal when it is contained in the plane of the plate, or its projection in the contrary case, and that the position of this axis has been determined with great care, for each plate individually, by means of polarized light; so that with this datum and the details into which we shall enter, the position occupied by any plate within the mass of the crystal may easily be represented to the mind.

First Series. *Plates parallel to the Axis of the Hexahedron.*

If we consider first the plates i., v., ix., fig. 2. and 2, *bis*, which are parallel to the faces of the hexahedron, we see that they assume exactly the same modes of division: one of these modes, that which is represented by dotted lines, consists of two nodal lines, which cross each other rectangularly, whilst the other resembles the two branches of a hyperbola, to which the two preceding lines serve as axes. The sound of the first system being F, that of the second is the D♯ of the same octave. Thus, in any plate taken parallel to the faces of the hexahedron, one of the nodal lines of the rectangular system always corresponds with the axis of the crystal. In this case everything occurs the same as in plates composed of parallel fibres and which contain in their plane at least one of the axes of elasticity; but this is no longer the case for the plates III., VII., XI., perpendicular to two parallel faces of the hexahedron, although they are also parallel to the axis like the preceding: instead of a system of lines crossed rectangularly and a hyperbolic system, they exhibit only two hyperbolic systems, which appear exactly similar, and which however are accompanied by very different sounds, since one of them gives D and the other F♯ of the same octave. The principal axes *l m*, *l′ m′* of each of the two hyperbolic curves appear to intersect each other at the centre of the plate; their mutual inclination is from 51° to 52°, so that the branches of these curves intersect each other; and if a line *o p* be drawn through the centre of the plate equally inclined to each of the axes *l m*, *l′ m′*, and this line be supposed to be the section of a plane perpendicular to the plate, this plane will, for the plate III., be parallel to the face eXf of the pyramid fig. 1.; for the plate VII., to the face aXb; and lastly, for the plate XI., to the face cXd; so that it must hence be concluded that the six faces of the pyramid do not possess the same properties, and that the three we have just indicated perform an important part in the phænomena in question. It must be remarked that the modes of division of these plates are exactly the same as those of the plate No. 3 of fig. 14, Pl. III.*, which contains neither of the axes of elasticity in its plane. Now, if we consider the plates II., IV., VI., VIII., X., XII. intermediate to the preceding and to those which are parallel to the faces of the hexahedron, we find also in them properties which seem to depend on both jointly, as well with respect to the nodal lines of the two systems as to the sounds they produce. Thus with reference to the process of investigation which we have employed, all the plates parallel to the axis do not possess the same properties, whilst with regard to light it is well known that they exhibit exactly the same appearances.

Although this result has been verified many times, it was still important to verify it again, which I did in the following manner: I took, first, two plates like Nos. I. and V., and then two plates like III. and VII., and after having crossed their optic axes, I placed successively each of these pairs in the path of a large pencil of light polarized by a black glass, the plane of the plates being placed perpendicularly to the luminous rays, and their axes making an angle of 45° with the plane of polarization. It is known that if we look through a similar pair by means of a tourmaline, the axis of which is in the plane of polarization, we perceive two systems of coloured hyperbolas, the tints of which

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* For Pl. III. see Scientific Memoirs, Part I.

appear to follow sensibly in their succession the order of those of Newton's rings: it was required therefore only to compare the phænomena observed in the two cases, and to see whether they presented any hitherto unobserved differences; but it was impossible to recognise any. Thinking that perhaps a considerable augmentation of thickness in the plates might bring to view some appreciable differences, I repeated the experiment with pieces of rock crystal which were eight centimetres (3·149 inches) in thickness, and I saw nothing that could indicate that all the plates parallel to the axis did not comport themselves in the same manner with regard to light: whence it must be concluded that what we can learn respecting the structure of crystals by means of light, is not of the same order as that which sonorous vibrations may enable us to discover. It would appear from what precedes, that this latter process indicates more specially the elastic state and the force of cohesion of the integrant particles in the different directions of every plane, whilst the phænomena of light, depending more specially on the form of the particles and on the position they assume round their centre of gravity, are, to a certain point, independent of the mode of junction of the different plates of which the crystal is formed.

SECOND SERIES. *Plates cut round the Edge a b, fig.* 1, *and according to the different Azimuths of the Plane m n X o p Y, fig.* 3, *normal to the Faces No.* 1. *and No.* 4. *of the Hexahedron and passing through its axis.*

One of the modes of division of all the plates of this series remains constantly the same, fig. 3, *bis*; it is formed of two straight lines crossing each other rectangularly, and xy, one of these lines, is always the projection of the axis of the crystal on the plane of the plate. The other mode of division consists of two hyperbolic curves, which undergo various modifications depending on the inclination of the plates to the axis of the hexahedron, and which are in general analogous to those we have observed in the two first series of plates belonging to bodies possessing three rectangular axes of elasticity.

No. 1. represents the two modes of division of the plate perpendicular to the axis XY; they are both composed of straight lines; or, if either is formed of two curves, their summits are so near each other that they appear to coalesce. Rock crystal being a crystal with one axis, in respect to light, it was natural to presume that the elasticity would be equal in every direction of the plane of the plate in question, and that, in consequence, this plate might assume only a single mode of division, having the property of placing itself in any direction; but this is not the case, even in plates cut with extreme care, and which by their optical properties appear sensibly perpendicular to the axis. Nevertheless, the interval which is observed between the sounds of the two systems being always very small, and not being constant in different crystals, it appears more natural to attribute this difference of elasticity to an irregularity of structure than to suppose that it depends on a determinate and regular arrangement, the more so as in very large crystals, like those I have employed, it is very rare not to meet with irregularities of structure sufficiently obvious even to be recognised by the naked eye.

The plate No. 2, inclined 78° to the axis, begins to present a difference in the disposition of these two systems of nodal lines; one of the two transforms itself into two hyperbolic branches, which become more straightened in the plate No. 3, inclined 75° to the axis, and which afterwards approach each other again, and become two straight lines, which intersect each other at right angles in the plate No. 4, inclined about 51° to the axis, and which consequently is nearly perpendicular to the face *a* X *b* of the pyramid fig. 1; the inclination of the faces of the pyramid to those of the hexahedron being 140° 40′.

The numbers of vibrations which were nearly the same for No. 1, from which only the sounds D and D + were obtained, differ more as the plate approaches No. 4, when the gravest sound being C, the second is the G of the same octave, although the two modes of division are the same as those of No. 1. It is this sound C, given by one of the modes of division of the plate perpendicular to the face of the pyramid, which I have taken as the term of comparison, and to which the sounds of all the other plates are referred. Recommencing with the plate No. 4, the variable system separates once more, but in the contrary way; the curves which form it continue to straighten, whilst their summits recede from each other, and at the same time the two sounds approximate until they are sensibly the same in No. 8, inclined about 12° to the axis. The hyperbolic system ceases to assume a determined position, and it can, without the sound undergoing any change, transform itself gradually into the rectangular system which form the axes, so that this plate appears to be exactly in the same conditions as No. 5 of fig. 8, Pl. III. In a crystal of quartz there are three planes analogous to the preceding, since the phænomena which are presented by the plates cut round the edge *a b* of the base of the prism, are, as I have satisfied myself, precisely the same as those which are presented, for the same degrees of inclination, by plates cut round the two other edges *c d*, *e f*.

Beyond No. 8. the sounds begin to differ from each other, and the branches of the hyperbola continue to straighten until No. 11, parallel to the second face of the pyramid. There the distance between their summits is greater than for any other degree of inclination of the plates, and the sound of the rectangular system is the same as that of the same mode of division in No. 4, perpendicular to the face *a* X *b* of the pyramid. Lastly, from No. 11 until the plate perpendicular to the axis, the sounds approximate again, as well as the summits of the hyperbolic curves, and at the same time the two systems of nodal lines again become rectangular; the sounds thus become almost the same.

Among the plates which we have just examined there are two which merit particular attention; these are Nos. 5 and 11, parallel to the the faces *e*X*d* and *a*X*b* of the pyramid, and the elastic state of which undoubtedly differs very much, since in one it is the hyperbolic system which gives the gravest sound, whilst in the other it is the rectangular system, and that, besides, there is a great difference between the sounds which correspond to each of their nodal systems. The faces *a*X*b* and and *e*X*d* of the pyramid being opposite, one of the two ought to be susceptible of cleavage, whilst the other ought not to be capable of this mechanical division; consequently if we knew which of the two plates Nos. 5 and 11 possesses this property, we might, by examining its acoustic figures, determine which are the faces of the pyramid parallel to the faces of the primitive rhombohedron. Rock crystal not yielding in the least to any attempt at dividing it into regular layers in any direction, it was impossible for me to ascertain directly which of the two faces *a*X*b* or *e*X*d* were those of cleavage; but this question can be resolved with ferriferous carbonate of lime, a substance which is cleaved with almost the same facility as pure carbonate of lime, and which appears to possess, in reference to sonorous vibrations, properties in general analogous to those of rock crystal. Now, if we cut in such a crystal two plates,—one taken parallel to a natural face of the rhombohedron, the other corresponding with a plane inclined to the axis by the same number of degrees as these faces, and which are besides equally inclined to the two faces which form one of the obtuse solid angles,—we find that the first possesses the same properties as No. 11, whilst the second has a structure analogous to that of No. 5; whence it ought to be concluded, from analogy, that the face *a* X *b* of the pyramid fig. 1. is that which is susceptible of cleavage. This once established, it is not even requisite, in order to ascertain which of the faces is susceptible of cleavage, to cut a plate parallel to one of these faces; it is obvious that a plate parallel to the axis and normal to two parallel faces of the hexahedron should be sufficient to attain this end. Thus, let fig. 5, *a b c d e f*, be the horizontal projection of the prism represented fig. 1; according to what has been said, *r s t v* will be the projection of the primitive rhombohedron; again, let *l l'* be the projection of a plate parallel to the axis and equally inclined to the two faces of *a* and *f* of the hexahedron; according to what we have above said, this plate will assume the mode of division of No. 3, fig. 2, *bis*, and the line *op* will be parallel to the plane *r s t u* normal to the plate, that is to say, to one of the cleavage planes; thus the direction of this line, in a plate parallel to the axis and normal to two faces of the hexahedron, is sufficient to enable us to ascertain which of the faces of the pyramid are susceptible of cleavage.

In order to complete all that relates to the transformations of the nodal lines of this series of plates, it would have been important to determine with accuracy the degree of inclination to the axis, of the plane situated between No. 3 and No. 4, for which the summits of the nodal hyperbola are at the greatest distance from each other: but, having been stopped in these investigations by the difficulty of procuring a sufficient quantity of rock crystal very pure and regularly crystallized, I have been reduced to determine this *maximum* of recession on another substance, and I have chosen for this purpose the ferriferous carbonate of lime, a substance whose primitive form is a rhombohedron, which differs from that of rock crystal only in the angles formed by its terminating planes. As we have already observed, there is a sufficiently great analogy between the phænomena presented by these two substances, with respect to sonorous vibrations, to enable us to admit that what occurs in one occurs also in the other: thus, let A E, fig. 6, be a rhombohedron of carbonate of lime, of which A is one of the obtuse solid angles; A B C D corresponding to the face of cleavage of the pyramid of rock crystal, the diagonal B D will be the line round which all the plates must be supposed to be cut; and they are consequently normal to A C E G, represented separately in fig. 7, in which the lines 1, 2, 3, &c., are their projections, and indicate at the same time the angles which they make with the axis A E. We will first remark that the modes of division of the plate No. 1, fig. 7, *bis*, perpendicular to the axis, are the same as those of the corresponding plate of rock crystal, and that the plate No. 5, perpendicular to A C, assumes also the same modes of division as the plate perpendicular to the cleavable face of the pyramid of rock crystal, which establishes a sufficient analogy between the two orders of phænomena. The inspection of fig. 7, *bis*, shows then that the branches of the nodal hyperbola of No. 3, parallel to A G, consequently to the plane B D F H, are straighter than those of the plates which precede or follow it; and admitting that this maximum of recession occurs equally in quartz for the corresponding diagonal plane of its rhombohedron, as this plane forms with the cleavable face of the pyramid an angle of 96° 0′ 13″, the plate in question will be inclined 57° 40′ 13″ to the axis of the crystal, the face of the pyramid forming with this axis an angle of 38° 20′; thus the projection of this plate on the plane *m n* X *o p* Y of fig. 3. will be the line A B.

Now since the *maximum* of recession of the summits of the nodal hyperbola is in this manner determined, it is easy to recognise a great analogy between the phænomena of fig. 8, Pl. III., and those of fig. 3, *bis*, Pl. IV.; for, supposing several intermediate plates between Nos. 3 and 4, that which would be inclined 57° to the axis would correspond to No. 1 of fig. 8, Pl. III.; No. 4 in the crystal would correspond to No. 3 in the wood; and lastly, No. 11 of the crystal plates, in which a second *maximum* of recession of the summits of the hyperbola occurs, would correspond to No. 6 of the plates of wood; so that the same phænomena, which are included, in a body having three rectangular axes of elasticity, in an arc only of 90°, to be afterwards reproduced in a contrary direction in the following quadrant, are included in rock crystal in an arc of 96° 0′ 13″, and cannot be entirely reproduced, because similar phænomena to those we have just observed for a series of plates cut round *a b*, fig. 1, Pl. IV., occurring, for the same degrees of inclination, in the two series of plates which might be cut round *c d* and *e f*, both are confounded together in the vicinity of the plate perpendicular to X Y.

THIRD SERIES. *Plates cut round the diagonal a c, fig.* 1, *and according to the different Azimuths of the Plane b e′ Y b′ e X, fig.* 4.

These plates present phænomena much more complicated than those of the two preceding series. It may be easily conceived that this ought to be the case, since the plates parallel to the two adjacent faces of the pyramid assume very different modes of division, which supposes that their elastic state also greatly differs: consequently the plates perpendicular to the plane which passes through the two opposite edges of the hexahedron ought to participate in the properties of both. It is thus that the plates perpendicular to two parallel faces of the prism, and passing through its axis, assume a disposition of nodal lines in which the direction of the planes of cleavage, parallel to one of the faces of the pyramid, exercises a considerable influence.

In the plates of this series (fig. 4, *bis*,) neither mode of division is constant; nevertheless, in order that they may be easily distinguished from each other, I have continued to indicate them, one by uninterrupted lines, the other by dotted lines. And for the purpose of preserving, in all the plates, the projection *x y* of the axis parallel to the axis X Y of fig. 1, I have here supposed that the crystal had been turned round until its edge *b e′* was in front. This is besides sufficiently indicated by figure A, which represents the modes of division of the plate perpendicular to the axis, as it also does the section of the hexahedron by a plane parallel to this plate.

The inspection of figures A, B, C, D, E.... shows that the nodal system indicated by the perfect lines is formed of two hyperbolic branches which at first straighten themselves, and the summits of which recede more from each other, so far as the plate E inclined 51° to the axis, beyond which they approach each other until they coalesce in K, after which they again diverge until the plate N, which is parallel to the axis.

The nodal system indicated by the dotted lines follows another course; the summits of the two curves which compose it at first recede from each other, but they soon reapproach each other, and these curves transform themselves into two straight lines in the plate E, where the curves of the other mode of division attain their *maximum* of recession: beyond this limit they separate, but in a perpendicular direction to that in which they approached, and they attain their *maximum* of recession towards the plate H, for which the two systems of curves are nearly similar: they afterwards approach each other, and like those of the other system, they transform themselves, in K, into two straight lines, which intersect each other at right angles. Lastly, starting from this point, they diverge again, until the plate N, for which the two systems again become equal, assuming, with respect to the axis of the crystal, a direction different from that which they had taken at I and at H. I must observe that my supply of rock crystal having failed at the end of my experiments, I have not been able to cut the plate K; but the transformations of the nodal lines so clearly indicate that there ought to be a plate which presents these modes of division, that I have not hesitated to admit its existence.

The course which the two sounds follow, in this series of plates, is much more simple than that of the nodal figures: at first those of the dotted system become lower, commencing with the plate A, and proceeding as far as the plate E, inclined 51° to the axis, and which gives the sound *C* like the plate No. 4, inclined the same number of degrees to the axis; afterwards the sound of this system gradually ascends until the plate N parallel to the axis, where it attains its *maximum* of elevation. As to the sounds of the other series of modes of division, it is observed that they gradually ascend from the plate perpendicular to the axis unto K, in which the nodal systems both consist of lines crossed rectangularly, and that they afterwards descend again until the plate N parallel to the axis. It is obvious that it is not necessary to examine such plates as A′, B′, C′, D′, fig. 4, since they ought to present the same phænomena as the corresponding plates A, B, C, D: only, that which was inclined to the right of the axis in the plates B, C, D is found inclined to the left in the plates B′, C′, D′.

There is none of the modes of division of this series which is not analogous to some one of those which have been presented to us by bodies in which there are evidently three rectangular axes of elasticity; nevertheless, considered all together, the transformations we have just described present peculiarities which do not exist in the fourth series of plates of wood, fig. 14, Pl. III. The most striking consists in this, that in the transformations of this last series, none of the systems, except the first and the last, was rectangular, whilst in rock crystal this mode of division may establish itself.

Summary.

1st. The elasticity of all the diameters of any plane perpendicular to the axis of a prism of rock crystal, may be considered as being sensibly the same.

2nd. All the planes parallel to the axis are far from possessing the same elastic state; but if any three of these planes be taken, restricting ourselves only to this condition, that the angles which they form with each other are equal, then their elastic state is the same.

3rd. The transformations of the nodal lines of a series of plates cut round one of the edges of the base of the prism are perfectly analogous to those which are observed in a series of plates cut round the intermediate axis in bodies which possess three unequal and rectangular axes of elasticity.

4th. The transformations of a series of plates perpendicular to any one of the three planes which pass through two opposite edges of the hexahedron are, in general, analogous to those of a series of plates cut round a line which divides into two equal parts the plane angle included between two of the three axes of elasticity in bodies where these axes are unequal and rectangular.

5th. By means of the acoustic figures of a plate cut in a prism of rock crystal, nearly parallel to the axis, and not parallel to the two faces of the hexahedron, we can always distinguish which are the faces of the pyramid susceptible of cleavage. The same result may be obtained by the disposition of the modes of division of a plate taken nearly parallel to one of the faces of the pyramids.

6th. Whatever be the direction of the plates, the optical axis, or its projection on their plane, always occupies a position on them which is intimately connected with the arrangement of the acoustic lines: thus, for example, in all the plates cut round one of the edges of the base of the prism, the optical axis, or its projection, invariably corresponds with one of the two straight lines which compose the nodal system formed of two lines which intersect each other rectangularly.

Although there is doubtless a great analogy between the phænomena which rock crystal has just presented to us, and those we have observed in bodies in which the elasticity is different according to three directions perpendicular to each other, nevertheless we are forced to acknowledge that, with respect to the mode of experiment we have employed in these researches, rock crystal cannot be placed in the number of substances with three rectangular and unequal axes of elasticity, and still less in the number of those all the parts of which are symmetrically arranged round a single straight line. For the same phænomena are constantly reproduced in it in three different positions; and it seems that everything in it has reference to the different directions of cleavage, to the faces, and to the edges of the primitive rhombohedron. Thus all the plates cut parallel to the natural faces of the hexahedron possess exactly the same properties, and these properties are very different from those of the plates equally parallel to the axis, but which are normal to two faces of the hexahedron. Likewise, the plates parallel to the cleavable faces of the pyramid produce the same sounds, and exhibit the same acoustic figures; whilst the plates parallel to the three other faces present figures different from those of the preceding plates. It appears therefore to result from this identity of phænomena for three distinct positions, that there are in rock crystal three systems of axes or principal lines of elasticity.

But in this point of view, what would be the precise directions of these axes for each system? This can, to a certain point, be determined by comparing the phænomena we have observed in rock crystal with those presented to us by wood. For, all the plates cut round one of the edges which result from the junction of a face of the pyramid with the adjacent face of the hexahedron, producing a nodal system composed of two lines cutting each other rectangularly, one of which always corresponds with the edge in question; and the transformations of the acoustic lines in it being entirely analogous to those of a series of plates cut round the intermediate axis in wood, it follows that this edge, which is nothing else than the great diagonal of the primitive rhombohedron, ought to be regarded as the intermediate axis of elasticity. In the next place, as the *maximum* of straightening and of deviation of the branches of the nodal hyperbola takes place in the plate No. 11, fig. 3, *bis*, parallel to the cleavable face of the pyramid, and as at the same time this plate is a limit for the sounds which it produces, it is equally natural to suppose that it ought also to contain in its plane another axis of elasticity, which can correspond only to the second of the crossed nodal lines, that is to say, to that which serves as the second axis of the nodal hyperbola, and which is, at the same time, the smaller diagonal of the lozenge face of the primitive rhombohedron. This line may therefore be considered as the axis of greatest elasticity of each system. Lastly, following the same analogy, as the plate which is cut parallel to the diagonal plane, the intersection of which with the lozenge face of the rhombohedron forms its great diagonal, is besides a *maximum* of deviation for the summits of the nodal hyperbola, it must thence be concluded that this plane contains the axis of least elasticity, and, at the same time, that this axis is perpendicular to the intermediate axis, and forms with that of greatest elasticity an angle of 57° 40′ 13″, since such is the inclination of the face of the rhombohedron to the diagonal plane. Thus, first, the axis of greatest elasticity and the intermediate axis are contained in the plane which forms the face of the rhombohedron, and they are perpendicular to each other; secondly, the intermediate axis and the axis of least elasticity are contained in the diagonal plane, and they are in like manner perpendicular to each other.

Such are the consequences to which the analogy observed between the successive transformations of the nodal lines in plates of wood and of rock crystal seems to lead. The co-existence of three systems of axes of elasticity in the latter body, introduces however so great a complication in the particulars of the phænomenon, especially in the progression of the sounds, that the elastic state of this substance can only be definitively determined by a method analogous to that which I have above employed for wood, that is to say, by comparing together the numbers of vibrations of a series of small rods of the same dimensions, and cut according to the different directions in which the preceding experiments appear to indicate that the elasticity differs the most. Without in the least prejudging the results to which these new researches might lead us, we may even now foresee that there ought to be a great difference between the greatest and the least degree of elasticity in rock crystal, since, among the various plates of beech-wood, a substance in which these two extremes are as one to sixteen, there is none the sounds of which have a greater interval than that of a major third between them, whilst, among the plates of crystal, there are some, the two sounds of which are a fifth from each other.

As we have already remarked above, the transparent carbonate of lime and the ferriferous carbonate of lime appear to possess elastic properties which are, for the most part, analogous to those of rock crystal; three systems of principal lines of elasticity, which appear exactly similar to each other, are likewise recognised in them; but the extreme facility with which carbonate of lime may be cleaved, enables us to discover in it a peculiarity which cannot be perceived in rock crystal, and which may explain why it is that the plates cut round one of the edges of the base of the hexahedron, all present a nodal system composed of two lines crossed rectangularly.

It is well known that the rhombohedron of carbonate of lime is frequently susceptible of a mechanical division according to the directions parallel to its diagonal planes; now, these planes cutting each other perpendicularly two and two, the intersection of each of these pairs with the lozenge faces of the crystal, forms the great and small diagonal of each of them, so that, if a plane be imagined which turns round the great diagonal, it ought always to remain normal to the supernumerary joint which passes through the small diagonal. It hence results that, if a series of plates be cut round the same line, their structure, considered in the different directions of their plane, will differ according to two directions perpendicular to each other; this explains the production of the nodal lines crossed at right angles, as in the series of plates cut round one of the axes of elasticity, in bodies in which these axes are rectangular. It appears therefore that we may conclude from this observation that rock crystal possesses, like carbonate of lime, supernumerary planes of cleavage parallel to the diagonal planes of its primitive rhombohedron, and that it is to the existence of these supernumerary joints that the principal peculiarities of the elastic state of this substance must be attributed.

The only striking difference there appears to be between the structure of carbonate of lime and that of quartz consists in this, that, in the first of these substances, the small diagonal of the rhombohedron is the axis of least elasticity, whilst it is that of greatest elasticity in the second. To be convinced of the accuracy of this assertion, it is sufficient to cut, in a rhombohedron of carbonate of lime, a plate taken parallel to one of its natural faces, and to examine the arrangement of its two nodal systems, one of which consists of two lines crossed rectangularly, which are always placed on the diagonals of the lozenge, the primitive outline of the plate, and the other is formed of two hyperbolic branches, to which the preceding lines serve as axes (see fig. 7, *bis*, No. 6); but with this peculiarity, that it is the small diagonal which becomes the first axis of the hyperbola, whilst it is its second axis in the corresponding plate of rock crystal (see fig. 3, *bis*, No. 11). It may be here asked how far this difference of structure may influence the phænomena of light which are peculiar to each of these two substances, one of which is a crystal with attractive (positive) double refraction, and the other with repulsive (negative) double refraction.

It appears, therefore, to result from this approximation between the phænomena presented by carbonate of lime and rock crystal, with respect to sonorous vibrations, that the arrangement of the acoustic figures, and the numbers of vibrations by which they are accompanied, are always found intimately connected with the directions of cleavage in each plate; and it may be said in general, that if these directions intersect each other at right angles, in the plane of the plate, one of the two modes of division will always consist of two lines crossed rectangularly; whilst if they are inclined to each other the two nodal systems will be hyperbolic curves.

The disposition of the nodal lines upon circular plates of sulphate of lime gives additional support to this conclusion. For thin plates of this substance break according to two directions inclined to each other at 113° 8′; and experiment shows that the two modes of division of which they are susceptible are two nearly similar hyperbolic curves, one of which appears to have for its asymptotes the directions of cleavage, and the other for its principal axis that one of these two directions in which the plates do not break clean off; for there is, it is well known, an obvious difference in the manner in which sulphate of lime breaks according to one direction or the other. We will, on concluding, remark, that these modes of division are precisely the same as those of a disc of rock crystal parallel to the axis and perpendicular to two faces of the hexahedron, and that the mean of the optic axes in sulphate of lime occupies in it the same position relatively to the nodal curves, as the projection of the single axis of rock crystal assumes in that of the plates of this substance of which we have just spoken. (See fig. 2, *bis*, No. 3.)

The preceding researches are, doubtless, far from deserving to be considered as a complete examination of the elastic state of rock crystal and of carbonate of lime; nevertheless we hope they will be sufficient to show that the mode of experiment we have employed may hereafter become a powerful means of studying the structure of solid bodies, regularly or even confusedly crystallized. Thus, for instance, the relations which exist between the modes of division and the primitive form of crystals allow us to presume that the primitive form of certain substances which do not at all yield to a mere mechanical division may be determined by sonorous vibrations. It is equally natural to think that less imperfect notions respecting the elastic state and cohesion of crystals than those we now possess, may throw light upon many peculiarities of crystallization: for example, it is not impossible that the degrees of elasticity of a determinate substance may not be exactly the same, for the same direction referred to the primitive form, when the secondary form is different; and, if it be so, as some facts induce me to suspect, the determination of the elastic state of crystals will lead to the explanation of the most complicated phænomena of the structure of bodies. Lastly, it appears that the comparison of the results furnished concerning the constitution of bodies, on the one hand by means of light, and on the other hand by means of sonorous vibrations, ought necessarily to contribute to the progress of light itself, as well as to that of acoustics.

ARTICLE XII.

Researches concerning the Nature of the Bleaching Compounds of Chlorine; by J. A. BALARD.

From the *Annales de Chimie et de Physique*, vol. lvii. p. 225.

AMONG the remarkable properties which chlorine possesses, there is one which became advantageous to manufactures very shortly after its discovery, viz. its energetic action on colouring matter. The illustrious Swede to whom we owe the knowledge of this body, mentioned the facility with which it destroys vegetable colours; but that which to Scheele was merely an interesting experiment, became to Berthollet the basis of a new art. Berthollet conceived the happy idea of applying the decolorizing property of chlorine to the purpose of bleaching, and the success which he obtained even in his first attempts exceeded his hopes. Up to this period, cotton and linen manufactures were spread in meadows, and by exposing them alternately moist and dry to heat and cold, light and shade, they were indeed, after a very long time, perfectly bleached.

The eagerness with which a new process was welcomed will be conceived, when manufacturers could produce by it in a few hours what previously occupied several months. The new process of bleaching, to which public gratitude gave the name of the Berthollean method, was generally adopted, and chlorine thus proceeded from the laboratory of the chemist to the workshop of the arts. Manufactures were first bleached with chlorine gas, and afterwards with the aqueous solution; but it was soon found that its penetrating smell, and its powerful action on the lungs, were very prejudicial to the workmen employed. In his endeavours to free them from these dangerous exhalations, Berthollet perceived that by the addition of a little quicklime, and even carbonate of lime or of magnesia, the penetrating smell of the chlorine was removed from the aqueous solution, without diminishing its bleaching power. This important observation conducted him to another still more important. He stated that if, instead of dissolving the alkali in aqueous solution of chlorine, a current of the gas was made to pass into the alkaline solution, it would dissolve a much larger quantity than mere water, and the liquid possessed decolorizing power in a much higher degree.

These new compounds, in which the chlorine seemed in some degree deprived of its hurtful properties, while it retained only those which are useful, were soon generally employed in the art of bleaching. They were at first prepared with the solution, and as the first trials were made in the manufactory of Javelle, the new liquid with which industry was enriched was called *eau de Javelle.* But in 1798, Tennant and Knox of Glasgow attempted to substitute hydrate of lime, which forms a solid bleaching compound, while chlorine retains its power only when diluted with a large quantity of water. This substitution was generally adopted, and the new compound, which is more easily preserved and prepared, and at a less price, and more easy of transport, soon became an article of manufacture and considerable commerce, under the name of bleaching powder.

The employment of these compounds of chlorine was further extended in 1822. M. Labarraque, an apothecary of Paris, proved at this period, by numerous trials, that these compounds, which had rendered so many services to the art of bleaching, might be as successfully used for disinfecting. His own trials, and the fresh proofs that his example incited, placed the decolorizing compound of chlorine decidedly among the most valuable resources of the art of preserving health.

We should be at first inclined to believe that the nature of these compounds, which had rendered us such various services, was perfectly understood by chemists; but it is not at all so; and notwithstanding the researches which they have occasioned, the place which they ought to occupy in a classification is not clearly determined. It is, indeed, true that their elementary composition and immediate analysis are well known. In fact, obtained by the action of chlorine upon a metallic oxide, they evidently must be formed merely of chlorine, oxygen, and a metal; on the other hand, the experiments of several chemists have proved that in these compounds, for every two atoms of chlorine*, there is one atom each of oxygen and metal. But how are these three elements arranged? This is not at present known with certainty; and yet this knowledge is indispensable for determining by what reactions they serve in decolorizing and disinfecting.

§ 1. *Of the Opinions which have been entertained as to the Nature of the decolorizing Compounds of Chlorine.*

The opinions of chemists on this subject are divisible into two hypotheses. According to some, these compounds are merely chlorides of oxides; according to others, they are to be considered as mixtures of metallic chlorides with a salt which contains an acid of chlorine, less oxygenated than the chloric, and which it has been proposed to call chlorous acid.

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* Not so in England, but foreign chemists reckon the weight of chlorine only half that of English chemists, hence the author states two atoms.—Ed.

On the first supposition it is admitted, that chlorine, in acting on some metallic oxides, combines with, without decomposing them, so as to form compounds which are not very permanent. The gas being thus but slightly retained, acts upon vegetable colours as if it were free; that is to say, it destroys them, either by dehydrogenating them in a direct manner, or occasioning their oxidation by means of the oxygen of the water. The chlorine, by taking away the hydrogen, either of the water or the colouring matter itself, is converted into hydrochloric acid, and subsequently into a hydrochlorate.

On the second supposition, on the contrary, it is supposed that the chlorine acts upon the metallic oxide employed, so as to decompose part of it; that one portion of this chlorine unites to the metal to form a chloride, and the other to its oxygen to become chlorous acid; and that this, saturating the portion of the base undecomposed, thus forms a true chlorite. In this manner of regarding the subject, the product obtained is complex, and contains a mixture of chloride and chlorite. It is thus supposed that chlorine and water act upon the metallic oxides like sulphur, which, under the same circumstances, produces a mixture of sulphuret and hyposulphite. It is also supposed that these chlorites, coming into contact with putrid organic or colouring matter, yield to them all the oxygen both of their acid and base, and are converted into chlorides; and that it is thus entirely by an oxidizing action that they serve as decolorants and disinfectants.

In attempting to resolve the question *à priori*, upon theoretical considerations, we are tempted to consider this last supposition as the most probable. In fact, the combinations of simple with compound bodies are not common; and although the hydrates of chlorine, bromine, and phosphorus are incontestible examples of the union of a simple body with an oxygenated compound, combinations of this nature are not numerous. It is therefore good logic to admit of the existence of similar compounds, only when the phænomena which are concerned in their production cannot be explained by other views more consistent with general facts. It appears, also, difficult to suppose, that a body which so readily combines with the metals as chlorine does, could unite with their oxides without decomposing them, as is the case with the other metalloids, and remain in contact with the metals, with which it forms very neutral and permanent compounds, without so doing.

The facts hitherto observed agree with theory, and seem to support in preference the hypothesis of the chlorites.

Chemists, indeed, considering that the compounds which we are now considering had the property of disinfecting and decolorizing, like chlorine itself, were at first induced to think that this body existed in them in some sort of ephemeral combination, which allowed of its exerting the same kind of action as if it were free. But it has been since admitted, that it is not only chlorine and the analogous bodies which possess decolorizing power; the same property was found in oxygenated water, and the hypermanganates; and at present everything tends to the opinion that oxygenating agents are as effective as chlorine in producing decoloration. Perhaps indeed, as many chemists suppose, chlorine, when acting with water on coloured bodies, produces this effect by indirect oxidation, induced by its tendency to combine with hydrogen.

Welter had, however, made an experiment on this subject which seemed to prove the existence of chlorides of oxides; he found that the decolorizing power of chlorine was constant, whether it was free, dissolved in water, or combined with an oxide.

This remarkable fact could scarcely be explained but by supposing that the chlorine existed in the two cases in analogous conditions. Supposing, therefore, that it was in a state of solution in water saturated with chlorine, it must be admitted that it existed in the state of a chloride of oxide in the decolorizing compound; or, if the latter was a chlorite, it followed that the solution of chlorine in water was a mixture of chlorous and hydrochloric acid; for how can it be supposed that two different bodies, producing decoloration by different causes, can effect it with precisely the same efficacy? The greater number of chemists adopted the first supposition; Berzelius alone preferred the second, although it appeared to be less probable.

The experiments of Soubeiran have since explained these facts. They have shown that Welter's statement was correct only when a solution of sulphate of indigo was used, which, on account of the sulphuric acid it contained, decomposed the decolorizing chloride, and evolved all the chlorine which had served to form it. But if an ink which is not acid or a vegetable infusion be employed as a chlorometric liquor, it is found that the decolorizing power is no longer the same, and that it may always be increased by more than half, by means of an acid which sets free the chlorine contained in the solution of the chlorides.

The property which is possessed by the decolorizing chlorides, of yielding the whole of the chlorine which they contain by the action of the weakest acids, such, for example, as the carbonic acid, has been regarded as a strong proof in favour of their being chlorides of oxides; and it must be admitted that these phænomena of decomposition are much more easily explained by this hypothesis than the other. Nothing is in fact more easily conceivable than the action of an acid combining with a base, and thus disengaging the simple body with which it had formed an ephemeral compound. But this disengagement of chlorine is also easily explained by the hypothesis of chlorites; for it may be conceived that the chlorous acid, set free by the acids themselves, determines a double decomposition, by reacting upon the metallic chlorides with which the chlorites are necessarily mixed by the very mode of their preparation. The results of this double decomposition would be, the oxidation of the metal of the chloride, which in this new state would saturate, as the base of the chlorite, the acid employed; and a disengagement of gaseous chlorine would take place from a double source: from the chlorous acid and the metallic chloride.

Although this explanation may appear less natural than the other, it is nevertheless supported by analogous chemical facts. If, for example, a mixture of phosphuret of calcium and phosphate of lime be made red hot, phosphorus is evolved, and the residue is lime nearly pure; this for a long time favoured the supposition that this mixture was a phosphuret of oxide, whilst the contrary is now demonstrated.

The phænomena of oxidizement produced by the action of decolorizing compounds do not clear up the doubts which exist as to their true nature, for these phænomena may be explained by both hypotheses. Undoubtedly some are more readily so by one and some by the other, but there is no one to which they may not both be strictly applied.

We are, however, indebted to M. Liebig for some experiments which seem to render the hypothesis of the chlorites preferable. This able chemist observed that chlorine could not only expel carbonic acid from the bicarbonates, but also acetic acid, which is stronger, so as to form decolorizing compounds. Now it is difficult to conceive that a simple body can expel an acid from combination with a base; it is more natural to suppose that it is another acid, which overcomes the affinity even of the acetic acid; and this circumstance seems to justify the supposition of the existence of chlorous acid. It may seem astonishing, at first sight, that an acid so weak as chlorous acid, and which may be expelled from its combinations by carbonic acid, should, in its turn, expel acetic acid from its combinations. But the science presents us with facts equally singular, and which are well ascertained. Acetic acid itself, for example, decomposes the carbonates, and yet carbonic acid acting upon acetate of lead precipitates carbonate from it, and sets acetic acid free, which may be distilled.

Berzelius first undertook this subject, and among other interesting experiments, we are indebted to him for one, which, if it has not entirely settled the question, has at least thrown great light upon it. On passing a current of chlorine into a solution of carbonate of potash, saturated with chloride of potassium, this learned chemist observed, that from the first moment of the disengagement, the liquor became strongly decolorizing, and much pure chloride of potassium was deposited. The first action of the chlorine on the metallic oxide seemed to produce chloride of potassium. Then, as no chlorate was as yet deposited, and as in this experiment no deutoxide of hydrogen is formed, nor is any oxygen disengaged, it must be admitted that what is expelled from the metal by the chlorine has acted upon a portion of this elementary body, and formed with it some oxygenated compound, which is not chloric acid. Although this fact may be consistently explained by supposing that the presence of a chloride of an oxide in the saturated solution of chloride of potassium has diminished, in this case, the solvent power of the liquid with respect to this compound, and that the salt which is obtained is only a portion of that which previously existed in the liquor, and thus is not produced by the action of the chlorine, as supposed by Berzelius, the first explanation is yet the most probable, and induces the belief that metallic chlorides exist ready formed in the decolorizing compounds.

M. Soubeiran has confirmed this fact by an experiment which appears to me to be at present the only one not liable to objections. After having determined, by a preliminary trial, the intensity of the decolorizing power of a given volume of chloride of soda, he evaporated it *in vacuo* to dryness. He has stated that during the evaporation cubic crystals of chloride of sodium are formed, which may be separated in a state of perfect purity; and that the remaining solid residue dissolved in water and tested with a coloured but not acid liquor, possessed absolutely the same decolorizing power as the liquid from which it was procured. This decolorizing power not having suffered any diminution, it cannot be admitted that the chloride of sodium obtained was the product of the decomposition of the decolorizing compound. This chloride of sodium, therefore, existed in the solution of the alkaline chloride before its evaporation. If, then, in acting upon an alkali, the chlorine had formed chloride of sodium, without the production of a corresponding quantity of chlorate, of oxygenated water, or of gaseous oxygen, it necessarily follows that an oxygenated compound was formed, different from chloric acid.

The crystallization of chlorite of soda *in vacuo*, led M. Soubeiran to hope that he should succeed in isolating chlorous acid. But the continuation of his researches, although announced three years since, has not yet been published.

It will be observed, from what has preceded, that there may still exist among chemists some indecision as to the choice which may be made between the two hypotheses proposed as to the nature of the decolorizing compounds of chlorine. Although the hypothesis of chlorites is by much the most probable, it is nevertheless true, that not only chlorous acid has not been obtained in a free state, but even chlorites also; they not having been yet procured, but in a state of mixture with the metallic chlorides.

Thus, although very probable, the existence of these salts is far from being demonstrated, and the composition of chlorous acid, which was supposed to be formed of two volumes of chlorine and three volumes of oxygen, remains undecided.

It appeared, therefore, to me desirable to attempt some fresh experiments, with the endeavour of elucidating a theoretical chemical question, which is not only of itself of considerable importance, but the solution of which might also throw some light on the true mode of action possessed by these decolorizing compounds, the uses of which are daily multiplied in medicine and the arts.

I think that I have succeeded in proving that these compounds are really saline combinations of a peculiar acid formed of chlorine and oxygen. It is this acid, which I have isolated, that forms the subject of this essay, in which I shall treat successively of the manner of obtaining it, of the properties by which it is distinguished, the proofs of its composition, and the generic characters of the combinations which it forms.

Until a knowledge of the proportion of its elements, of which I shall treat in a following paragraph, allows of my stating the true name which the rules of chemical nomenclature assign to this acid, I shall continue to call it chlorous acid, and designate its compounds by the name of chlorites.

§ 2. *On the Processes by which Chlorous Acid may be prepared.*

When reflecting on the best mode of proceeding in these researches, it will be soon perceived in reasoning on the hypothesis of the existence of chlorites, that the question would be on the point of being determined, if the supposed chlorite could be separated from the chloride with which it is considered as mixed in the decolorizing compound. Nothing would be easier to perform, if there existed a metal which would form with chlorine a compound soluble in water, and the oxide of which could at the same time form with the chlorous acid a compound insoluble in this liquid. But, unfortunately, all the known decolorizing compounds are soluble in water, and therefore nothing is to be hoped for in this respect.

This separation would also be very easy if, on the contrary, a metal was known which would form an insoluble compound with chlorine, and the oxide of which, by uniting with chlorous acid, would form a soluble and stable compound, up to a certain point. But the metallic chlorides are all soluble in water, except chloride of silver, lead, and protochloride of mercury; these three metals are therefore evidently those only which afford a choice.

Economical motives made me at first think of the salts of protoxide of mercury and of lead; but I soon found that no good result could be obtained by employing them.

When a solution of chloride of lime or of soda is treated with protonitrate of mercury, there is immediately precipitated a great quantity of protochloride of mercury, and the supernatant liquid is strongly decolorizing; but this property soon disappears, and the liquor then contains a notable quantity of deutochloride of mercury: the precipitate soon becomes red, and changes to an oxichloride.

The salts of lead possess no advantage over those of mercury. If a solution of acetate or nitrate of lead be poured into a decolorizing chloride, a precipitate of chloride of lead is immediately formed; but this chloride is itself susceptible of being altered by the chlorite. If it is not soon separated from the liquor, it becomes quickly brown, is converted into peroxide, and gives out a strong smell of chlorine. This double phænomenon is undoubtedly effected by the decomposition of the chlorous acid.

Since the salts of lead did not, any more than those of mercury, lead to the end intended, I was compelled to have recourse to the action of those of silver; they are more costly, it is true, but they apparently would be followed with more success. My attempt was not altogether unsuccessful. Nevertheless the employment of the salts of silver is attended with some inconveniences which it is requisite to state.

If a neutral solution of nitrate of silver be precipitated by a solution of chloride of lime, containing a slight excess of lime, a great quantity of chloride of silver is precipitated, and there is formed at the same time oxide of silver, which imparts a grey colour to the deposit obtained. The supernatant liquid is strongly decolorizing; but if an attempt be made to separate it by filtration, a brisk effervescence is produced as soon as a little has filtered, and when the filtration is over the liquor has completely lost its decolorizing property. The gas which is evolved in this case is oxygen. Berzelius had previously observed the phænomenon which I have now mentioned, and discovered the cause of it. I have ascertained in operating directly with the chlorites and the oxide of silver, that these salts occasioned the formation of chloride of silver and a disengagement of oxygen. This oxygen is produced, both from the chlorous acid and the decomposed oxide. A portion of this oxygen gas evolved is absorbed by the portion of oxide in excess in the liquor, and converts it into peroxide, whilst the remainder is given out in the state of gas. In trying to obtain free chlorites, the precipitation of oxide of silver must be avoided, and the decolorizing chlorides must have no excess of alkali, which is avoided by neutralizing them with nitric acid; but it is requisite not to add excess of it, which would be as active, though a different cause of the decomposition of the chlorite.

If, indeed, a solution of chloride of lime supersaturated with nitric acid be precipitated by nitrate of silver, the mass of chloride of silver formed is soon raised by abundant bubbles of chlorine gas, and the decolorizing property is in a great degree lost. If an attempt be made quickly to remove the chloride of silver, which is the cause of this decomposition, from the supernatant liquid, by throwing it in a cloth and pressing it strongly, the reaction is accompanied with a disengagement of a very considerable degree of heat. Some direct experiments, which I shall state hereafter, have proved that the chlorous acid, which is in this case set at liberty, exerts the same re-action on the chloride of silver which it produces on the other chlorides, and that the presence of a small excess of nitric acid much accelerates this decomposition.

It will be observed, then, that in order to have a chance of success in this operation, it is requisite to make use of a decolorizing chloride which is perfectly neutral. It is impossible to prove that it is in this state by means of coloured tests, for their tint is not merely modified but completely destroyed by the chlorides. It is preferable to prepare it by adding nitric acid drop by drop, and until the precipitate formed by the chloride in the solution of silver ceases to have the brown tint which the oxide of silver communicates to it when they are mixed.

When this perfect neutralization is attained, and not exceeded, the metallic chloride and the alkaline chlorite are decomposed, chloride of silver is deposited, and the liquid possesses decolorizing power in a very high degree, which is undoubtedly due to the chlorite of silver remaining in the liquor.

But the substance which it contains is extremely easy of decomposition; it was impossible to obtain the liquid clear even by filtration; it becomes turbid, precipitates, and deposits much chloride of silver. The liquor gradually ceases to be decolorizing, and it then contains chlorate of silver. Thus the attempts which I made to separate chlorous acid have almost always been fruitless, and therefore it was necessary to make fresh ones.

According to what I have stated as to the action of the salts of silver on the decolorizing compounds of chlorine, it appears to me that it must be admitted that the oxide of this metal can form with chlorine compounds similar to those which it yields with the alkalis, though they are less permanent. It seemed to me therefore proper, as I had so little success in treating the alkaline chlorides with the salts of silver, to try the action of oxide of silver upon chlorine itself.

Chemists are generally of opinion that, when chlorine acts upon the salts of silver, it converts them into chlorate and chloride. Vauquelin mentions having observed the same phænomena in treating chlorine with free oxide of silver simply diffused in water. But I had every reason to think, according to the facts above stated, that these two compounds were formed only by the decomposition of a chlorite.

Some pure oxide of silver was therefore suspended in distilled water, and agitated with chlorine. This was absorbed. The portion of oxide which was in contact with the chlorine formed a white compound, and the other portion became of a very deep black colour. I ascertained that the first was chloride of silver; as to the second, it possessed all the characters of peroxide of silver. During this re-action heat was evolved, but I did not perceive any disengagement of oxygen. This liquid immediately after filtration was limpid and strongly decolorizing, but it did not long retain either of these properties; in a few seconds, and without any action of the air towards the production of the phænomena, it became turbid, deposited chloride of silver, and the liquor contained chlorate.

Analogous phænomena occur when the solutions of any salt of silver, the nitrate, acetate, chlorate, &c. are treated with chlorine. Chlorate of silver is formed, and the acids of these salts are set free. But the filtered liquor, which in its limpid state decolorizes strongly, very soon loses its limpidity and its decolorizing power; a chloride is deposited, and chlorate of silver remains in solution.

The facts which I have now adduced show that, whether the decolorizing combinations of chlorine with the alkalis are treated with nitrate of silver, or chlorine is made to act upon oxide of silver, or finally a salt of silver itself is acted upon by this agent, a soluble combination of silver is obtained which is strongly decolorizing, and which every circumstance leads to the supposition of being a chlorite; but that this combination is almost ephemeral, and changes rapidly at common temperatures into chlorate and chloride of silver.

It would be in vain to attempt the conversion of this compound into another of the same nature, and more stable, by treating it with an alkaline substance; for this conversion could not be effected without precipitating the oxide of silver, and this could not fail to re-act on the chlorite in the manner which I have already indicated; that is to say, it would be changed into chloride and oxide of silver, and would disengage oxygen gas.

That which appeared to me to be the most efficacious manner of retarding a decomposition, which in the heat of summer proceeds with great rapidity, and quickly destroys the chlorous acid, consists in precipitating the base of the chlorite of silver by chlorine itself. I have already mentioned that chlorine, in acting upon any salt of silver whatever, decomposes its base; and these two bodies are converted into chloride of silver and chlorous acid.

It is therefore evident that if this chlorine be made to act upon a liquid which contains chlorite of silver, there can only be obtained, as a last result, insoluble chloride of silver and chlorous acid in solution, derived from a double origin. This is, in fact, what constantly happens, when in the execution of one of three operations which I have mentioned a slight excess of chlorine is employed.

The liquid which is obtained after the separation of chloride of silver by filtration is not however pure chlorous acid. When a decolorizing compound of chlorine is precipitated by nitrate of silver, it contains a nitrate of the base employed, besides chlorous acid. If a salt of silver has been decomposed by chlorine, it contains the acid which forms part of these salts, mixed with chlorous acid. Lastly, in the case even in which the operation is conducted with chlorine and oxide of silver diffused in water, the chlorous acid, which would seem to be pure, is nevertheless mixed with a large quantity of chloric acid.

Indeed, when chlorine is agitated with oxide of silver, it is an operation which, whatever be the rapidity with which it is conducted, requires a minute or two that the absorption of the chlorine may be complete; even in this case a portion of the chlorite is decomposed, and converted, as usually happens, into chloride and chlorate, and the latter decomposed in its turn by the chlorine produces chloric acid. Thus, in whatever manner the operation is conducted, a notable portion of the chlorous acid is changed into this new compound.

But this chlorous acid is fortunately possessed of a volatility which allows of its separation from the bodies with which it is mixed, and it may be obtained diluted with water by distilling the liquid prepared by employing one of these three methods. However, as a high temperature may decompose it in part, and as at the heat of boiling water some of the substances with which it is mixed, as hydrochloric and nitric acid, may come over in distillation, it is better to operate in vacuo, or at least under low pressure, at a temperature below 212° Fahrenheit. A solution of chlorous acid is thus obtained, but diluted with much water. The first product is the richest in chlorous acid; if this be kept apart and be again distilled, chlorous acid may be obtained in a concentrated state.

These methods supply, it is true, but very small quantities, and I should have renounced the study of the properties of this new oxigenated compound of chlorine, if I had not discovered a more economical and productive process. This method consists in treating chlorine with red oxide of mercury suspended in water.

The action of chlorine upon this compound has been already studied by M. Grouvelle. This chemist has stated that oxichloride of mercury, very slightly soluble in cold water, was formed. M. Thénard, on the other hand, has observed that the liquid contained chloride and chlorate of mercury also in solution; but I am at liberty to suppose that these bodies are only formed consecutively, and that their existence had been preceded by that of a mercurial chlorite, as takes place with the salts of silver.

Certain theoretical considerations made me conceive some hope in employing oxide of mercury. In fact, if we reflect on the conditions which appear the most favourable for producing and isolating chlorous acid, it will be seen that they are reducible to the four following; 1st, the action of chlorine on a strongly alkaline oxide; 2ndly, that this oxide should form a chlorite possessing a certain degree of stability; 3rdly, that the metallic chloride formed may, on account of its insolubility, easily separate from the chlorite; 4thly, and lastly, that it should re-act but feebly on the chlorous acid when it is separated by distillation. The red oxide of mercury seemed to me to present all these advantages. Its alkaline power is strongly marked. I had no fear that chlorite of mercury could be compared, with respect to its instability, to chlorite of silver, which does not decompose so readily, merely on account of the insolubility of the chloride. Although soluble in water, the chloride of mercury [bichloride] loses much of its solubility by combining with oxide, and thus forming an oxichloride; and this state of combination would retard to a certain point the decomposition which the chlorous acid would readily produce in other circumstances. My attempt was followed by success: by employing this substance, I obtained in fact chlorous acid in larger proportion and more concentrated.

This operation may be conveniently performed as follows: the specific gravity of red oxide of mercury does not allow of its being subjected to the action of chlorine in a Woulfe's apparatus; it is much more convenient to pour the red oxide of mercury reduced to a fine powder by trituration, and mixed with about twelve times its weight of distilled water, into bottles filled with chlorine gas.

By strong agitation the absorption of chlorine takes place rapidly, and operates as quickly as if the gas were treated with an alkaline solution. It has happened to me during this operation, that bottles which were perfectly stopped have broken in my hands, on account of the almost perfect vacuum which is produced in this case. If the proportion of red oxide of mercury employed is insufficient, the powder deposited at the bottom of the bottle is white, and the colour of the gas indicates the presence of chlorine. If the red oxide of mercury, on the contrary, is in slight excess, it colours the deposit spoken of red, and the chlorine then disappears completely. It appeared to me to be preferable to operate with a slight excess of oxide of mercury, to prevent the chlorous acid from being mixed with free chlorine. When the absorption of the chlorine is complete, the matter contained in the bottle should be thrown upon a filter, upon which there remains the greater part of the oxichloride formed: the liquor which filters, subjected to distillation in vacuo, furnishes weak chlorous acid, but it may be brought to greater state of concentration by subjecting the first products to a second distillation.

§ 3.—*Properties of the Aqueous Solution of Chlorous Acid.*

The chlorous acid diluted with water, obtained as above stated, has the following properties: it is a transparent liquid and slightly yellow-coloured when it is concentrated. Its smell is penetrating, and quite distinct from that of chlorine and the deutoxide of chlorine of Davy. It more nearly, however, resembles the first than the second. Its taste is extremely strong, but not sour.

It attacks the epidermis with great activity. A drop left in contact with the skin for half a minute destroys it, and more deeply than nitric acid does in the same space of time. The tint which it acquires is reddish brown, and not yellow.

Chlorous acid when slightly concentrated is extremely unstable, and decomposes partially even at common temperatures. During the great heat of summer it can only be preserved for a few days, except by keeping it in ice. When more diluted and kept from the light it may, on the contrary, be preserved for a much longer time. During this decomposition it disengages an infinite number of small bubbles, which are merely chlorine gas, and at the same time a certain quantity of chloric acid is formed. Agitation, especially with angular bodies, hastens this decomposition; and when fragments of powdered glass are thrown into this acid, their contact with the liquor is followed by a well-marked effervescence.

At a moderately high temperature, the decomposition is much more rapid. Yet it is only partial at 212°, for chlorous acid may be distilled at the usual pressure, and thus brought to a higher degree of concentration.

A strong light produces similar decomposition. A few moments exposure to the solar rays are sufficient to convert it into chlorine and chloric acid. Sometimes also deutoxide of chlorine is formed.

When an aqueous solution of chlorous acid is exposed to the influence of the voltaic pile, an abundant disengagement of oxygen takes place at the positive pole. The portion of the liquid in the midst of which this disengagement is effected, does not appear to change its nature by absorbing a certain quantity of the gas. It does not deepen in colour, nor does its decolorizing property appear to diminish. Thus the action even of nascent oxygen does not seem to have the power of changing chlorous acid into chloric acid or deutoxide of chlorine.

In this experiment no chlorine is disengaged at the positive pole. There is no doubt that chlorous acid and water are simultaneously decomposed, and that the hydrogen and chlorine, meeting in the nascent state, form hydrochloric acid. What tends to induce this opinion is, that at the end of a certain time the oxygen obtained is mixed with chlorine; a phænomenon which could not occur, except there be formed in the liquid a compound in which this body is electro-negative.

Chlorine cannot exert any action on the aqueous solution of chlorous acid; but it is different with bromine and iodine. Each of these bodies is susceptible of decomposing it, and of acidification at the expense of its oxygen.

If a few drops of bromine be put in contact with a small quantity of chlorous acid, a disengagement of chlorine is perceived on the surface of the drops of bromine. On exposing this liquid to the contact of the air for a few seconds, this latter compound is liberated, and free bromic acid remains. This is a process which might be successfully adopted for preparing bromic acid, if others easier of execution did not exist.

Iodine acts in the same manner, but, as might be anticipated, its action is more energetic; when the chlorous acid is concentrated it is accompanied with a slight disengagement of heat. Chlorine is abundantly evolved, and a portion of iodine is acidified, whilst another small portion is changed into chloride of iodine.

The acid formed under these circumstances, treated by nitrate of silver, gives a white and not a yellow precipitate soluble in ammonia, and which is merely iodate of silver. The product of this action is merely iodic and not hyperiodic acid, as might be suspected according to the mode of preparing this acid proposed by MM. Magnus and Ammermuller, to whom the discovery of it is owing.

Among the simple non-metallic combustibles, azote and hydrogen in the gaseous state appear to have no action upon chlorous acid; but sulphur, selenium, phosphorus and arsenic act upon it with great energy. By their contact with this compound they undergo analogous alterations; they are acidified, and give rise to an abundant disengagement of chlorine gas. The sulphur is converted into sulphuric acid, the phosphorus into phosphoric acid, and the arsenic into arsenic acid, as happens when they are treated with nitric acid. As to selenium, it produces also selenic and not selenious acid; that is to say, the chlorous acid performs then what nitric acid itself cannot effect. The whole of the chlorine is not disengaged in the state of gas; a small portion combines with the elementary body, so that there are produced at the same time chloride of phosphorus, of sulphur, of arsenic, &c.; these by the contact of water undergo double decomposition, the products of which are water and additional portions of phosphoric, sulphuric, and arsenic acids.

Charcoal in powder did not appear to me to exert any action on chlorous acid. As to that of boron and silicium, the want of materials has not allowed of my ascertaining it.

Chlorous acid acts variously with metallic substances; potassium thrown in pieces into chlorous acid burns immediately without any disengagement of chlorine being perceived. The product of this combination is formed of chloride of potassium and chlorite of potash. The presence of water, which may complicate the action, does not allow of accurately determining what passes during its operation. It is, however, probable that it is at the expense of the chlorous acid, that these two compounds of chlorine are formed, and that the water merely dissolves the results of this decomposition.

Iron filings, when made to act upon chlorous acid, decompose it instantaneously; the action is accompanied with an abundant emission of heat, and a brisk effervescence produced by the chlorine. The iron is oxidized, the chlorine is partly disengaged, and also in part combines with the metal, which it converts into a liquid of deep yellow colour, which is acid but does not decolorize; it appears to be perchloride of iron; it does not seem to be mixed with chlorate. We might be tempted to suppose that the action of chlorous acid on other metals is comparable to that which it exerts upon iron; but this is not the case. The greater number of the other metallic substances, put into contact with chlorous acid, cannot decompose it, and I am yet entirely ignorant of the cause of the peculiarity of iron in this respect.

Tin filings may remain in contact with weak chlorous acid for several days, without causing it to undergo any sensible decomposition, and without losing their metallic lustre. The same is the case with zinc, with fragments of antimony, bismuth, and lead. But the presence of another acid renders these metals capable of effecting this decomposition.

The nature of the acid employed for this purpose is not a matter of indifference. This acid must form a compound, soluble in water, with the oxide of the metal employed. Thus with zinc and tin, sulphuric acid causes the decomposition of the chlorous acid most rapidly; with antimony, sulphuric acid and even nitric acid does not answer, but tartaric acid succeeds extremely well in causing its oxidation; lastly, chlorous acid, which alone or mixed with sulphuric acid scarcely acts upon lead, exerts an intense action upon it when mixed with a little nitric or even acetic acid.

In this re-action, induced by the presence of these acids mixed with the chlorous acid, the metal, in order that it may saturate them, must decompose the acid, and not the water; for in combining with the oxygen, it is chlorine, and not hydrogen, which it sets at liberty. However when in operating with chlorous and sulphuric acid on zinc or tin, the gas obtained contains a little hydrogen, which indicates that in some cases it is at the expense of the water itself that the oxidation occurs.

If concentrated chlorous acid be used, the action is not at first more energetic, but it becomes so at the end of a certain time. During its spontaneous decomposition chlorous acid forms chloric acid, the mode of action of which is the same as that of sulphuric, nitric, and other acids, as I determined by direct means.

Gold and platina do not appear to suffer any action by chlorous acid, either alone, or mixed with nitric and sulphuric acid; but copper, mercury, and silver decompose it, and each with a peculiar mode of action. Copper filings put into contact with chlorous acid are partly dissolved: the solution after some time is found to contain chloride of copper. There is also formed at the same time a green powder, which appears to be merely oxichloride of copper, and it disengages chlorine mixed with a very small proportion of oxygen.

It is probable that the disposition of oxide of copper to combine with the chloride of this metal to form an oxichloride, contributed to render copper, though less oxidizable than zinc or tin, proper to produce the decomposition of chlorous acid,—an operation which these two last metals cannot effect.

This is probably also the cause which renders the action of mercury so prompt. When a few globules of this metal are shaken with chlorous acid, it is almost instantaneously decomposed, without any apparent disengagement of gas of any kind, and oxichloride of mercury is found to be the product of this re-action. The disposition which the two products of this decomposition have to combine is undoubtedly the reason which renders it so easy of execution.

The kind of alteration which silver causes chlorous acid to undergo, is precisely the reverse of that of the bodies whose action upon this acid has been hitherto described. When very finely divided silver is put into contact with chlorous acid, a brisk effervescence takes place, which is produced by a disengagement of oxygen gas, without any trace of chlorine, and the whole of this combines with the silver and converts it into chloride.

To recapitulate:—The bodies which I have tried, chlorine and the metals of the last section of Thénard excepted, decompose chlorous acid; they are oxigenated at its expense, and set some chlorine at liberty, which can be only partially absorbed by the combustible, except in some peculiar circumstances. Silver alone is an exception to this law of disengaging oxygen: and this phænomenon, as well as the decomposition of fluoride of silver by chlorine, indicates the very peculiar affinity which exists between these two bodies.

Chlorous acid is therefore one of the most energetic oxidizing agents. The property which it has of converting bromine into bromic acid, and selenium into selenic, and not selenious acid, gives it in this respect an incalculable advantage over nitric acid, and up to a certain point over oxigenated water.

To judge of its effects as an oxidizing agent only by its action on the metals, it might perhaps be ranged below nitric acid; but it must be remembered, that the action of nitric acid on metallic substances does not depend on their oxidability alone, and the easy decomposition of the acid, but also on their disposition to form a nitrate, and a nitrate soluble in acid, of the degree of concentration employed. This is proved by the singular action which it exerts upon iron and tin in certain cases.

Chlorous acid not having much power to form salts, except with the oxides of those metals which yield energetic bases, it ought not to cause surprise that they excite so little definite action upon the others.

This power of oxidizement possessed by chlorous acid is a perfectly natural circumstance, and one which exhibits the instability of the oxigenated combinations of chlorine. But what is much less so, is the disengagement of chlorine gas, which almost always accompanies its decomposition. The energetic affinity of chlorine for the greater part of the metalloids and metals is well known; from this it might be supposed that, in the re-action of these bodies upon chlorous acid, the chlorine would combine with them as well as the oxygen, and that this decomposition would take place in a somewhat latent manner, since, each of the elements uniting with the combustible, nothing would be eliminated. It does not however so happen, and the facts which I have related prove that it is almost uniformly by its oxygen that chlorous acid acts upon various simple combustibles.

The phænomena which accompany the decomposition of chlorous acid by the compound combustibles prove the same fact, and show this body to be one of the most energetic agents of oxidation, but at the same time as very little calculated for chloridation.

I have not attempted to put those bodies in contact with this acid which are altered by water itself: for the presence of this fluid in the chlorous acid of which I made use would have complicated the re-actions, and would have prevented me from assigning the products which I should have obtained to their true origin. On this account I have not experimented with the chlorides, bromides, &c. of sulphur, of phosphorus, and of selenium: I preferred employing the action of chlorous acid upon the compound combustibles which suffered no decomposition by water.

The compounds of halogène bodies with carbon are not only unalterable by water, but even resist the action of several of the most energetic chemical agents; I was therefore curious to see how chlorous acid acted upon them.

The several chlorides and bromides of carbon, as well as the hydrocarburets of chlorine and bromine, although attacked by chlorous acid, appear to me to exert a very slow action upon it, and one which I have not hitherto perfectly appreciated. As to the periodide of carbon of Sérullas, that on the contrary is attacked with great energy by chlorous acid. At common temperatures it produces a rapid disengagement of gas, which is a mixture of chlorine, carbonic acid, and oxide of carbon, and iodine is deposited; the liquid contains a mixture of iodic acid and hydrochloric acid. If the chlorous acid is in excess, no deposit of iodine is observed, it being converted into iodic acid.

Chlorous acid also decomposes cyanogen. If a few drops of chlorous acid be added to a bottle containing this gas, effervescence is soon observed, and the bottle is filled with chlorine, recognisable by its yellow tint. At the bottom of the liquid, which has ceased to decolorize, and which has changed its nature, there appear a few drops of an oily liquid, which seems to be a mixture of the chloride of cyanogen and of azote, described by Sérullas. The liquid contains hydrochloric acid and cyanic acid of Sérullas, and the compressed gas which fills the bottle is a mixture of chlorine, azote, carbonic acid, and chloride of cyanogen.

Sulphuret of phosphorus is also decomposed by chlorous acid. The action at first is slow in the cold; but the mixture gradually becomes warm, and it is then more rapid. The elements of these compounds both combine with oxygen, and chlorine is disengaged in abundance. The liquor contains however, besides sulphuric and phosphoric acid, some hydrochloric acid.

Sulphuret of carbon acts in the same manner with chlorous acid; but the effervescence is more brisk, for the gas which is disengaged is a mixture of chlorine and carbonic acid. The liquid contains both sulphuric and hydrochloric acid; which leads to the supposition that, in this case, as in the former, part of the sulphur is converted by the nascent chlorine into chloride of sulphur, which is afterwards decomposed by the contact of the water.

Marsh carburetted hydrogen is not acted on by chlorous acid, either in the dark or in the solar light; but it is not the same with bicarburetted hydrogen. This gas is decomposed at common temperatures, chlorine is disengaged, and there are found at the bottom of the vessel some drops of an oily fluid, which are heavier than water and have an æthereal smell; these are undoubtedly a chloride of carbon, the nature of which I have not directly determined.

Chlorous acid and ammonia give rise by their mutual action to very different phænomena, according to the circumstances under which the action occurs.

If very dilute ammonia be added to chlorous acid, also very dilute, a disengagement of bubbles of azote is perceived; it is however not copious, and much less than it would be if the whole of the substances mixed were decomposed. The liquid which is thus obtained, rendered alkaline by a sufficient addition of ammonia, still possesses the property of decolorizing the solution of sulphate of indigo. But the disengagement of bubbles of gas continues to take place; the alkalinity gradually disappears, and on the contrary the liquor becomes acid, and in this state it no longer decolorizes indigo. These facts render the existence of a chlorite of ammonia very probable, which is an ephemeral chlorite, already described by M. Soubeiran.

If a mixture of a more concentrated ammoniacal solution and chlorous acid be made with the greatest precaution, and so as to keep down the slight heat which is developed during their re-action, a white cloud is produced, which renders the liquor opake for some time.

This cloud is sometimes deposited in oily drops, which have all the properties of chloride of azote; it is however more frequently carried off in the state of vapour by the gas which continues to be disengaged.

This chloride of azote is very easily produced, especially when there is suspended in dilute chlorous acid a fragment of an ammoniacal salt, as for example of sulphate, phosphate, &c. The decomposition then goes on very slowly, but a small quantity of gas is absorbed, which is in this case a mixture of chlorine and azote, and not of pure azote, and there is formed at the same time a notable quantity of chloride of azote. This process appears to me the most convenient that can be employed for the preparation of this substance. A very small quantity of it however is obtained, if, instead of placing the fragment of salt in chlorous acid, it is mixed with the acid after being dissolved in water. The action then occurring instantaneously and rapidly on the whole mass, and no chloride of azote is obtained; or at any rate, if this compound is formed, it is carried off by the brisk effervescence which occurs in this case.

It is almost useless to say, that no chloride of azote is obtained when concentrated chlorous acid is poured into concentrated ammonia. The action is then very rapid; it is accompanied with so great a disengagement of chlorine and so abundant an emission of gas, that it takes place with a sort of detonation.

This action is still more vivid when bubbles of ammoniacal gas are added to a few drops of chlorous acid in a receiver over mercury. There is not only a great disengagement of heat, but an emission of yellow light, and the upper part of the receiver contains a mixture of azote and chlorine gas.

It will be readily perceived that in all these circumstances it is principally by the oxygen of chlorous acid that the decomposition of the ammonia is effected, and that the chlorine and azote, simultaneously set free, sometimes coalesce to form chloride of azote, and are at others disengaged in a state of aëriform existence, according to circumstances.

The two elements of the compound combustible are, on the contrary, completely burnt, in great measure by the oxygen when chlorous acid is made to act upon gaseous hydrogenated combinations of phosphorus, arsenic, and of sulphur. Phosphuretted hydrogen, introduced into a small receiver containing a few drops of chlorous acid, burns on coming into contact with this liquid. There are produced phosphoric acid and chloride of phosphorus, which are ultimately decomposed into hydrochloric and phosphoric acid. No uncombined phosphorus is observed, but pure chlorine is collected in the upper part of the bottle.

The same phænomena occur with arseniuretted hydrogen. The combustion takes place with a flame of a blue tint. In the place of chlorous acid are found arsenic and hydrochloric acids, and the upper part of the receivers is filled with gaseous chlorine. In this case, as when phosphuretted hydrogen is operated with, the disengagement of chlorine can only be procured on adding the gas bubble by bubble, and so as never to be in excess; for then it would burn at the expense of the chlorine itself.

Hydrosulphuric acid acts in the same manner, but with this difference, that its action on the chlorous acid is not accompanied with emission of light, although the heat developed is very strong. There are formed water and sulphuric acid, and chlorine is disengaged, which afterwards exerts its usual action on the fresh bubbles as they arise.

The phænomena are nearly the same when a current of these gases is passed into chlorous acid, nor does the nature of the products at all vary; water, and phosphoric, arsenic, and sulphuric acids are always formed. The greater part of the chlorine is disengaged in the gaseous state, producing a brisk effervescence, whilst a portion remains liquid in the state of hydrochloric acid.

Chlorous acid acts upon the liquid or gaseous hydracids nearly in a similar manner. In operating with hydriodic acid gas I obtained water, iodic acid, and a disengagement of chlorine gas. In this case a violet tint is, though scarcely, perceptible, which evidently arises from the action of chlorine set free upon some bubbles of hydriodic acid which have escaped the chlorous acid. Much heat, but no light, is given out in this experiment.

A disengagement of heat only, occurs when chlorous acid is made to act upon these hydracids dissolved in water; the decomposition is instantaneous. With hydrochloric acid, water and a disengagement of chlorine is produced; with hydrobromic acid, there are obtained bromic acid, bromine, chloride of bromine and an abundant disengagement of chlorine. Hydriodic acid gives rise to similar phænomena.

Anhydrous hydrocyanic acid and chlorous acid also exert a remarkable action on each other. Chlorine is produced in abundance, and the liquid, besides hydrochloric acid and the cyanic acid of Sérullas, contains a certain quantity of chloride of cyanogen.

The metallic sulphurets, treated with liquid chlorous acid are immediately converted into sulphates. Heat is produced, and chlorine is disengaged: sometimes also I have perceived the odour of chloride of sulphur. I obtained analogous results, in causing chlorous acid to act upon phosphuret of lime [calcium?].

The action of compound combustibles on chlorous acid fully confirms then what its manner of acting upon simple combustibles had before indicated, and shows it to be one of the most marked agents of oxidation, and at the same time but little calculated to act by the chlorine which it contains. Nevertheless, when the action proceeds slowly, its two elements combine with the two elements of the compound combustible; and the oxygen, seizing the most electro-positive of the two, leaves to the chlorine the most electro-negative. It is in this way, for example, that it takes place in the action of chlorous acid upon bicarburetted hydrogen and ammonia. In other cases, the chlorine combines well with one portion of the electro-negative element, but it disengages a portion which is larger according as the temperature is higher. When it reaches incandescence, the two elements of the compound combustible are almost totally burnt by the oxygen, as if the affinity of oxygen for bodies increased with the temperature in a higher ratio than than of chlorine for them.

It may nevertheless be supposed that the composition of chlorous acid is such, that the electro-negative element of the compound combustible being saturated with chlorine, an excess of this gas may remain; but this is not the case. On comparing its composition, which I shall hereafter state, with that of phosphuretted hydrogen, for example, even admitting that all the hydrogen is burnt by the oxygen, the chlorine is insufficient to combine with the phosphorus. Yet in this decomposition a large proportion of this gas is set free, which appears to me to render it very probable that in this case, as in the greater part of others, it is with the oxygen of chlorous acid that the elements of the compound combustible are both combined.

It is easy to foresee, according to this, how chlorous acid must act with the combinations of oxygen which are not saturated with this principle. These combinations are almost always carried to the highest degree of oxigenation, and the chlorine set free is disengaged in the gaseous form.

There are some, however, which do not appear to be altered by chlorous acid, as the oxide of carbon: but, on the contrary, it exerts most energetic action on oxalic acid; a fragment of this acid, when thrown into moderately concentrated chlorous acid, occasions an intense emission of light, and a very strong effervescence produced by the disengagement of a mixture of carbonic acid and chlorine, as might be expected.

Nor does the first degree of oxidation of azote appear susceptible of being acted upon by chlorous acid; but all the other oxigenated combinations of this gas, the deutoxide of azote, nitrous vapours, hyponitrous acid, fuming nitric acid impregnated either with hyponitrous acid or deutoxide of azote, act very energetically upon chlorous acid. Colourless nitric acid and chlorine gas are produced.

In the series of oxigenated compounds of sulphur, the hyposulphuric acid exhibits the same anomaly as that which characterizes the manner in which oxide of carbon and protoxide of azote act with chlorous acid. It is not altered by this oxigenating agent; whereas sulphurous acid, whether gaseous or liquid, is immediately converted by it into sulphuric acid, with the disengagement of chlorine. This peculiarity may, perhaps, furnish those chemists with an additional reason, who think that hyposulphuric acid is not a primary compound of sulphur and oxygen, but a secondary combination of sulphuric acid and sulphurous acid.

The compounds of oxygen and phosphorus do not present anything similar. In fact, oxide of phosphorus, hypophosphorous acid, phosphorous acid, and even phosphatic acid, are all immediately converted into phosphoric acid, with the disengagement of heat and of chlorine gas.

The same is also the case with arsenious acid, which is converted into arsenic acid, and with selenious acid, which is changed into selenic acid, giving rise to the production of the same phænomena, to an evolution of chlorine, and a considerable disengagement of heat.

The metallic oxides act variously with liquid chlorous acid. It will be imagined that so energetic an agent of oxidation would convert the greater part of these compounds susceptible of it into peroxides, but that it would not exert any action on those which are already saturated with oxygen. This is what actually happens. The deutoxide of tin, the peroxide of iron, &c. suffer no change by chlorous acid; whereas the protoxide of iron, tin, manganese, nickel, cobalt and lead are converted into peroxides with the evolution of chlorine. The protoxide of chromium is immediately converted into chromic acid. There are, however, some oxides susceptible of a higher degree of oxidation, such as the oxide of bismuth, and the peroxide of manganese, upon which chlorous acid does not appear to act.

Although the alkaline oxides can, under some circumstances, combine with a larger proportion of oxygen, chlorous acid, instead of superoxidizing them, merely combines with them. Still further, it decomposes the peroxides of these metals, and disengaging oxygen it reduces them to the state of protoxides, with which it forms decolorizing chlorites. It is at any rate in this way that I have observed it to act with the peroxide of barium.

With the peroxide of lead and the two oxides of silver, which are metals with which chlorine forms insoluble compounds, it acts in an entirely different manner. These oxides are decomposed with the production of chlorides, and not chlorites, and with a disengagement of oxygen mixed with a little chlorine. This happens from the re-action of chlorous acid upon the chloride formed.

The metallic chlorides are decomposed by chlorous acid. This decomposition is always accompanied with an abundant disengagement of chlorine, and the metal is oxidated. As to the nature of the definitive product, it evidently depends upon the manner in which this oxide acts either with chlorine or with chlorous acid. Thus the chlorides of the alkaline metals form mixtures of chlorides and chlorites. Those of manganese, iron, nickel, cobalt, lead, and tin, occasion a disengagement of chlorine and form peroxides. The chloride of copper forms both chloride and oxichloride of copper. The protochloride of mercury changes, without evolving gas, into a red powder, which is unquestionably an oxichloride. The perchloride of mercury and the chloride of silver are also attacked, but very slowly, by concentrated chlorous acid: the gas disengaged is chlorine, mixed with a small quantity of oxygen. I do not at present know how to explain the production of the last-mentioned gas.

The bromides undergo a slightly different action. I have observed that with those of potassium, mercury, and silver, there is a disengagement of chlorine, bromine, and chloride of bromine, and the formation of a bromate and a metallic chloride.

The iodides of potassium, mercury, and silver appeared to produce analogous phænomena.

Saline compounds suffer two kinds of action by chlorous acid: it may decompose them by evolving their acid: it may, on the contrary, be itself decomposed, and by superoxidizing their acid or base, thus convert them into new salts.

There are but few acids which can be expelled from their saline combinations by chlorous acid, but it expels with effervescence the acid from the carbonate of soda and of lime, and forms a chlorite with the base. When an acetate is treated with chlorous acid, especially if heated, the odour of acetic acid is perceived, chlorine mixed with a little oxygen is disengaged, and after a certain time chlorate of potash is formed, which agrees perfectly with the observation of M. Liebig.

Bromic acid even is expelled from its combinations by chlorous acid. The same phænomena occur as with the acetates,—disengagement of chlorine mixed with a little oxygen, formation of a chlorate, and evolution of a part of the bromic acid.

As to the action which chlorous acid exerts on the salts as an oxidizing agent, it may be stated in a few words. With respect to their acids, it acts as if they were free; that is to say, not at all upon those salts the acids of which are saturated with oxygen, and it converts those to this condition which were not so previously. Thus the oxalates are converted into carbonates, the sulphites into sulphates, &c. All these re-actions occur with the disengagement of chlorine, and frequently with heat, without altering the neutrality of the salt; the iodates and chlorates are not however converted into hyperiodates and hyperchlorates; the same anomaly occurs here which I have noticed when speaking of the action of chlorous acid upon these free acids. The hyposulphate of barytes, which nitric acid converts into sulphate, is not acted upon by chlorous acid, notwithstanding the insolubility of the product which would be formed by the oxidation of its acid.

Chlorous acid acts in the same manner on salts with respect to their bases; that is to say, the protoxides are immediately converted into peroxides, provided always that they can neutralize the acid. Thus the salts of protoxide of iron, of copper, and of tin, are immediately converted into salts of the peroxides: but as to those of nickel, cobalt, and lead, they suffer no alteration. If the base were superoxidized, it would cease, in this case, to be proper to saturate the acid, and the chlorous acid would then act so as to destroy a combination already existing, instead of contributing to form more neutral and stable compounds, as when it changes the salts in *ite* to salts in *ate*. Nevertheless the salts of protoxide of manganese, treated with chlorous acid, deposit peroxide, and the solution becomes acid; but the action is so slow, that I think it may be attributed to the disengagement of chlorine, which always accompanies the spontaneous decomposition of chlorous acid, and not to this acid itself.

It was natural to suppose that organic substances would be essentially altered in their constitution by chlorous acid, which we have seen act energetically on several inorganic compounds; this opinion was confirmed by experience. I put a great number of vegetable and animal substances into contact with this acid, and in almost every case I perceived indications of a re-action, which was frequently very vivid. It forms no part of the plan which I have proposed, to describe in detail the modifications which each organic substance underwent in this case. It will be sufficient to say in a general manner, that in the greater number of them the re-actions were accompanied with a disengagement of chlorine mixed with variable proportions of carbonic acid gas. When the substance contains azote, it is disengaged, but the odour of the gas indicates that chloride of azote is also formed. This is especially observed with urea, lithic acid, and the vegetable alkalis which did not appear to me susceptible of forming chlorites.

In some cases the quantity of carbonic acid obtained represented the oxygen which enters into the composition of chlorous acid. I thought that I perceived this in operating on indigo, a colouring matter which chlorous acid instantly changes into a yellow substance, which is soluble in alcohol and rather bitter.

In the greater number of cases but very little carbonic acid is obtained; a notable portion of oxygen disappears at the same time, and contributes to form new and more oxigenated compounds. Thus the products of its action on sugar, gum, starch, &c. are strongly acid.

Sometimes, however, the decomposition is slow, and the two elements of the chlorous acid are both absorbed by the organic matter. This is observed with alcohol; this liquid, by mixture with chlorous acid, is converted into acetic acid, and there is obtained at the same time a certain quantity of an oily liquid, produced by the action of the chlorine upon the alcohol.

It appears to me that these observations are sufficient to prove that chlorous acid acts upon organic bodies principally on account of the oxygen which it contains.

Could not an oxidizing agent, very superior to nitric acid, give rise to some new compounds in acting upon several organic compounds? It is natural to think so, and to suppose that a knowledge of chlorous acid may thus, in an indirect manner, contribute to the progress of organic chemistry, by exciting fresh researches, which I propose hereafter to undertake, as soon as I have hopes of doing so with success.

§ 4. *Chlorous Acid Gas.*

I had repeatedly observed that the aqueous solution of chlorous acid, when exposed to the air, soon lost its tint, and a great part of its odour. This change of properties, which I satisfied myself was not derived either from the absorption of the oxygen or the moisture of the air, made me think that chlorous acid was volatile, and that I should be able to obtain it in the gaseous state, and it was towards this end that I directed my researches.

I first tried the action of heat on concentrated liquid chlorous acid. At a temperature much below ebullition, I observed in fact that it disengaged a very small quantity of a yellow-coloured gas, which, passing through the mercury in small bubbles, was absorbed by it, leaving occasionally a residue of oxygen. As to the liquid, even after being some time exposed to a temperature near ebullition, it retained the property of acting on combustible bodies with the same activity as at first. I presumed from this that chlorous acid had great affinity for water, and that by the action upon liquid chlorous acid of a substance having great affinity for this water, I should obtain the acid in the state of gas.

I first attempted this with sulphuric acid, and I succeeded in obtaining a gaseous body. But this gas was of a very deep yellow colour, and its smell, instead of resembling that of liquid chlorous acid, was more like deutoxide of chlorine. The water with which I attempted to act upon it dissolved a certain quantity, and left as a residue a mixture of much chlorine and very little oxygen: the solution did not resemble liquid chlorous acid; it was of a very deep yellow, and possessed the properties of a solution of deutoxide. The sulphuric acid, therefore, while taking the water from the chlorous acid, had converted it into oxygen, chlorine, and deutoxide.

I suspected from this, that chlorous acid, like the nitric, chloric, bromic, and other acids, could not exist without water; but before I proceeded on this idea, I thought I would try other bodies, which, although greedy of water, like sulphuric acid, would not excite any such marked action. It being impossible to employ chloride of calcium, I had recourse to nitrate of lime, a very deliquescent calcareous salt, the action of which changed my idea on the subject.

When a mixture is made of nearly equal volumes of concentrated liquid chlorous acid, and very dry solid nitrate of lime, a brisk effervescence ensues, and a gas is produced, which re-dissolved in water gives a product possessed of all the properties of liquid chlorous acid, and which ought consequently to be considered as pure chlorous acid gas. The same results are obtained by using vitreous phosphoric acid* instead of nitrate of lime.

If an attempt be made to collect chlorous acid gas in the mercurial trough in the usual way, the metal is attacked, and oxygen alone remains; sometimes indeed the absorption is complete. It is then necessary, in order to obtain it, to operate in a peculiar manner. The following method has always succeeded.

After having introduced into a receiver filled with mercury about $\frac{1}{50}$th of its volume of concentrated chlorous acid, I gradually passed up fragments of dry nitrate of lime. The gas is disengaged with effervescence, and, as it does not touch the mercury, being separated from it by the solution of nitrate of lime, it may be kept in the trough for a long time. Afterwards it may be transferred from one receiver to another, provided this operation be effected rapidly; for it is but slightly decomposed by the metal, when it passes through it in large bubbles.

Chlorous acid gas is of a yellow colour, and but little deeper than that of chlorine, with which in the course of my researches I had long confounded it, on account of this similarity of tint. Its smell is extremely pungent, and like that of the liquid acid; it is completely absorbable by mercury, which it converts into red oxichloride.

Water dissolves many times its volume. I have not determined this solubility very exactly, but I believe it to be more than 100. The solution is very slightly coloured, and has the properties of liquid chlorous acid. The solution of chlorous acid in water takes place very rapidly, but there always remains, after the operation is over, a very small residue of chlorine and oxygen, which indicates a very slight decomposition of this gas during its preparation. A small increase of temperature separates its elements with explosion, and a very considerable disengagement of heat and light. Although it appears to me more difficultly decomposable by increase of temperature than the oxides of chlorine, it has happened to me that it has exploded during transfer. On this account, care should be taken to add the nitrate of

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* Care must be taken to use phosphoric acid which is not prepared by the decomposition of phosphate of ammonia by heat. As this often retains a little ammonia, it produces chloride of azote, which, by the slightest increase of temperature, gives rise to detonations, which are the more dangerous as not being foreseen.

lime gradually during its preparation, in order that the heat which is generated during the solution of this salt should not be too strong. I have frequently seen chlorous acid gas detonate by the influence of this cause; and although this detonation is not in itself very dangerous,—for the receiver, which is projected vertically, is never broken into small fragments,—yet the chlorous acid being then dispersed in small drops, which are extremely corrosive, it is prudent to prevent it.

An exposure of some hours to a weak diffused light did not appear to me susceptible of altering chlorous acid; but solar light decomposes it in a few minutes without detonation.

The manner in which different bodies act upon the gaseous acid is similar to that upon liquid chlorous acid.

Oxygen and chlorine cannot act upon chlorous acid: hydrogen, at common temperatures, exerts no action upon it; but if a lighted taper be put to a mixture of these two gases, a loud detonation is produced, and white thick vapours of hydrochloric acid gas are observable.

I have not tried what effect boron and silicium produce upon this gas, but I have stated what bromine and iodine can do. If a small quantity of these substances be added to a proper volume of chlorous acid gas, it is quickly absorbed, and chloric and bromic acids are produced, and chlorides of bromine and iodine.

This action of bromine and iodine takes place slowly and without detonation. This is not the case when sulphur, selenium, phosphorus, and arsenic are operated with. Scarcely do these bodies come into contact with the chlorous acid gas when they decompose it with a strong detonation and a vivid light. The arsenic and phosphorus are converted into arsenic and phosphoric acids; the sulphur forms sulphurous acid. I have not determined whether selenious or selenic acid is produced by selenium. One part of the chlorine combines with the combustible bodies, but a notable portion is disengaged in the gaseous state. This gaseous chlorine is moreover always mixed with a quantity of oxygen. When charcoal is used, there is also immediate detonation; but the gas obtained is a mixture of oxygen and chlorine, and contains but very little carbonic acid. I believe that, in this case, the decomposition is effected less by the affinity of the carbon for oxygen, than by the heat which is developed by the absorption of the gas in the pores of the charcoal.

The metals act differently with chlorous acid gas according to the circumstances under which they are brought into contact with it. If fragments of several of the metals, wrapped in sized paper to prevent amalgamation, be passed into a rather narrow jar containing a small quantity of chlorous acid, it is absorbed completely in a few moments, and without detonation. There are formed both an oxide and a chloride. But if the quantity of chlorous acid employed measures several cubic inches, the absorption, which begins to take place very slowly, terminates with a detonation accompanied with disengagement of light, and there is then found in the upper part of the receiver a mixture of chlorine and oxygen. It is probable that the heat developed in this case by chemical action occasions the decomposition of the portion of gas which had not been absorbed.

Silver leaf also acts upon chlorous acid after a certain time. The metal is partly converted into chloride, and oxygen is disengaged; but the heat developed by this re-action also occasions the decomposition of a part of the gas, and chlorine is found mixed with oxygen.

Chlorous acid gas is also decomposed by the greater part of the compound combustibles. Cyanogen and chlorous acid gas act but slowly upon each other; however, at the end of a certain time, chlorine is found in the receiver, carbonic acid and azote, and the gaseous mixture gives out the odour of chloride of cyanogen.

Common carburetted hydrogen does not act upon chlorous acid; but this acid and bicarburetted hydrogen decompose each other, without evolving heat and light, water and a chloride of carbon being produced.

Phosphuretted and arseniuretted hydrogen gases, on the contrary, occasion detonation, and the gaseous residue found is chlorine mixed with a little oxygen. The combination of sulphuretted hydrogen is accompanied with a blue flame, similar to that afforded by sulphur when it is burnt in contact with the air. The detonation produced by ammonia is also very vivid, and, in this case like the former ones, much chlorine is set free.

Sulphuret of carbon also produced a vivid explosion, after which there was found in the receiver in which it occurred, chlorine, sulphurous and carbonic acid. The odour of these gases indicates that there is formed at the same time a small quantity of chloride of sulphur.

The decomposition of chlorous acid by hydrochloric acid is accompanied with the disengagement of heat only, without producing light. It is the same when hydriodic acid is operated with.

Phosphuret of lime [calcium?] instantly occasions the decomposition of chlorous acid with a loud detonation, and the gaseous residue contains much chlorine.

The greater number of sulphurets—those of barium, tin, mercury, antimony, &c.—produce in a few seconds the same effect. When there is but little gas it may be absorbed without the occurrence of detonation. In the latter case, the odour indicates the formation of chloride of sulphur.

Oxalic acid also decomposes chlorous acid without producing explosion. Oxide of carbon, upon which the liquid acid did not appear to exert any action, decomposes chlorous acid gas but slowly. After the lapse of some hours the colour of the mixture disappears, and the gas has the penetrating odour of chlorocarbonic acid. Nitrous oxide gas does not appear to undergo any alteration by chlorous acid; but with nitric oxide gas this acid occasions a violent detonation. This is accompanied with the production of nitrous gas, which fresh bubbles of chlorous acid are susceptible of converting into nitric acid.

Sulphurous acid gas, when dry, is but slowly attacked by chlorous acid; yet, after some hours, the gaseous mixture placed over mercury disappears, and the sulphurous acid is converted into sulphuric acid.

Filtering paper and indigo are the only organic matters which I have put in contact with chlorous acid gas. The first occasioned detonation, but very little carbonic acid was produced. The receiver, after the explosion, was found full of oxygen and chlorine, nearly in the proportions which constitute chlorous acid; this indicates that it was chiefly by the heat disengaged in its action on the paper that the gas detonated.

Indigo also decomposes chlorous acid without detonation, and converts it into a compound of a yellow colour. The volume of carbonic acid produced is much smaller than that of the oxygen contained in the acid employed; and the chlorine evolved is in part absorbed by the mercury, and partly retained in the pores of the vegetable matter, which yields, undoubtedly from this cause, acid vapours when heated.

The facts which I have now detailed prove, as it appears to me, that chlorous acid in the state of gas acts nearly in the same way as when it is liquid. If its two elements are absorbed together when the temperature is but little raised, it is on the other hand principally owing to the oxygen gas which it contains that it acts whenever much heat is emitted. The disengagement of chlorine gas which accompanies the greater number of its re-actions appears to me to be a sufficient proof of the truth of this assertion.

It may be thought at first that the chlorine comes from the decomposition of a portion of the gas, occasioned by the high temperature which the action of a combustible body most commonly develops; but only one portion of the chlorine can be attributed to such an origin. In fact, the quantity of oxygen with which it is mixed, is almost always smaller than that which in chlorous acid is united with the volume of chlorine obtained; which induces the opinion, that in these cases the two combustible elements of the compound are principally combined with oxygen.

§ 5. *Composition of Chlorous Acid.*

The experiments which I have now related are sufficient to determine that the new compound, which I have described under the name of chlorous acid, is uniformly composed of chlorine and oxygen; but they do not determine the proportions in which these two elements are united.

It was necessary therefore to attempt some more exact experiments, in order to proceed to its exact analysis.

I first made them upon chlorous acid in the state of aqueous solution. When I had afterwards succeeded in extracting pure chlorous acid gas, I verified by direct analysis the results which I had obtained with the aqueous acid.

Several methods suggest themselves for analysing chlorous acid diluted with water. It may, in fact, be decomposed by a combustible, which sets the chlorine free, and then appreciating together the proportions of this gas and those of the oxigenated compound which is produced at the same time with it. On the contrary, it may be treated with metallic silver, and the quantities of oxygen and of chloride of silver formed may be ascertained; but in both these methods only one of the elements of the chlorous acid is obtained in the state of gas: it is requisite to determine the volume of the other by weighings and calculation, which render these analytic processes rather long in executing. I therefore endeavoured to find another, which would allow of my attaching the composition of chlorous acid to the nature of some well-known combination, and to reduce its analysis to that of a gaseous mixture, a kind of operation which unites the double advantage of accuracy and brevity. The action which chlorous acid exerts on oxalic acid, and in which these two bodies are converted into chlorine and carbonic acid, appeared to offer an easy method. It is well known that oxalic acid yields, by decomposition, equal volumes of carbonic acid and oxide of carbon, and that the latter requires half its volume of oxygen to convert it into a volume of carbonic acid equal to its own. It results from this, that when oxalic acid is changed into carbonic acid, the quarter of the volume of this gas obtained represents that of the additional oxygen necessary for this alteration.

The analysis of chlorous acid is brought by this method to that of a mixture of chlorine and carbonic acid, which is easy of execution by means of mercury.

I made in this way various attempts, which, although they indicated that the volume of chlorine was almost double that of the oxygen, differed however too much from this result, and did not besides agree sufficiently together to inspire me with confidence. I persevered nevertheless, for the method is easy and it appeared to me to be certain. Convinced, however, at last that the results obtained were not attributable to any fault in the execution, I examined by nitrate of silver the residue of a re-action of this kind in which the oxalic acid was in excess, and the quantity of chloride of silver insoluble in nitric acid which I obtained proved that a notable portion of chlorine remained in the liquid. I have since convinced myself that the quantity which is not disengaged in a gaseous form is greater as the chlorous acid is more dilute. I am ignorant of the state in which this chlorine exists: perhaps it forms a little of the chloroxalic acid, which M. Dumas obtained by exposing acetic acid and chlorine to the influence of the solar rays; perhaps also it existed in the state of hydrochloric acid, a state to which it might be reduced by the decomposition of water, the oxygen of which would contribute to the formation of the carbonic acid.

Inexact as this method of analysis is, it renders it very probable that chlorous acid is formed of two volumes of chlorine and one volume of oxygen. But the manner of its action with hydrochloric acid leaves no doubt on this subject.

Chlorous acid and hydrochloric acid produce, by their double decomposition, water and chlorine, as already mentioned. Then if an excess of chlorous acid be made to act upon a given quantity of hydrochloric acid, the relation between the volumes of the acid gas decomposed and of the chlorine obtained, will allow the composition of the chlorous acid to be inferred from that of the hydrochloric acid.

I attempted to effect this decomposition, both by passing hydrochloric acid gas over mercury into a graduated tube containing in its upper part a little very concentrated chlorous acid, and by introducing chlorous acid into a tube already containing hydrochloric acid gas. But the decomposition was in both cases produced in a very imperfect manner. The chlorine gas which is disengaged at first renders the solution of the hydrochloric acid gas difficult. In order that it may be complete, recourse must be had to agitation, which cannot be effected without a portion of the chlorine being absorbed by the mercury.

The following mode of operating, however, succeeded perfectly: after having filled a stopped bottle with hydrochloric acid gas in the mercurial apparatus, I introduced a small phial of glass filled with chlorous acid and hermetically sealed; I stopped the bottle, and I shook it so as to break the small phial. As soon as the chlorous and hydrochloric acid came into contact, decomposition took place with disengagement of heat, and the interior of the bottle assumed a yellow tint. When this had returned to the temperature of the atmosphere I was able to open the phial over the mercury without a drop of this liquid metal entering or a bubble of gas escaping. The gas which thus filled it was entirely absorbed by mercury.

In its decomposition by chlorous acid, the hydrochloric acid had been converted into an equal volume of chlorine. Then in this volume of hydrochloric acid there was half a volume of hydrogen: the chlorous acid which had converted this hydrogen into water had yielded a quarter of a volume of oxygen. On the other hand, the hydrochloric acid decomposed could give only half a volume of chlorine; and, as a whole volume had been produced, the other half volume must have been furnished by the chlorous acid. The latter was therefore manifestly composed of two volumes of chlorine and one volume of oxygen.

I was however apprehensive that one circumstance might contribute to render this mode of analysis imperfect, which however appeared to me as simple as it is elegant. It was, in fact, possible that the heat generated might disengage a portion of the chlorous acid in the gaseous state, which, as I have already stated, is completely absorbable by mercury. The gas then obtained would have contained something besides chlorine, and the volume of this gas elicited would not then have been rigorously equal to that of the hydrochloric acid employed.

When also I had afterwards observed that concentrated sulphuric acid, in acting upon liquid chlorous acid, disengaged from it, if not pure chlorous acid, at least the gaseous products of its decomposition, I determined to ascertain in what proportion they contained chlorine and oxygen.

For this purpose, I submitted 50 volumes of this gas to the action of heat, in order to effect its detonation. I thus obtained 72 volumes, which treated with an alkaline solution were reduced to 25 volumes of oxygen gas. If it be considered that in this mode of experimenting a small portion of chlorine is necessarily absorbed by the mercury, the slight loss sustained will be readily explained; and, as it appears to me, it will be concluded that this experiment proves, as well as the former ones, that chlorous acid is composed of two volumes of chlorine and one volume of oxygen.

When the methods which I have described allowed of my procuring pure chlorous acid, I confirmed the previously obtained results by direct analysis. By the detonation of 45 volumes of this gas I procured 69 volumes of a gaseous mixture, which was reduced to 23 volumes when I agitated it with an alkaline solution. This last experiment, not only justifies the results with which other methods had already furnished me, but allows of appreciating the contraction which the chlorine and oxygen undergo in combining to form chlorous acid. It will be observed, in fact, that this contraction is one third of the whole volume, and equal to that of the oxygen which enters into its composition. The number 67·5, the product of 45 by 1·5, differs too little, it appears to me, from the number 69 which I obtained, to allow of any doubt remaining in this respect.

The analysis of chlorous acid thus shows that it is formed of the same elements and in the same proportions as the gas obtained from chlorate of potash and hydrochloric acid, a gas which chemists have long considered as protoxide of chlorine. If it were satisfactorily demonstrated that this product is really a distinct compound, as it differs much from the chlorous acid which I have been describing, these two bodies would afford a fresh example of isomerism. But the recent labours of M. Soubeiran have rendered it extremely probable that this supposed protoxide is merely a mixture of chlorine and deutoxide of chlorine, which had long been suspected by chemists on account of the peculiar condensation of its elements.

This composition of chlorous acid differs, it will be observed, very much from that which had previously been assigned to it by chemists. On account of the impossibility of directly analysing chlorous acid, chemists have endeavoured to determine its composition either according to the re-actions which are produced during its formation or when the decolorizing chlorides are in contact with certain compounds, or from theoretical considerations; but it is easy to prove that these observations, the accuracy of which on account of the ability of the chemists who made them is unquestionable, agree perfectly with the results which I have obtained, and that my views are most consistent with theory.

M. Liebig, by causing the decolorizing compound of chlorine to act upon the sulphurets of barium, lead, &c., observed that they were immediately converted into sulphates, without evolving chlorine or precipitating sulphur. Now in order to convert 1 atom of these sulphurets into sulphate, 4 atoms of oxygen are requisite, 3 to form the acid and 1 to form the base. M. Liebig has supposed that this effect was produced by 1 atom of chlorite; and, as the base of this chlorite could only yield 1 atom, he has admitted that the other 3 atoms were furnished by the chlorous acid. On the other hand, the atom of metal of the base is found in the liquor in the state of chloride; there were required therefore 2 atoms of chlorine to form this compound.

Chlorous acid, according to this, seems to be composed of 2 of chlorine and 3 of oxygen. But if it be supposed that 2 atoms of this acid are necessary to convert 1 atom of sulphuret into sulphate, the observations of M. Liebig will then entirely agree with mine. Of the 4 atoms of oxygen requisite, 2 will be furnished by the 2 atoms of acid, and the 2 others by the 2 atoms of base; and the 4 atoms of chlorine combining with 2 atoms of metal will form 2 atoms of chloride.

M. Soubeiran arrived at the same conclusions as M. Liebig from the following considerations: if chlorous acid, he says, is formed as chemists suppose, it is necessary for its production that 3 atoms of metallic oxide should be decomposed to furnish the 3 atoms of oxygen which enter into its composition, and there should be produced 3 atoms of metallic chloride; so that the decolorizing compounds of chlorine must contain 3 atoms of chlorine in every atom of chlorite.

In order to verify this supposition, M. Soubeiran converts a solution containing 4 atoms of soda into decolorizing chloride. He evaporated this in vacuo, and treated the residue of the evaporation with a saturated solution of common salt, intended to dissolve the chlorite, leaving the chloride unacted upon. He found this quantity of chloride of sodium equivalent to 2·1 atoms of soda; and, with a little too much allowance, as it seems to me, he concluded that these 2·1 atoms were equal to 3, and thence that chlorous acid is very probably formed of 2 of chlorine and 3 of oxygen. If it be considered that in such a mode of experimenting, the quantity of metallic chloride ought rather to be greater than smaller than that at first formed by the re-action of the chlorine upon the soda, no doubt can be entertained that these 2·1 should be reckoned only as 2, and this establishes a perfect agreement between my results and those of M. Soubeiran, and gives for the composition of chlorous acid the numbers which I have already adopted.

M. Morin, in his work on the decolorizing chlorides, has decidedly proved that during their decomposition, whether spontaneous or effected by heat, these combinations are converted into 17 atoms of chloride for 1 atom of chlorate, and that 12 atoms of oxygen are at the same time disengaged, two thirds of what they previously contained. In supposing that chlorous acid is formed of 2 of chlorine and 3 of oxygen, the following table is the expression of the atomic re-action.

Atoms employed.

$$\text{9 atoms chlorite} = \begin{cases} \text{9 atoms oxide ...} = \begin{cases} \text{9 atoms metal.} \\ \text{9 atoms oxygen.} \end{cases} \\ \text{9 atoms acid ....} = \begin{cases} \text{18 atoms chlorine.} \\ \text{27 atoms oxygen.} \end{cases} \end{cases}$$

Atoms produced.

$$\text{24 atoms oxygen.}$$

$$\text{2 atoms chlorate} = \begin{cases} \text{2 atoms oxide ...} = \begin{cases} \text{2 atoms metal.} \\ \text{2 atoms oxygen.} \end{cases} \\ \text{2 atoms acid ....} = \begin{cases} \text{4 atoms chlorine.} \\ \text{10 atoms oxygen.} \end{cases} \end{cases}$$

$$\text{7 atoms chloride} = \begin{cases} \text{7 atoms metal.} \\ \text{14 atoms chlorine.} \end{cases}$$

If to these 7 atoms of chloride there be added the 27 which were mixed with the 9 atoms of chlorite in the decolorizing compound, there would be 34 atoms of chloride for 2 of chlorate, 17 for 1.

But the supposition that chlorous acid is formed of 2 atoms of chlorine and 2 of oxygen, agrees also with these results; the atomic re-action may then be much more simply expressed, as shown by the following table.

Atoms employed.

$$\text{9 atoms chlorite} = \begin{cases} \text{9 atoms acid ....} = \begin{cases} \text{18 atoms chlorine.} \\ \text{9 atoms oxygen.} \end{cases} \\ \text{9 atoms base ....} = \begin{cases} \text{9 atoms metal.} \\ \text{9 atoms oxygen.} \end{cases} \end{cases}$$

Atoms produced.

$$\begin{array}{l} \qquad\qquad\qquad\qquad\qquad\qquad\qquad\qquad\quad \text{12 atoms oxygen.} \\ \text{1 atom chlorate} = \begin{cases} \text{1 atom acid} \ldots\ldots = \begin{cases} \text{2 atoms chlorine.} \\ \text{5 atoms oxygen.} \end{cases} \\ \text{1 atom base} \ldots\ldots = \begin{cases} \text{1 atom metal.} \\ \text{1 atom oxygen.} \end{cases} \end{cases} \\ \qquad\qquad\qquad \text{8 atoms chloride} = \begin{cases} \text{8 atoms metal.} \\ \text{16 atoms chlorine.} \end{cases} \end{array}$$

The 8 atoms of chloride formed, and the nine with which they were mixed,—admitting that chlorous acid is composed as I state,—make the 17 atoms for 1 of chlorate obtained by M. Morin.

There is only one fact which does not agree with the new composition which I assign to chlorous acid; it is an experiment of Berzelius, in which this chemist observed, when precipitating chloride of lime by nitrate of silver, that the metallic chlorite while decomposing produced an atom of chloride, while 2 atoms of silver remained in solution, unquestionably in the state of chlorate. But according to my view of the constitution of chlorous acid there ought on the contrary to be formed 2 atoms of chloride for 1 of chlorate. But it must be stated that Berzelius does not attach any such importance to this result, so as to deduce the composition of chlorous acid from it; for this mode of experimenting cannot be exact. I have already shown, that during the precipitation of the chloride by the salt of silver and the filtration of the liquid, the chlorite of silver decomposes; and of the two products of this decomposition, the chlorate only is obtained, which remains in solution, while the corresponding chloride is retained by the filter: the result of this in the experiment of Berzelius ought to have been to increase the quantity of chlorate, and to diminish that of the chloride formed by the decomposition of the chlorite of silver.

It is not only on account of the facts which I have mentioned that chemists had assigned the supposed composition to chlorous acid, but they were also guided by certain theoretical considerations.

The experiments made up to the present time prove with certainty, that in the decolorizing liquors which chlorine forms with the alkaline oxides, the proportions of their elements are 1 atom radical, 1 atom oxygen, 2 atoms chlorine*; and in the hypothesis of chlorides of oxides, this composition was represented by this formula $\dot{R}\,\underline{Cl^2}$. It is evident that, whatever other supposition may be adopted with respect to the nature of these combinations, it ought to fulfill the double condition, and that the same relations should always subsist, and the number of the atoms be expressed by a whole number. In multiplying the first formula by the natural series of whole numbers, and arranging

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* In England 1 atom chlorine also, for a reason already stated, viz. that the weight of the atom is double that of foreign chemists.

the atoms so as to form of them metallic chlorides and salts with an oxacid of chlorine, we shall have the following results:

$$\mathrm{R} + \mathrm{O} + 2\,\mathrm{Cl} \times 2 = \mathrm{R\,Cl}^2 + \dot{\mathrm{Cl}}^2\,\dot{\mathrm{R}}.$$

$$\mathrm{R} + \mathrm{O} + 2\,\mathrm{Cl} \times 3 = 2\,\mathrm{R\,Cl}^2 + \ddot{\mathrm{Cl}}^2\,\dot{\mathrm{R}}.$$

$$\mathrm{R} + \mathrm{O} + 2\,\mathrm{Cl} \times 4 = 3\,\mathrm{R\,Cl}^2 + \dddot{\mathrm{Cl}}^2\,\dot{\mathrm{R}}.$$

$$\mathrm{R} + \mathrm{O} + 2\,\mathrm{Cl} \times 5 = 4\,\mathrm{R\,Cl}^2 + \ddddot{\mathrm{Cl}}^2\,\dot{\mathrm{R}}.$$

Of these different formulæ, the third has been preferred by chemists, and chlorous acid has been assimilated as to its composition to nitrous and phosphorous acid. But why not adopt the second, which is certainly more simple, and in which chlorous acid is equivalent to hyposulphurous acid?

Omitting all experimental proof, this supposition is much more natural than the other; for the circumstances under which chlorous acid is formed do not at all resemble those under which nitrous and phosphorous acid &c. are obtained, whereas they are identically the same as those which produce hyposulphurous acid. It is well known, in fact, that it is by treating the alkaline oxides with sulphur and water, that mixtures of 1 atom of hyposulphite and 1 atom of polysulphuret are obtained. If on this re-action we substitute chlorine for sulphur, we shall have 1 atom of chlorite and 1 atom of chloride. The only difference existing between the two cases is, that the number which expresses the chemical equivalent of chlorine being double that which represents its atom, while with sulphur these two numbers are equal, we shall have $\dot{\mathrm{Cl}}^2$ for the formula of chlorous acid, whilst that of sulphurous acid will be $\dot{\mathrm{S}}$*.

The supposition which contributed to the adoption of the fourth formula by chemists, is that of Davy's protoxide of chlorine being a distinct compound. But as it was very evident that it was not the acid of the chlorites, the second [fourth?] formula, which led to this conclusion, was necessarily rejected. It is only since the experiments of M. Soubeiran have rendered it almost certain that the supposed protoxide of chlorine is merely a mixture of chlorine and of deutoxide, that the true composition of chlorous acid could be ascertained *à priori*. What denomination ought now to be assigned to this compound? It is evident that the name of chlorous acid can no longer be given to it, and that it is much more proper to call it *hypochlorous*, a name which recalls its analogy of constitution with the hyposulphurous, hypophosphorous acids, &c., formed like it of one equivalent of their radical and one equivalent of oxygen. Its combinations will be called *hypochlorites*. If this denomination were adopted,

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* This inconsistency does not happen when the number for chlorine is doubled.

the name of chlorous acid would be reserved for the yet unknown combination of two volumes of chlorine and three of oxygen, and that of hypochloric would signify, as M. Thénard has proposed, the compound now called deutoxide of chlorine.

§ 6.—*Of the Hypochlorites.*

The hypochlorous acid, which is obtained, with difficulty it is true, from the decolorizing compounds of chlorine, by employing the processes which I have described, presents the same characters as that furnished by the action of this gas on the red oxide of mercury. It is therefore extremely probable that these compounds contain hypochlorites mixed with chlorides. But as it is not impossible that the hypochlorous acid obtained from them, instead of being really formed in these bodies, is merely a product of their decomposition, it appeared to me proper, in order to render the demonstration more complete, to study the general properties of the compounds of this acid with bases, and, by showing that they are the same as those which have been ascertained in the decolorizing compounds, to confirm by synthesis the results obtained by analysis.

Pure hypochlorites may be obtained in two different modes, one direct and the other by double decomposition.

The direct combination of concentrated hypochlorous acid with powerful bases, whether solid or in concentrated solutions, is accompanied with the disengagement of intense heat. This heat, when it is a little too great, changes the hypochlorite into chlorate and chloride.

The presence of a certain excess of base prevents this conversion, whereas it is very rapidly effected when the chlorous acid is in excess. It is therefore necessary to add the alkaline substance to the acid, in quantity insufficient for saturation, and constantly to agitate the bottle, immersed in cold water; and not to reverse the operation, by gradually saturating the acid with the base. By taking these precautions, concentrated solutions of hypochlorous acid and potash may be used without precipitating chlorate of potash, notwithstanding the sparing solubility of this salt; this proves, that if any is formed the quantity is very small. If either of these precautions be neglected, chlorate is abundantly precipitated; when both are observed, the hypochlorous acid combines simply with the base, and no gas is disengaged; but when a high temperature or an excess of chlorous acid occasion the decomposition of the hypochlorite, a gaseous disengagement occurs. This gas is pure oxygen if the base is in excess; when on the contrary the hypochlorous acid predominates, it is oxygen mixed with chlorine.

It is easy to explain this double phænomenon. The experiments of M. Morin have proved, on the one hand, that the decolorizing compounds of chlorine lose part of their oxygen when they are converted into chlorates; and, on the other hand, I have already stated, that the metallic chlorides are decomposed by chlorous acid, and yield, among other products, chlorine gas. The formation of a chlorate is therefore always accompanied with a corresponding production of chloride.

The hypochlorites of barytes and lime serve for the preparation of other hypochlorites, by double decomposition.

Potash, soda, lithia, strontia, barytes, lime and magnesia are bases which, by combining with hypochlorous acid, form salts with it, the existence of which is incontestible. I could not however forget that M. Grouvelle, while examining the decolorizing combinations of chlorine, observed that peroxide of iron, oxide of copper and of zinc, absorbed this gas very rapidly, and formed with them decolorizing compounds, which heat and exsiccation converted into chlorine and oxides. I therefore endeavoured to produce the hypochlorites of these metals, either by directly combining hypochlorous acid with their oxides, or by double decomposition between the sulphates of these bases and the hypochlorite of lime and barytes.

In experimenting with iron, I did not obtain indications of combination by either of these methods. On the one hand hypochlorous acid, made to act upon peroxide of iron, did not dissolve the slightest portion of it; and on the other, by the decomposition of hypochlorite of lime by persulphate of iron, I obtained sulphate of lime and peroxide together; and the liquor, which contained free hypochlorous acid, contained no iron. This proves two things; first, that hypochlorous acid is extremely weak, since the base with which it was combined acts with persulphate of iron as if it were uncombined; and secondly, that hypochlorite of peroxide of iron cannot exist.

The results which I have obtained thus differ from those observed by M. Grouvelle; and I endeavoured to discover the cause of this disagreement. When operating as he did, that is to say by subjecting hydrate of iron to the action of chlorine, I found that the gas was slowly absorbed, and that the liquor was, as he stated, strongly decolorizing, even after boiling for a quarter of an hour; but I also convinced myself that during the ebullition not only chlorine, but chlorous acid, was liberated.

The liquid, before ebullition, contained a persalt of iron; but during distillation almost the whole of the peroxide was deposited in a pure state.

It appears, then, that in the action of peroxide of iron upon chlorine, both perchloride of iron and chlorous acid were formed, compounds which might co-exist, on account of the diluted condition of both of them. The intervention of heat had the effect of destroying that which had been produced in the cold, and of producing an inverse re-action, from which there resulted peroxide of iron and chlorine, while a portion of engagement of oxygen. Nevertheless, with the addition of certain cirthe chlorous acid escaped decomposition and was disengaged in the form of vapour.

The phænomena are rather different when oxide of copper or of zinc is employed; the solutions of the sulphates of these metals are decomposed by hypochlorite of lime, and sulphate of lime and the metallic oxide are precipitated together. If the chlorite of lime is in excess, the liquor does not retain the slightest portion of either metal, and by distillation hypochlorous acid only is obtained.

When however the hydrates of zinc and copper are treated with chlorous acid, a certain quantity of them is dissolved, and the liquid possesses decolorizing properties. Since, then, perfectly free chlorous acid dissolves these oxides, and these compounds are precipitated by solutions of alkaline chlorites, which contain an excess of chlorous acid, it is natural to suppose that in these cases this chlorous acid is not in a perfectly free state. It is therefore probable that some alkaline oxides, lime for example, are susceptible of forming bihypochlorites, which are decomposed by evaporation in vacuo into neutral hypochlorites and hypochlorous acid.

The hypochlorites of zinc and copper, the existence of which is rendered probable by what I have stated, suffer decomposition very readily. When they are distilled they evolve hypochlorous acid, and probably a little oxygen, and they are converted into oxichlorides. The oxichloride of copper is of a fine green colour; that of zinc is white, with an agreeable pearly lustre; it decomposes spontaneously into chloride and chlorate, with the disengagement of oxygen and a little chlorine. As to that of copper, it is decomposed by an excess of oxide, which it converts into insoluble oxichloride, also disengaging a mixture of oxygen and chlorine.

These chlorites, mixed with chlorides, may be obtained as mentioned by M. Grouvelle, by agitating either of these oxides, diffused through water, with chlorine. The absorption of the gas is rapid, especially by the oxide of zinc; the distilled liquor precipitates an oxichloride, as observed by M. Grouvelle, and it contains a metallic chloride in solution. A portion of dilute hypochlorous acid is condensed. In the absence of peroxide of mercury, the oxide of zinc and that of copper may serve for the preparation of this acid. The hypochlorites of powerful bases possess the following properties: their odour and colour are identically the same as the corresponding decolorizing compounds of chlorine, from which it is impossible to distinguish them by their physical properties; they are salts of a very changeable constitution. A slight increase of temperature, the influence of solar light, even of diffused light, converts them into chlorides and chlorates. I have not estimated the relation between the atomic quantities of these two salts.

This change is effected in the greatest number of cases with the discumstances, which I have yet imperfectly ascertained, I have not perceived any disengagement of this gas. I shall, however, soon endeavour to supply this hiatus; for the exact determination of the circumstances may greatly influence the fabrication of chlorate of potash. It is evident that if the conversion into chlorite occurred without the disengagement of oxygen, then would be obtained, for a given weight of potash, three times more of this salt than is usually procured. The presence of an excess of base may prevent the decomposition of the chlorites, but this base must be powerful; oxide of zinc and magnesia are insufficiently so for it. Therefore their chlorites, soon after formation, yield oxygen gas mixed with chlorine if they have an excess of acid, and are converted into chlorates and chlorides. The attempt to obtain these in a dry state would be fruitless, even when mixed with an excess of oxide, and the evaporation performed by the air-pump.

The case is different with those with a base of potash, soda, lime, barytes, and strontia. These may be obtained in a solid state by evaporation in a dry vacuum, or even by distillation at a low temperature; but for this there must always be a great excess of alkali. But notwithstanding this precaution, it often happens that during this evaporation, even in vacuo, a notable portion of hypochlorite is converted into chlorate and chloride.

The hypochlorites are very readily decomposed by the acids. Although hypochlorous acid expels carbonic acid from its combinations, it is in its turn expelled from its own by a current of carbonic acid gas, which shows how weak are its affinities for bases. When the dry hypochlorites are pure, they may be used for preparing hypochlorous acid gas, provided they are treated with an acid which contains but little water, —concentrated phosphoric acid for example. But as it is difficult to obtain them both dry and free from chlorides and chlorates, and, as in this case, the hypochlorous acid gas is mixed with chlorine, it is better to employ the process which I have already described for preparing it.

It has already been stated that the decolorizing compounds of chlorine have a great disposition to convert sulphur, iodine, phosphorus and arsenic, into sulphuric, iodic, phosphoric, and arsenic acid, which, combining with the oxide of these compounds, give rise to saline combinations. I have also observed the same property in the hypochlorites. Fragments of arsenic blackened on their surface by a little protoxide, put into a solution of these salts, have their metallic lustre immediately restored, as stated by M. Soubeiran to occur with the chlorides of lime, soda, &c.

The metals act with the hypochlorites as with the decolorizing combinations themselves. Gold and platina are not altered. Silver is transformed, though slowly, into chloride, with the disengagement of oxygen. Iron is very readily oxidized. As to tin and copper, they readily become oxichlorides, giving rise to a slight disengagement of chlorine mixed with oxygen. Mercury is changed into red oxichloride by contact with hypochlorite of lime.

The recently precipitated sulphurets are immediately converted into sulphates by the hypochlorites; and these salts, as well as the hypochlorous acid, might undoubtedly, as well as oxigenated water, serve for the restoration of pictures, in which the white colour employed in painting has become black by the change of carbonate of lead into sulphuret.

The greater part of the combinations of oxygen which are not saturated with this principle, undergo the same action by the hypochlorites as by hypochlorous acid itself. Thus nitric oxide is absorbed by them as by the decolorizing chlorides, and converted into nitric acid. The metallic protoxides are converted into peroxides, and the salts in *ite* are converted into salts in *ate*. It is not necessary to enumerate in detail all these re-actions, which are absolutely the same, as chemists have already observed, with the decolorizing chlorides.

The comparative action of the hypochlorites and decolorizing chlorides upon some organic matters proves, as well as the preceding facts, that a perfect identity exists between these bodies. Both possess the same power of destroying vegetable colours; but for this purpose they must not have excess of base; for alkaline hypochlorite of lime and tincture of litmus may remain during some hours in contact without the colour being destroyed.

It is well known with what activity concentrated decolorizing chlorides attack fabrics. The pure hypochlorites, unmixed with chlorides, possess also this property in a high degree: their action upon lignin, and especially upon filtering-paper, is attended with a considerable disengagement of heat; and as the hypochlorites are susceptible, as I have already stated, of being converted by heat into chlorides and chlorates with the disengagement of oxygen, the heat developed effects this conversion and produces this disengagement. The paper is essentially altered; it usually becomes friable, but it is not carbonized; and when the operation is conducted in a close vessel, so as to collect the gases, it is found that little but oxygen is disengaged, mixed with a small quantity of carbonic acid. But if rather a large quantity be acted upon, the heat developed is more intense; the paper then inflames, and there is a production in this case, not of oxygen, but of carbonic acid.

The experiments of M. Soubeiran and of M. Liebig have proved that the decolorizing chlorides can convert alcohol into a peculiar chloride of carbon. I readily convinced myself that the hypochlorites possess the same property. I have not analysed the compound of chlorine and carbon which is produced in this case; but its physical properties, and especially its odour, so much resemble those of the chloride of carbon examined by M. Liebig, that I have no doubt of their being identical, which induces the opinion that they are derived from a similar compound.

Time has not allowed of my further extending the comparison between the hypochlorites and the decolorizing chlorides: I am also of opinion that such an undertaking would be quite useless. The facts which I have announced in this chapter appear to me to be sufficient to justify the three following conclusions: 1st, the hypochlorites possess a great number of properties which characterize free hypochlorous acid: 2ndly, these properties are identically the same as those previously observed in the decolorizing chlorides; and these latter ought hereafter to be considered as mixtures of one atom of chloride and one atom of hypochlorite; 3rdly and lastly, the presence of a metallic chloride in these decolorizing compounds does not alter the properties of the hypochlorite itself. But how do these compounds serve for decolorizing and disinfecting? The answer is easy, and it arises from all the facts which I have detailed in this memoir. In the case in which an acid is added to them, they disengage chlorine, and it is then this chlorine itself which decolorizes and disinfects, by a mode of action which is not well known, but every circumstance induces the belief that it is an oxidation, produced in an indirect manner at the expense of the elements of water. If, on the contrary, they act without the assistance of acids, it is entirely by the oxygen of the acid and of the base of the hypochlorite that they decolorize and disinfect, and they are converted into chlorides.

The analogies which associate chlorine and bromine afford a presentiment that there must also exist an oxacid of this body, corresponding with the new compound of chlorine which I have now described; and this is in fact confirmed by experiment. By operating in modes similar to those by which I succeeded in obtaining hypochlorous acid, I obtained a new acid of bromine, which bears so strong a resemblance to it, that I have no hesitation in naming it *hypobromous acid* although the investigation which I have undertaken is not yet completed, and I have not determined its composition by direct experiments. I content myself at present with announcing its existence, waiting till additional researches permit me to trace its history in a less imperfect manner.

ARTICLE XIII.

On the Laws of the Conducting Powers of Wires of different Lengths and Diameters for Electricity; by E. LENZ.

(Read to the Academy of St. Petersburgh, the 28th of November, 1834.)

From the *Mémoires de l'Académie Impériale des Sciences de St. Petersbourg*:
VI^{me}. series, tom. i. 1835.

THOUGH Van Marum, Priestley, Children, Harris, and Davy* had previous to Galvani's brilliant discovery endeavoured to determine the conductibility of different metal wires by discharges of the Leyden battery, yet the first more accurate experiments on this subject were made at a later period by means of the electromotor and the voltaic pile; but, strange to say, these later and more accurate experiments have led to results at variance with each other. Whilst the experiments of Davy, Pouillet, Becquerel, Christie, Ohm, and Fechner prove the law that wires of the same metal conduct electricity inversely as their lengths, and directly as their sections—that is to say, as the squares of their diameters—Barlow and Cumming consider, according to their experiments, that the conductibility is inversely proportionate to the square of the lengths, and directly as the diameters of the wires (or as the square roots of their sections). Ritchie, whose observations on this subject are the most recent (Phil. Trans. for 1833, p. 313), but who, unfortunately, like most English authors, is totally unacquainted with the works of the German natural philosophers Ohm and Fechner, endeavours to explain this contradiction by assuming that the conductibility of the wires varies according to the force of the current; for having connected two wires with two different batteries, he found by means of his galvanometer that the strength of the currents were not in the same proportion to each other. He explains this according to his own view of the conductibility of electricity in the following manner:

"Let us suppose that there is no actual transfer of electricity along the wire, but that all the phænomena of deflection, &c. result from a definite arrangement of the electric fluid essentially belonging to the wire itself. Let us further suppose that a section of wire contains one hundred particles of electricity, and that the battery is capable of ar-

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* [M. Lenz is here in error; the experiments of Children, Davy, and Harris were all made subsequently to the discovery of Galvani.—EDIT.]

ranging one fourth of these, or twenty-five particles, then there will only remain seventy-five to be arranged by any increase of power. Let us now suppose we have another wire of the same length, whose section contains only twenty-five atoms; it is obvious that this battery will be able to arrange more than one fourth of this number, so that the ratio of the conducting powers cannot be as one to four, but will be found by actual experiment a very different ratio. If we increase the size of the battery, suppose the size of the plates to be doubled, then it is obvious we shall not double the deflecting power. For out of one hundred particles there are only seventy-five remaining, a part of which only can be arranged by the increased part of the battery. Hence the deflecting force increases very slowly with the increased size or energy of the battery."

That this view of Ritchie's is wrong may easily be proved by experiments, of which the numerical determinations of Fechner afford numerous examples. There are, in fact, some arrangements, particularly in closed galvanic series, which show such a relation between the pile and the connecting wires, that the increase of the plates does not perceptibly increase the strength of the current in the wire. This would, according to Ritchie's opinion, prove the whole of the fluid to be already entirely disposed of, and that it would be impossible to pass any more fluid through the same wire. But this is by no means the case; for if, instead of doubling the number of plates, their size be doubled, the force will be almost exactly double. This proves, therefore, that the whole of the fluid has not yet been disposed of, and that another arrangement of the voltaic pile only was required to produce the arrangement of a double number of particles of the fluid. Besides, Ritchie's theory does not explain the difference of effect produced by a voltaic pile of many plates, and by one of fewer plates but of larger size.

Ohm however several years ago furnished us with a theory of the galvanic battery which supplies this deficiency; but being only published in German, it is unknown both in France and in England. This theory explains perfectly the difference between Barlow's results and those of other natural philosophers who have occupied themselves with this subject, as well as the doubts of Ritchie. The latter says, in his paper above quoted, that "The conducting power of a wire must be a function of all the quantities concerned in the experiment. These quantities are obviously the diameter of the wire, its length, the size of the battery, and the strength of the acid." Had he, instead of the term "conducting power" used that of strength of the current, he would have been nearer the mark, and given a proper explanation of those apparent anomalies.

Before quoting the simple formula of Ohm we must observe, that this philosopher always uses the term conducting resistance instead of the term conductibility. The latter is inversely proportional to the conductibility, so that when L signifies conducting resistance,

$$L = \frac{1}{\text{conductibility}}.$$

If therefore the conducting resistance of a galvanic arrangement be expressed by L, and its electromotive power by A (which we may here suppose to be produced either by contact or chemical affinity), then Ohm's formula for the power of the current will be

$$F = \frac{A}{L}.$$

But L expresses not only the conducting resistance of the connecting wire, but that of the entire voltaic arrangement; that is to say, the sum of all the conducting resistances of the fluid and of the fixed parts of the circuit. If we take for unity of conducting resistance the length and thickness of a wire of a given substance, we may in this unity express by l the resistance of the battery itself (both plates and acid taken together); and by λ the resistance of the connecting wire, the conductibility of which is to be ascertained; and the formula will then stand thus:

$$F = \frac{A}{l + \lambda},$$

which will give for the conducting resistance of the connecting wire

$$\lambda = \frac{A}{F} - l.$$

We may take the electromotive power itself for unity, should it remain unaltered, which is almost always the case in experiments on the conductibility of wires; the formula will then stand thus:

$$\lambda = \frac{1}{F} - l.$$

By closing now the battery with wires of different conducting resistances, $\lambda_{(1)}$, $\lambda_{(2)}$, $\lambda_{(3)}$, &c., and denoting the corresponding currents by $F_{(1)}$, $F_{(2)}$, $F_{(3)}$, we shall have the following proportion:

$$\lambda_{(1)} : \lambda_{(2)} : \lambda_{(3)} \ldots\ldots = \left(\frac{1}{F'} - l\right) : \left(\frac{1}{F''} - l\right) : \left(\frac{1}{F'''} - l\right).$$

The conducting resistances therefore are not in an inverse ratio to the intensities of the observed currents, or in a direct ratio to the conductibilities; but we must deduct from the latter the constant quantity l before such proportion can obtain.

After having established this simple principle, let us now return to the above-mentioned experiments of various philosophers on the conductibility of wires, and we shall find that Ohm and Fechner calculated the results of their experiments according to this principle; whilst Davy and Becquerel conducted their experiments in such a manner as to avoid the errors which otherwise would have been the consequence of disregarding it. This explains why the results of their experiments agree with those of the German philosophers. Davy, for instance, connected the poles of a voltaic pile in two ways at the same time; the one was by a simple wire, the other by an apparatus for decomposing water: the current therefore was divided between these two paths, and the conducting power of the current passing through the metal conductor was so far increased, by shortening the length and increasing the thickness of the conductor, that the current passing through the water became so weak that no decomposition took place. Davy endeavoured to obtain the same limit by means of wires of different thicknesses, and in this manner found that two wires of different thicknesses attained the same limit when their lengths were proportional to their sections. Both connecting wires attaining thus the same strength of current, it would be necessary to bring the conductibility of one wire to be exactly equal to that of the other, independent of every theory of the dependence of the intensity of the current on the parts of the voltaic arrangement, in order to ascertain the proportion of the length to the thickness. Such experiments would certainly produce results containing no decisive errors; they will, however, admit but little accuracy of determination.

Becquerel coiled two wires of the same substance, but of different length and thickness, round the frame of a multiplier, so that the coils of the one laid between those of the other; when therefore a current of the same strength was passed through them in opposite directions the needle of the multiplier remained at rest. He joined the ends of each wire with the same voltaic pile, but in opposite directions with respect to its poles. The wires having different sections, the current was, for equal lengths, stronger in the thicker than in the thinner wire, and he therefore found a deviation of the needle of the multiplier. He then diminished the length of the thinner wire till the current became equal in both,—that is to say, till the index returned to its place of rest. He thus obtained two wires of different length and thickness, which both conducted the electricity equally well; and concluded, from comparing their dimensions, that in equally good conducting wires of the same substance the lengths are proportional to the masses, that is to say, to the sections. It is this proposition alone which the experiments of Davy also demonstrate, and Ritchie is perfectly right in objecting to experiments of this kind; but he unjustly charges Becquerel with not having well observed it himself. It was only after having ascertained by other experiments that the conductors are in an inverse ratio to the lengths, that Becquerel arrived, from the above-mentioned experiments, at the conclusion that they are in a direct ratio to the sections.

Pouillet placed successively various wires between the poles of one and the same pile, and determined the strength of the current by the tangent of the angle of deviation. He asserts that he found the conductibility proportional to the section; and with respect to the length, he came to this interesting result, that the strength of the current is in an inverse ratio to the length of the wires, if constant quantities are added to them. If we denote the strength of two currents by F and F′, the corresponding lengths of the conducting wires by λ and λ', and a constant quality by l, Pouillet's formula will be

$$\frac{F'}{F}=\frac{l+\lambda}{l+\lambda'};$$

but this is only the immediate consequence of our formula for the strength of the current according to Ohm's theory, for according to it we have for the conducting resistances λ and λ' (which are proportional to the lengths),

$$F=\frac{A}{l+\lambda} \text{ and } F'=\frac{A}{l+\lambda'};$$

therefore

$$\frac{F'}{F}=\frac{l+\lambda}{l+\lambda'},$$

which exactly resembles Pouillet's formula, with the difference merely that l has here a determinate signification; it is, namely, the conducting resistance of all the other parts of the circuit, except that of the wire experimented upon. A very careful series of experiments referring to the point in question is furnished us in a paper by Christie on the production of currents by electro-magnetic induction by means of Knight's great magnet. Totally unacquainted with the theory of Ohm, he was, like Pouillet, induced, by an accurate discussion of his own observations, to add to the conducting power of the wires employed in the experiments a constant expressing the conducting resistance of the wire of the multiplier. He must have obtained in this manner correct results, and was in fact led to the formula for conductibility $\frac{D^2}{L}$, in which D signifies the diameter of the wire and L its length. Calculating by means of this formula the angles of deviation, he found that the calculations perfectly agreed with the observations made*.

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* We perceive in Christie's otherwise very valuable paper the want of a correct distinction between the electromotive power and the generated current; the latter being equal to the former divided by the resistance to conduction. The electromotive power induced by the magnet in spirals, of any substance and dimension whatever, is always the same (see *Mémoires de l'Académie de St. Pétersbourg,—Sciences Mathém.,* vol. ii. p. 427,) whilst the current is in an inverse

Barlow made his experiments in the same manner as Pouillet, and would have obtained the same results had he added, as Poullet did, a constant to the conducting power of his wires, without which his results necessarily proved erroneous. He supposed that the conducting power was inversely as the square root of the length, and directly as the section. Such a proportion may indeed easily be found between the resistance of the pile and the two wires employed for the experiment, that, according to Barlow's calculation, nearly such a law may be obtained. Had he employed, for instance, two wires of the same diameter, the lengths of which were m and n, his view must have been confirmed, when the conducting resistance of the pile itself was reduced to that of one wire of the same diameter and the length

$$\frac{m\sqrt{(n)} - n\sqrt{(m)}}{\sqrt{(m)} - \sqrt{(n)}}.$$

It is indeed not very probable that such would have always been the condition of the pile, but the experiments agree so little with the calculations performed according to his principle, that differences of 6° and 7° may be found in them.

Cumming used the thermo-electric pile to excite the current, but he also did not in the least consider the conductibility of the thermo-electric metals; and he himself owns that the experiments agree but approximately with his theory *.

We perceive, then, by the above, that all the contradictory results of Barlow's, Cumming's, and Ritchie's experiments, in opposition to the law established by other philosophers, are reduced to a mere nothing by an accurate appreciation of the mode in which they performed their experiments. The axiom *that the conductibility of wires of the same substance is inversely as their lengths and directly as their sections* is established by Ohm and Fechner in so conclusive a manner, and with such a full appreciation of all the connected circumstances, that a further and more minute illustration seems to be almost superfluous; still the method of determining the power of conductibility by the induced electro-dynamic current† offers so much facility and accuracy

ratio to the resistance; and as in the experiment on this subject by Faraday the entire resistance of the voltaic arrangement remained the same, when the wires were compared two by two, the currents produced by induction must also have been the same.

* [The results obtained by Harris, by means of his thermo-electrometer, (Trans. Royal Soc. of Edin. 1832,) are also easily reconciled with the theory here advocated. This able experimentalist found that the differences in the conducting powers of wires of different diameters became more apparent within certain limits, as the force of the battery increased. These experiments were made with wires of very short lengths and small diameters.—Edit.]

† Christie has also employed, in his above-mentioned experiments, currents produced by electro-dynamic induction, but more recently than I have. His paper was presented to the Royal Society on February the 8th, 1833, whilst

in observation*, that it appears to me worth while to apply this method, and thus exhaust all the proofs of this important point in the theory of the pile. I shall therefore propound them in the following series of experiments, which, though they do not establish a new law, will yet I hope secure for ever the old one against all further objections.

The manner in which these experiments have been performed does not at all differ from that described in the above-mentioned memoirs, and I refer therefore to them for the description of the apparatus and the manner of experimenting. I shall but expatiate upon one point, which should have there been more fully investigated. Having fastened the horse-shoe magnet in a vertical position, I suddenly detached from it the cylindrical keeper with the surrounding electromotive spiral in order to produce the current, and observed through a glass the suddenly produced deviation of the needle of the multiplier. From this mode of proceeding a doubt may arise, whether the current thus produced was always of the same strength, or whether the suddenness of the disruption, which cannot be always strictly equal, did not exercise a real influence; and Christie has indeed, for fear this might be the case, performed the disruption by the fall of a determined weight from a determined elevation. I convinced myself however at the very beginning by experiments that the suddenness of the disruption, having surpassed a certain term, exercises no influence whatever on the strength of the current. The results of the experiments were the following:

	Experiment.			Average.
	1st.	2nd.	3rd.	
By purposely breaking contact very slowly......................	100·7	100·7	100·8	100·73
By purposely breaking contact very suddenly	100·7	101·0	100·6	100·77
By breaking contact with ordinary quickness	101·0	100·2	100·7	100·63

The averages show that the influence of the suddenness of disruption may be considered as = 0, the differences of the deviations of the needle not surpassing $\frac{2}{10}$ of a degree. Besides the above experiments, performed solely for this purpose, there are also numerous confirmations of

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my first paper on this subject was read before the Imperial Academy of Sciences on November the 7th, 1832. (See *Mémoires,—Sciences Mathém. et Phys.*, vol. ii. p. 427.)

* See *Mémoires de l'Acad. Imp. de St. Petersb.* (*Sciences Mathém. et Phys.*, vol. ii.)

these results in my former memoirs; where, in many places, the same observations of deviation were successively repeated in order to obtain a greater accuracy, and where the results leave nothing to wish as to their accordance. Convincing proofs may moreover be found in the following series of experiments.

The reason why the strength of the current cannot depend on the suddenness of the disruption might also be easily demonstrated by theoretical considerations. The current in the electromotive spiral arises from this cause, that the magnetic intensity of the keeper of soft iron about which the spiral is coiled diminishes from a maximum to a minimum,—the latter approaching nearer to 0 in proportion as the iron possesses a less coercitive power. We may imagine this diminution of the magnetic intensity of the keeper to be divided into indefinitely small parts, each of them producing in the spiral an indefinitely small current, and this again exercising an indefinitely small effect on the needle of the multiplier: all these effects taken together produce the entire deviation of the needle. If, therefore, the whole series of indefinitely small effects act on the needle successively, but in so short a time that they exercise their whole force when the needle is in a position very little different from the normal one, the sum of the effects will be as great as that which would be produced by the whole force acting at once, or by the iron passing suddenly from the maximum to the minimum; and within these limits suddenness of disruption will have no influence over the deviation. Experience teaches therefore that the suddenness I commonly employed was within these limits; which will be more easily conceivable from the circumstance that the intensity of the keeper diminishes in the beginning much more quickly than at the end: so that we may take it for granted that more than $\frac{9}{10}$ of the indefinitely small diminution of intensity takes place during the contact of the magnet with the keeper and within the distance of one inch from it.

My first experiments refer to the law of conductibility of wires of different lengths. I cut for this purpose a copper wire (0·023 Engl. inch thick), covered with silk, into five pieces, so that each was 7 Engl. feet long, and observed the deviations of the needle produced by the disruption of the keeper, together with the electromotoric spiral from the magnet, by inserting between the wire of the multiplier and the electromotive spiral either no wire at all, or one, two, four, &c. wires successively. Their extremities were connected by immersion in mercury. The following table shows the results of these experiments; the signification of the numbers 1, 2, 3, 4 will be found in my former Memoir.

	Angles of deviation.					Calculated angles of deviation.	Differences.
	1.	2.	3.	4.	Average.		
Without any wire interposed at the beginning of the experiment...	85°·3	89°·7	89°·7	88°·5	88°·31	88°·52	+0·21
Without any wire interposed at its end	85·7	89·3	89·8	88·5			
With interposed wire 7 feet long	52·3	52·4	54·5	53·4	53·15	53·21	+0·06
———— 14 ————	38·2	38·1	39·6	39·1	38·75	38·51	−0·24
———— 21 ————	29·7	29·8	31·5	30·6	30·40	30·25	−0·15
———— 28 ————	24·3	24·6	24·8	25·8	24·87	24·93	+0·06
———— 35 ————	20·9	20·6	22·0	20·9	21·10	21·21	+0·11

The calculation of the values of the 7th column was performed in the following manner. I took for the unit of the conducting resistance the length of 1 English foot of the wire, from which the five pieces had been cut, and represented the unknown strength of the current corresponding to this resistance by $p\,y$: and I assumed x for the equally unknown conducting resistances of the wire of the multiplier and the electromotive spiral taken together. Assuming the hypothesis that the resistances of the wires are proportional to their lengths, and designating the deviations observed in the above table for the resistances x, $x+7$, $x+14$, &c. by a_x, a_{x+7}, a_{x+14}, &c. we shall find, by means of the formulæ given in the former Memoir, the following equations:

$$\frac{A}{1} = py$$

$$\frac{A}{x} = p \cdot \sin\left(\tfrac{1}{2}a_x\right)$$

$$\frac{A}{x+7} = p \cdot \sin\left(\tfrac{1}{2}a_{x+7}\right)$$

$$\frac{A}{x+14} = p \cdot \sin\left(\tfrac{1}{2}a_{x+14}\right). \qquad \text{\&c.}$$

Dividing the rest of the equations by the first, and designating the values $\sin\left(\frac{1}{2}a_x\right)$, $\sin\left(\frac{1}{2}a_{x+7}\right)$, $\sin\left(\frac{1}{2}a_{x+14}\right)$, &c. for brevity sake by a, a', a'', &c. we shall obtain

$$x = \frac{y}{a}\text{; and therefore } a\,x - y = 0$$

$$x + 7 = \frac{y}{a'} \text{ ———— } a'x - y + 7a' = 0 \qquad \text{(A)}$$

$$x + 14 = \frac{y}{a''} \text{ ———— } a''x - y + 14a'' = 0.$$

&c.

Now by solving these equations with regard to x and y according to the method of the least squares, we shall obtain

$$x = 12{\cdot}5386 \qquad y = 8{\cdot}7508$$

If we substitute these values in the equations (A) and ascertain from them the values a, a', a'', or $\sin(\frac{1}{2} a_x)$, $\sin(\frac{1}{2} a_{x+7})$, $\sin(\frac{1}{2} a_{x+14})$, we shall obtain the angles $\frac{1}{2} a_x$, $\frac{1}{2} a_{x+7}$, $\frac{1}{2} a_{x+14}$, and multiplying them by 2 we shall find out the angles of the seventh column.

The very slight differences of the values indicated in the 8th column from those observed, convince us that the hypothesis which is the base of the calculation is correct, and that therefore *the resistances of the wires are in a direct ratio, and their conductibilities in an inverse ratio to their lengths.*

I performed a short time previously another series of experiments, and diminished the electromotive spiral by two coils without shortening its length or altering its resistance. The results are contained in the following table:

	Angles of Deviation.				
	1.	2.	3.	4.	Average.
Without any wire interposed, at the beginning of the experiment............	77°·9	81°·1	81°·7	81°·7	80°·6
Without any wire interposed, at the end of it. ...	77·3	80·2	81·3	80·2	79·69
	77·3	80·2	81·2	79·8	
With interposed wire 7 feet long	47·6	48·7	49·9	49·5	48·92
—————— 14 ———	34·8	35·0	36·6	35·8	35·55
—————— 21 ———	27·4	27·7	28·4	28·6	28·02
—————— 28 ———	22·5	23·3	23·4	22·8	23·00
—————— 35 ———	19·4	18·8	19·8	19·3	19·32

We perceive by the deviations which occurred at the beginning and end of the series of experiments, performed without inserting the wires between the spiral and the wire of the multiplier, that the power of the magnet was a little diminished during the experiment. This induced me, therefore, before the calculation of the results of the experiment, to make a slight correction of the angles of deviation, founded on the principle that the diminution of power was proportional to the time, and that the observations with various lengths of wire followed each other at equal intervals, which was nearly the case. I represented the angle of deviation when no wires were interposed at the beginning of the experiment by a_x, and at the end of it by $a_{(x)}$ and found

$$\sin \tfrac{1}{2} a_x = (1 + m) \sin (\tfrac{1}{2} a_{(x)})\,;$$

consequently

$$m = \frac{2 \cdot \cos \frac{1}{4}\,(a_x + a_{(x)}) \cdot \sin \frac{1}{4}\,(a_x - a_{(x)})}{\sin \frac{1}{2}\, a_{(x)}}.$$

Having in this manner found out m for the end of the series of experiments, the correction of other angles became easy by multiplying the sines of their halves respectively by

$$\left(1 + \frac{m}{6}\right),\ \left(1 + \frac{2m}{6}\right),\ \left(1 + \frac{3m}{6}\right),\ \&c.$$

In this manner the angles of deviation of the 2nd column of the following table are calculated.

	Angles of Deviation		Difference.
	observed.	calculated.	
Without any interposed wires	80°·60	80°·53	−0°·07
With interposed wires 7 feet long	48·96	49·08	+0·12
———— 14 ————	35·60	35·62	+0·02
———— 21 ————	28·09	28·03	−0·06
———— 28 ————	23·07	23·12	+0·05
———— 35 ————	19·73	19·69	−0·04

The 3rd column is calculated entirely according to the above formulæ (A). The smallness of the differences of the 4th column proves likewise the correctness of the proportionality of the conducting resistances to the lengths of the wires. The following values were obtained for x and y:

$$x = 12{\cdot}583 \qquad y = 8{\cdot}133.$$

x was according to the previous calculation $= 12{\cdot}539$, the difference therefore of the resistances was only $= 0{\cdot}044$ of an English foot: y considerably differed from the preceding value, the electromotive spiral having here two coils less.

Passing now to experiments which are to indicate the proportion of the conductibility with reference to the diameters of the wires, I must previously observe, that the difficulty of obtaining accurately agreeing results is here far greater than in making experiments with wires of the same diameter though of different lengths. The reason of this is the difficulty of obtaining wires of different diameters quite equal in every other quality. Should we, for instance, take copper wires of different thickness as they are sold in shops, we should then obtain entirely different results. I once obtained two copper wires, of which that having the greater diameter was a worse conductor than that which was thinner, contrary to all previous approved experiments, all observers agreeing that thick conductors conduct electricity better than thin ones. How far the conductibility of metals is altered by even a very slight mixture of foreign substances, is proved by the experiments of Pouillet with wires made of different alloys of silver with copper, and gold with silver. These prove that their conductibility is far below that of pure unalloyed metals. Thus, for instance, the conductibility of fine gold = 84·41, that of the 18-carat gold = 14·77, whilst the silver with which the gold is alloyed is a much better conductor than fine gold itself.

In order to avoid the inequality of the purity of the copper, I procured the wires myself by cutting a thick piece of copper into smaller pieces, and drawing it afterwards to different thicknesses: but the thinner wires become thus by drawing somewhat more dense than the thicker ones, which produces always a difference in the substance. I endeavoured to remedy this evil by making the wires red hot before covering them with silk; but the greater or less degree of heat may perhaps have some influence.

From these observation it follows, that so close an agreement cannot be expected from the following experiments as from those made for the purpose of ascertaining the influence of the length of wires on their conductibility; this agreement will, however, be sufficiently close to remove every doubt with respect to the correctness of the law to be established.

The wires employed were, as has already been said, wrought all out of the same piece of thick wire, and drawn through the holes 1, 6, 11, 18, 24, 30, with which numbers they are also marked in the table; they were all heated to redness and overspun with silk, and were successively inserted between the electromotive spiral and the wire of the multiplier, and the deviation determined in the same manner as in the previous experiments. In order to obtain the proportion of the sections of the wires, I took 2 feet of each and weighed them before they were covered with silk. The weight is proportional to the section, and is as follows:

Weight of 2 feet of wire	No. 1	7·7370 grammes.
	No. 6	5·0250
	No. 11	3·2408
	No. 18	1·4783
	No. 24	0·7750
	No. 30	0·3616.

It is not of much importance to know the absolute thickness of the wires, but it may be easily deduced from this, that the diameter of the wire No. 1 was nearly 0·046 Engl. line.

The experiments were thus performed: I observed first the deviation without any wire being interposed, then with the interposition of No. 1, 2, to 6, and back from 6, 5, to 1, and finally without any interposition. Taking afterwards the average of observations of the same kind, the influence which the loss of the power of the magnet might have exercised has been thus entirely removed. The length of all the wires collectively was = 16 feet Engl. The experiments are stated in the following table:

	Angles of Deviation.				Average.
	1.	2.	3.	4.	
Without any wire interposed ...	90°·7	93°·6	94°·9	95°·3	93°·62
16 feet of wire No. 1 interposed	64·2	66·3	67·0	67·4	66·22
———— No. 6 ————	55·0	57·1	57·9	58·5	57·12
———— No. 11 ————	44·9	47·4	47·6	48·6	46·97
———— No. 18 ————	29·6	31·5	31·2	32·0	31·07
———— No. 24 ————	19·0	19·8	19·6	19·7	19·52
———— No. 30 ————	9·3	11·4	10·8	11·5	10·75
———— No. 30 ————	9·6	11·6	11·0	11·7	10·97
———— No. 24 ————	18·1	20·3	19·0	20·2	19·40
———— No. 18 ————	29·4	31·5	31·3	31·8	31·00
———— No. 11 ————	45·2	47·5	48·0	48·6	47·32
———— No. 6 ————	55·4	55·3	58·0	57·3	56·75
———— No. 1 ————	64·9	65·4	67·0	67·7	66·25
Without any interposition	91·3	91·9	93·6	94·7	92·87

Taking the average of observations made in similar circumstances, the values will be

	Angles of Deviation		Difference.
	observed.	calculated.	
Without any wire interposed	93°·24	91°·53	− 1°·71
With the wire No. 1	66·24	65·84	− 0·40
———— No. 6	56·94	57·52	+ 0·58
———— No. 11	47·16	48·09	+ 0·94
———— No. 18	31·04	31·22	+ 0·18
———— No. 24	19·46	19·78	+ 0·32
———— No. 30	10·86	10·56	− 0·30

The calculation was performed in the following manner:

Let the strength of the current when it passes through a wire of the length used in these experiments (that is to say 16 feet), and of such a thickness that 2 feet of it weigh 1 gramme, be $= p\,y$ (where y is an unknown quantity to be determined by the experiment); and let the length of the wire of the multiplier, together with the length of the electromotive spiral reduced to the same thickness as the wire be, $= x$.

Supposing therefore that the conductibility is in a direct, and the conducting resistance in an inverse ratio to the thickness of the wires, and that a, a', a'' represent the angles of deviation, we have the following equations:

$$\frac{A}{1} = py$$

$$\frac{A}{x} = p \cdot \sin\left(\tfrac{1}{2}a\right)$$

$$\frac{A}{x + \frac{1}{7{,}737}} = p \cdot \sin\left(\tfrac{1}{2}a'\right)$$

$$\frac{A}{x + \frac{1}{5{,}025}} = p \cdot \sin\left(\tfrac{1}{2}a''\right)$$

&c.

Dividing the first equation by all the following seven, and putting for brevity sake

$$\sin\left(\tfrac{1}{2}a'\right) = a', \text{ \&c. and } \frac{a'}{7 \cdot 737} = \delta', \ \frac{a''}{5 \cdot 025} = \delta'', \text{ \&c.,}$$

we obtain the following equations:

$$\begin{aligned} a\,x - y \quad &= 0 \\ a'\,x - y + \delta' &= 0 \\ a''\,x - y + \delta'' &= 0 \\ a'''\,x - y + \delta''' &= 0, \end{aligned}$$

&c.

From these equations were determined, according to the method of the least squares, the values

$$x = 0 \cdot 40679 \qquad y = 0 \cdot 29146.$$

and finally the values a, a', a'', by substituting the foregoing values in the equations, and developing a, a', a''....or $\sin \frac{1}{2} a$, $\sin \frac{1}{2} a'$, $\sin \frac{1}{2} a''$, &c. These values are placed in the above table under the head "Calculated Deviations." The differences between these and the observed angles of deviation, contained in the last column, are greater than those of the preceding observations, and even greater than can be ascribed to mere errors of observation; but the reasons of this have been already explained. Their agreement, however, is in every case great enough to remove every doubt with respect to the correctness of the hypothesis (which is the basis of the calculation), *that the conductibility of wires is in a direct ratio to their sections.*

ARTICLE XIV.

Memoir on the Polarization of Heat; by MACEDOINE MELLONI.

From the *Annales de Chimie et de Physique*, vol. lxi., April, 1836.

ABOUT twenty-five years since, M. Berard, of Montpellier, announced that heat was capable of undergoing double refraction and polarization[*]. His experiments, which were repeated in presence of Berthollet and Dulong, were universally admitted by philosophers until towards the close of 1829, when doubts as to the certainty of the conclusions which had been deduced from them were raised by Mr. Powell, in his account of some unsuccessful experiments of the same kind, made with an apparatus similar to that employed by Berard for the purpose of polarizing heat by reflection[†]. In 1834, I found that calorific rays, in their passage through plates of tourmaline which completely polarized light, gave no apparent sign of polarization[‡]. Nobili, whose recent death science has so much cause to deplore, arrived some time subsequently at the same result. He attempted also to polarize heat by reflexion, but obtained no satisfactory indication[§]. At last Mr. Forbes observed, about the close of 1834, signs of polarization in the heat transmitted through tourmalines and small piles of mica placed at a proper inclination to the incident rays. In these experiments, the greatest proportion of polarized heat was given by a system of piles composed of plates of mica, and amounted to $\frac{40}{100}$ when Mr. Forbes operated on the calorific rays of a spiral of platina kept in a state of incandescence by the flame of alcohol; but this proportion was reduced to $\frac{17}{100}$ or $\frac{6}{100}$ when the same piles were brought to act on the caloric issuing from a vessel heated by mercury or by water in a state of ebullition[||].

The different temperatures of the calorific rays are to radiant heat what the different colours of the luminous rays are to light. Now, it is known that the latter are all equally polarized by the action of the same polarizing system. The experiments of Mr. Forbes would seem

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[*] *Mémoires de Physique et de Chimie de la Société d'Arcueil,* tom. iii. page 5.

[†] Edinburgh Journal of Science, S. S. vol. vi. and x.

[‡] *Annales de Chimie et de Physique,* tom. lv. page 375.

[§] *Bibliothèque Universelle de Geneve,* tom. lvii. p. 1.

[||] Transactions of the Royal Society of Edinburgh, vol. xiii. part. I. p. 152.

then to indicate a very marked difference in this respect between the laws of polarization of heat and those of light.

Are calorific rays really susceptible of polarization? Are they all equally and completely so? Such are the questions which I propose to consider in this memoir, endeavouring at the same time to account for the contradictions (more or less obvious) exhibited by the results at which the different observers just quoted have arrived.

The instrument invariably employed by me in these researches is an excellent thermomultiplier constructed by M. Gourgon. In order to give the reader an idea of its great sensibility, it will be sufficient to state that the natural heat of the hand placed near one of the extremities of the tubes with which the pile is furnished, will impel the index to its maximum of deviation when the temperature of the atmosphere is below 15°. The pile, which has its two terminating faces perfectly symmetrical, consists of thirty pairs (bismuth and antimony,) formed into a bundle measuring eight lines in the diameter of its transverse section and ten lines in length*: the tubes or cylindrical appendages which envelop its two faces are nearly of the same breadth as the pile, but three times as long. The astatic system of the galvanometer, which consists of two needles very powerfully magnetized, measuring $0^{mm}{\cdot}47$ in diameter and 53^{mm} in length, makes but two oscillations a minute. If, however, after the communication with the pile has been interrupted, the system is turned aside 35 or 40 degrees

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* The symmetry, or rather the equality of the two opposite sides of the pile is a condition indispensably necessary, in order to render the observations independent of the slight changes of temperature that may take place in the surrounding air during the experiments. In fact, if the bars of bismuth and antimony were stronger, or their solderings less extended, on either of the two sides than on the other, the heating or cooling of the air would no longer be communicated (by contact) with equal promptitude to them both, and the extremities which presented the least mass in proportion to the extent of the soldering would be heated or cooled more rapidly than the opposite ends. This circumstance would produce a current which, by its intervention, would disturb the calorific effect of the rays that are received by the anterior face of the pile Hence it is obvious that with piles having their opposite faces unequal, exac measures of the calorific radiations are attainable only in that case in which the temperature of the atmosphere undergoes no sensible variation. If it varies, the results will be less accurate in proportion to the greater rapidity of the variation and the greater length of time required to make the experiments. The piles [à rayons et à biseau,] described by Nobili in the 57th volume of the *Bibliothèque Universelle* have not their opposite faces symmetrical; they are therefore not free from the defect just mentioned. The author himself admits it in the 8th page of the same volume, where, after having given the different rates of calorific transmission obtained in a series of bodies by means of his pile, he adds, that the conditions of temperature which could affect the results, and consequently *change them by their variations*, are the following: 1st, the tem perature of the source; 2nd, that of the bodies themselves, and *particularly* th of the surrounding air.

by means of a magnet or a piece of soft iron held at a proper distance, and is then left to itself, it resumes its natural position of equilibrium, and stands perfectly steady at the zero of the scale after the lapse of three or four minutes. This quick return to a state of rest is the effect of the neutralizing action of the copper disc placed for that purpose beneath the divided circle; for, since the beautiful discovery of M. Arago, it is well known that the action of this disc diminishes the amplitude without changing the time of the oscillations; so that the oscillatory motion of the needles about the point of equilibrium is considerably reduced in duration, though the forces by which it is produced have undergone no change of intensity. The time required for the return is still less when the galvanometer is left in communication with the pile, and the deviation of 35° or 40° is produced by the calorific radiation; for then about two minutes are sufficient to cause the index to resume its stationary position at zero after the interception of the radiation. In this case, the heat still remaining for some time on that face of the pile which has been exposed to the radiation may be said to sustain the needles in their fall, and prevent them from oscillating to the opposite side of zero, at which they arrive almost exactly within the time required for the re-establishment of the equilibrium of temperature between the two faces of the pile.

But in order to observe the latter periods with the utmost possible exactness, it was necessary to secure the pile from the differently heated currents of air which come successively into contact with each of the two appendages. These differences of temperature, though exceedingly small and imperceptible with the best common thermoscopes, are sufficient to produce, in the instrument used by me, such deviations to the right and left as occasionally amount to some degrees, and interfere most inconveniently with the action of the calorific rays. To prevent this inconvenience, I placed the pile and its stand in a large metallic receptacle or case resembling a trough with the bottom turned upwards, and measuring eighteen inches in length by eight in breadth. The sides of this trough (which are double) are of such a height that the bottom may not touch the most elevated parts of the mounting of the pile; and the interval between the interior and exterior side is filled with cotton at the lower edge, in order as much as possible to prevent the entrance of the external air. The wires which establish the communication between the pile and the galvanometer are passed under the edges, which for this purpose are grooved at one of the extremities of the case.

In one of the shorter sides there is, at the same elevation as the pile, a circular aperture about an inch in diameter; but this may be reduced to any degree of minuteness by sliding into an exterior frame a plate with a smaller aperture. A longitudinal opening made from end to end in the superior surface of the case enables us to see whether the axis of the pile is placed in the direction of the radiation. By gently moving the case in one direction or another, the circular aperture is brought into the proper position relatively to the pile, which continues to occupy the same part of the table on which it has been placed.

By means of this double case, the air surrounding the thermoscopic body is kept perfectly calm. At least the motions produced in it by its slow variations of temperature react upon the two sides of the pile with such uniformity that the index of the thermomultiplier stands exactly at the zero of the scale, and when a specific deviation has been produced by the influence of an exterior calorific radiation, returns to that point in some minutes after the communication of the rays has been intercepted by means of a metallic screen.

Those who are in the habit of frequently using astatic galvanometers must have remarked, no doubt, that these instruments, whatever may be the solidity of the table on which they stand, suffer, in consequence of the observer's changing his place or of the passing of vehicles in the neighbourhood, a slight tremulous motion, which, being transmitted to the suspension thread and to the needles, causes them to oscillate like a pendulum for a period of greater or less duration. For the purpose of preventing these eccentric oscillations, which prove so embarrassing in very delicate observations, we have but to fix the instrument on the marble slab of a chimney or any other horizontal plane firmly fastened to one of the solid walls of the building in which we are carrying on our operations. The wire will then always preserve its vertical direction; the needles will be no longer susceptible of any other than horizontal movements; and the index will deviate with so much regularity under the action of the electric currents, that, upon seeing it, one might fancy that, instead of being suspended by a thread, it moves on a pivot.

In the particular instance under consideration, the deviation commences as soon as the rays of the calorific source, at a constant temperature, have reached the anterior face of the pile through the aperture in the receptacle. The motion of the needles is at first very slow, but becomes gradually accelerated, and, having attained its maximum of velocity, is again retarded by imperceptible gradations, until at last it ceases altogether: the needles then return gently towards zero, describe an arc of some degrees, resume the direction of the first motion, and, after having made three or four oscillations successively decreasing in extent, stand still and take a fixed position. Now this steady deviation is always a little inferior to the deviation indicated by the needles at their first departure from the point of repose. The difference varies with the amplitude of the primitive deviation, which we shall call the *arc of impulsion*; and we perceive that, the slowness of the motion at the extremity of this arc enabling us to observe it with considerable exactness, it may be afterwards compared with the corresponding steady indication; which comparison may be easily extended to all points of the circuit if we vary the intensity of the calorific radiation by making the requisite change in the distance between the source and the pile. Moreover, the fixed deviations being given, the corresponding *forces* may always be determined by experiment*. We are therefore in possession of all the elements necessary for the construction of such a table as may immediately show the ratios of the forces according to the arcs of impulsion. The forces, as we know, represent the temperatures†: thus, by means of our table, the relative intensities of two ca-

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* For the description of the methods, see *Bibliothèque Universelle*, tom. lv. page 9; and *Mémoires de l'Académie des Sciences*, tom. xiv. p. 445 and 446.

† M. Becquerel had shown in 1826 (*Ann. de Chimie et de Physique*, tom. xxxi. page 371) that the intensities of the thermoelectric currents of copper, platina, and other metals, are proportional to the temperatures through the whole extent of the thermometric scale. Now, the currents which produce the greatest possible deviation in the common thermomultipliers are derived from a heat, which scarcely rises to a few degrees, acting on one of their faces: the proportionality between the forces of magnetic deviation and the temperatures was therefore already established by experiment when I was commencing my inquiries into the nature of radiant heat. Hence it is that I have admitted that proportionality, as a known fact, in the preceding Memoirs on this subject. Nevertheless, as M. Becquerel had not operated directly on the metals which enter into the composition of the pile, the committee appointed by the Academy of Sciences to examine my experiments on heat, manifested a desire that the proportionality of the forces to the temperatures in the thermomultiplier itself should be placed beyond the reach of doubt by some special experiments. With this view I procured a thermoelectric pile of four very minute elements (bismuth and antimony) bent like a siphon, in order that each of the two terminating faces might be introduced into a separate recipient and different temperatures imparted to them by the contact of heated liquids. The extremities of the two last elements stood out of the vessels into which the ends of the curved electric bundle had been plunged, and communicated with the galvanometer by two copper wires. But as a difference of some degrees was sufficient to drive the magnetic needles to the extremity of the scale, I placed in the electric circuit a very fine iron wire of several feet in length. The current then became so weak that a variation of a centigrade degree of temperature between the two faces produced in the galvanometer no more than a deviation of about one degree. Matters being now in this state, water more or less heated was successively introduced into one of the vessels and thawing ice into the other. The second face was thus kept constantly at zero, while the first took, in succession, the different temperatures of the water, which were determined by a very delicate mercurial thermometer. The numbers of the degrees indicated by the thermometer plunged in the hot water were found exactly proportional to the corresponding electric forces or intensities indicated by the deviations of the galvanometer. The experiment was now varied in order to obtain a nearer approach to the circumstances in which the thermomultiplier is commonly em-

lorific radiations will be obtained by the mere inspection of the two arcs of impulsion which they successively describe on the galvanometer. The time required by the needles to arrive at the extremity of these arcs is from ten to twelve seconds: they do not remain steady until after an interval of from eighty to a hundred seconds. Now the sources of

ployed. That branch of the pile which was surrounded with ice was carefully dried and then left exposed to the free action of the air, the other remaining still plunged in the water successively raised to different temperatures. In this instance the intensities of the electric currents became proportional to the excess of the temperature of the water in the vessel over that of the surrounding air; for the conducting powers of the bismuth and the antimony in minute bars are so very feeble that the heat communicated by the water to one of the faces can scarcely reach the other in any such quantity as to excite in it an appreciable elevation of temperature.

Although these experiments were repeated with equal success in different atmospheric temperatures, I did not yet consider them perfectly satisfactory. In fact, the pile received by contact the differences of temperature which produce the electric currents, and in the ordinary mode of using the thermomultiplier the differences of temperature arise from the action of radiation. It became necessary therefore to demonstrate, by means of radiant heat, that which had been proved by means of heat communicated by contact. After the preceding experiments, the only question now remaining was this: "*Whether calorific rays produce in thermoscopic substances equal dilatations, when they excite in the thermomultiplier equal currents of electricity, whatever may be the intensities and origin of those rays or the modifications they may have undergone in consequence of transmission, reflection, or refraction.*" In order to ascertain how far this question might be correctly answered in the affirmative, I engaged Mr. Bunten to construct an air thermoscope having its reservoirs made of thin copper, and its dimensions and mounting, as nearly as possible, the same as those of the pile of the thermomultiplier. The communications between the reservoirs were established by means of a glass tube, which at first descended very obliquely on each side to the horizon, and then took a horizontal direction in the intermediate part containing the liquid index: the extreme faces inclosed in the metallic appendages were, as well as the interior of these appendages, covered with lamp-black: the instrument was lastly furnished with a stem of the same thickness as that of the pile, so that the same stand might serve for either. I now opened the two tubes of the pile of the thermomultiplier, and brought the radiation of a different source to bear on each of its faces; for instance, that of the Locatelli on the one, and that of copper heated to 400° on the other: I then placed the weaker of the two sources more or less near until the index of the galvanometer stood fixed at zero. After having thus obtained two opposite calorific actions producing electric currents of the same force, I removed the pile and put the air thermoscope in its place. The same immobility was exhibited by the *liquid* index. This delicate experiment was repeated with the greatest care on different species of radiations; at first on those that were direct and rendered more or less intense by a suitable approximation of the sources; then on those that were transmitted through plates of different kinds or concentrated by means of lenses; and lastly upon the heat given back by reflectors. The result was always the same; namely, that all calorific radiations whatsoever, if they excite in the pile equal currents of electricity or equal electromagnetic actions, will also produce equal dilatations or temperatures in the air thermoscope. (For further details see the report of M. Biot in the xivth volume of *Mémoires de l'Académie des Sciences.*)

eat, however constant, are subject to slight changes in their physical state, which cause variations of the same order in the temperature; so that it is always desirable to abridge the time that elapses between two comparative experiments. The table which I have just mentioned will therefore render the observations more prompt and more exact than they would be if we directly observed the fixed deviations. I have accordingly always employed this method in the course of this Memoir, and the force corresponding to each result will be found beside the observed arc of impulsion. It is almost unnecessary to add, that the force to which all the others are referred is that which causes the needles to describe the first degree of the scale.

Having by various contrivances provided myself with a thermoscopic instrument of very great sensibility, promptitude, and certainty in its indications, I proceeded to the experiments on polarization by commencing with tourmalines.

The great difficulty that first presents itself in studying the polarization of heat by tourmalines, is the feeble calorific transmission of these substances; a circumstance which, together with the usual smallness of their dimensions, renders the intensity of the emergent rays extremely feeble, and scarcely appreciable with the most delicate thermomultipliers. Hence it becomes necessary to bring the source very close to the system of the tourmalines, in order that they may receive the greatest possible quantity of the calorific rays. But this extreme proximity of the source heats the tourmalines in a sensible degree, causes them to radiate on the pile, and, by the effect of this secondary heat, disturbs the action of the rays immediately transmitted by the system.

The quantity of incident heat might indeed be augmented, without a change in the ordinary distance of the source, by concentrating it on the tourmalines by means of a rock-salt lens. But then the plates become still more heated, and the pile must necessarily be placed at a great distance behind the tourmalines, in order to withdraw it from the disturbing force of this second calorific source. Now the distance of the pile from the tourmalines cannot be thus increased without subjecting us again to that very inconvenience of an over-feeble radiation, which it is our purpose to avoid; for the rays, after having crossed each other in the focus, undergo a considerable divergence and a rapid decrease of intensity as they proceed to a distance from the plates. In order to avoid both these inconveniences, and to obtain a calorific stream consisting solely of the rays directly transmitted by the tourmalines and yet powerfully acting on the thermoscope, I first receive the pencil of calorific rays on a large rock-salt lens, after having made them parallel by means of a reflector. The concentrated heat reaches the tourmalines; a great portion of it is absorbed, and converted into ordinary heat; the rest pursues its way without losing its radiating state, is afterwards dispersed, and falls upon a second lens with a shorter focus, placed beyond the first at a distance precisely equal to its own principal focal distance. The rays received by this second lens in a state of divergence are parallel to each other when they leave it; and form a pencil of condensed heat, which enters the thermoscopic case, and finally reaches the pile, which stands at a suitable distance from the aperture. The section of the pencil being a little less than that of the pile, all its parts concur in the production of the thermoscopic effect, and thus we lose the calorific effect of none of the rays that issue from the polarizing system.

It is very important to observe, that the common centre of the two superposed plates of tourmaline is not placed exactly in the common focus of the two conjugate lenses, but a little nearer to the second, in order that the portion of heat absorbed by those plates, and radiated on the second lens, may be necessarily refracted in diverging rays whose action becomes weaker, and is completely destroyed at a short distance, without influencing the thermoscopic body, which is thus affected only by the heat arising from direct transmission. That this condition is actually fulfilled may be ascertained by blackening the tourmalines, or by substituting for them any other plates well covered with lampblack; for the index of the galvanometer reassumes its natural position in equilibrio, and retains it whether the communication with the calorific source be established or intercepted.

By this simple contrivance we cause very minute plates of tourmaline to transmit a bundle of rays almost as broad as the surface of the first lens, and then bring *all* the emergent rays, and these *alone*, pure, and unmixed with even the smallest particle of the caloric derived from the heating of the plates, to produce their effect on the thermoscope.

Combining a lens of two inches and a half in diameter and three inches in the focus, with a lens of fourteen lines, I obtain a quantity of rays emerging from the tourmalines, such as, in several instances, produces a deviation of the needles, amounting to between 60° and 80°, at the distance of a metre from the small flame of a Locatelli lamp furnished with a reflector. This energetic action, though necessary for the experiments which I had in view, is then too great; but there is an easy way of reducing it as much as we wish: we have only to render the rays more or less divergent by a proper approximation of the lenses.

The plates of tourmaline are adjusted to the central part of two parallel covers of cork filling the interior of a round box, which is sufficiently shallow, has a small circular aperture at the centre, and is supported at the proper height by a metallic screen with a similar aperture.

One of the plates is fixed, the other is moveable together with that half of the box in which it is contained. Marks traced on their edges enable us to distinguish with ease the two principal positions of the axes of crystallization.

I exhibit here in a single table the results which I obtained by operating on nine pairs of tourmalines borrowed from different individuals. All these pairs polarized luminous rays almost completely; that is to say, that to a person looking at the flame of a wax taper through each of these systems, it appeared sufficiently vivid and brilliant, so long as the axes were parallel; but the image became nearly extinct when the axes were perpendicular.

TABLE I.

Source of Heat, the flame of a Locatelli lamp.

Numbers indicating the order.	Colour of each pair of tourmalines.	Calorific transmissions in the position of the axes.				Indices of polarization in hundredth parts of the quantity of heat transmitted when the axes are parallel.
		Parallel.		Perpendicular.		
		Arcs of impulsion.	Forces.	Arcs of impulsion.	Forces.	
		°		°		
1	Deep green	30·56	27·50	29·78	26·48	3·71
2	Bluish green	29·81	26·51	28·22	24·60	7·20
3	Blue green	32·35	29·40	30·11	26·90	8·50
4	Yellowish green	31·42	28·51	29·32	25·89	9·19
5	Yellowish green	33·23	30·18	30·01	26·77	11·30
6	Yellow green	31·96	29·07	29·11	25·61	11·90
7	Reddish brown	29·89	26·62	25·32	21·88	17·72
8	Muddy violet	30·69	27·67	25·45	22·00	20·48
9	Pale yellow	31·27	28·37	25·60	22·16	21·89

After what we have already stated, the numbers in the four columns which precede the last require no explanation. I have therefore only to observe, that each number under the head "*arcs of impulsion*" represents the mean of several observations, made alternately in the parallel and in the perpendicular positions of the axes; that is to say, that the arc described in consequence of the transmission through the plates with their axes parallel was first observed, and then the arc described in consequence of the transmission through the same tourmalines with their axes perpendicular. The observations on this pair, as well as those on each of the remaining pairs in each of the two positions, were several times repeated. This method affords a compensation for any possible errors of observation, as well as for those that may arise from the slight variations of intensity to which the radiation of the calorific source is liable, and which, owing to the perfection of the apparatus, do not, even in their extreme limits, exceed a fiftieth part of the mean value. As to the last column, we mean by the index of polarization that portion of heat which disappears when the axes are perpendicular, as compared with the quantity transmitted by the system when the axes are parallel. Thus, the first pair of tourmalines transmits 27·50 when the axes are parallel, and 26·48 when they are perpendicular: the difference between these two numbers, which is 1·02, represents the quantity of heat that has disappeared in consequence of the axes being crossed. In order to obtain the index of calorific polarization in this pair of tourmalines, expressed in hundredth parts of the quantity transmitted when the axes are parallel, we must evidently resort to the following proportion, 26·48 : 1·02 : : 100 : x, which gives $x = 3·71$.

The column of the indices shows that the proportion of heat polarized varies with the qualities of the tourmalines which compose each pair of plates employed. These variations, already numerous enough, if we consider the number of pairs submitted to experiment, led me to think that they might be found yet more numerous with other plates, and that they depended very probably on the diathermancy of each species of tourmaline, that is, that they arose from these different species of the same mineral substance being each permeable to a differently constituted calorific stream. In order to verify this conjecture, I placed on the apparatus that pair which polarized the greatest proportion of heat; and, after having made such arrangements as to render the quantity of heat transmitted as great as possible, I successively interposed, in the passage of the rays concentrated by the first lens, plates of different substances. The heat which fell on the tourmalines was thus more or less diminished by the partial absorption of the interposed screen; but I took care to make a suitable change in the reciprocal distance of the two rock-salt lenses, in order to obtain an almost constant calorific transmission through these different systems when the axes of the two tourmalines were parallel.

The results of this second series of experiments, performed on rays emanating from the same source, and with the same pair of tourmalines, are registered in the following table:

TABLE II. *Source of Heat, flame of a Locatelli lamp.*

NAMES of the substances interposed before the introduction of the calorific radiation into the tourmalines.	Thickness of the layers formed by these substances, reckoned in millimetres.	Calorific transmissions through each interposed layer, and the same pair of tourmalines (No. 9 of the preceding table) in the position of the axes.				Indices of polarization in hundredth parts of the quantity of heat transmitted by the system of the three plates when the axes of the tourmalines are parallel.
		Parallel.		Perpendicular.		
		Arcs of impulsion.	Forces.	Arcs of impulsion.	Forces.	
	mm	°		°		
No screen	0·00	17·37	15·06	13·47	11·76	21·91
Glass colourless	1·85	17·93	15·53	13·94	12·15	21·79
—— coloured (red) ...	1·80	16·75	14·54	13·04	11·40	21·57
———— (orange)	1·87	17·21	14·93	13·31	11·66	21·90
———— (yellow)	1·79	17·83	15·45	13·84	12·07	21·89
———— (blue)...	1·83	17·59	15·24	13·66	11·92	21·78
———— (indigo)	1·78	17·29	14·99	13·44	11·74	21·68
———— (violet)	1·81	16·81	14·59	13·02	11·39	21·92
Glass coloured (bluish green[*])	0·74	16·99	14·74	15·95	13·86	5·95
Ditto	1·93	17·32	15·02	16·85	14·62	2·76
Glass (opake black) ...	0·81	17·55	15·21	16·76	14·55	4·35
Ditto	1·98	17·80	15·42	17·52	15·19	1·51
Sulphate of barytes ...	2·60	17·10	14·83	13·18	11·52	22·30
———— of lime	2·71	16·95	14·71	10·54	9·18	37·63
Oil of colza...............	8·49	16·97	14·72	10·40	9·05	38·50
Tartrate of potash and soda	2·50	17·39	15·08	9·49	8·26	45·21
Water saturated with salt[†]	8·49	17·49	15·16	5·78	5·06	66·60
———— with alum	8·49	17·56	15·22	5·81	5·08	66·63
———— with tartaric acid	8·49	17·39	15·03	5·76	5·04	66·59
Ditto	0·74	16·96	14·72	10·76	9·38	36·31
Water (distilled)	8·49	16·77	14·55	5·54	4·85	66·67
Ditto	0·74	17·20	14·92	10·91	9·50	36·27
Amber (yellow).........	3·08	17·23	14·94	8·35	7·29	51·23
Alum	2·58	16·98	14·73	0·58	0·52	95·81

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[*] The physical characters of this species of glass, which acts so differently from the other species of coloured glass in all the phænomena of calorific absorption, are, 1st, its intercepting almost totally the rays which pass through alum; 2nd its entirely absorbing the red rays of the solar spectrum. I have already stated that their coloration is produced almost entirely by the oxide of copper.

[†] The temperature of these different saturated solutions was about 15°.

We were already aware that the rays immediately transmitted through bodies differing in their nature pass in very different proportions through a given plate of a diathermanous substance*: we were also aware that those rays are differently absorbed by the surfaces of certain opake bodies†. To these distinctive characteristics we are now enabled to add, that they undergo in the same system of tourmalines an apparent variable polarization.

We see, in fact, that of every hundred rays of heat transmitted by the tourmalines when they are placed with their axes parallel, about 22 are made to disappear by merely crossing the axes. This proportion suffers no very decided change in the rays transmitted by the common, the red, the orange, the yellow, the blue, the indigo, and the violet glass; but is reduced to $\frac{2}{100}$ or $\frac{3}{100}$ when we employ green or opake-black glass; and when we employ sulphate of lime, yellow amber, water either pure or saturated with salts, and alum, the quantity of heat polarized amounts successively to $\frac{23}{100}$, $\frac{38}{100}$, $\frac{51}{100}$, $\frac{67}{100}$, and $\frac{96}{100}$.

It is a fact worthy of remark, that the character derived from the index of polarization leads to the same consequences that we have deduced from the experiments of transmission. Indeed, the latter analytical process had authorized us to admit that the colouring matter introduced into the composition of coloured glass merely extinguishes a part of the calorific stream transmitted by the colourless glass, without sensibly affecting the proportions which the groups of rays composing that stream bear to each other in respect to quantity; so that the effect produced by that matter relatively to radiant heat is analogous to that which would be produced relatively to light by brown or blackish substances diluted in a liquid having no chemical influence on those substances‡. Now, since the proportion of heat polarized by the tourmalines varies with the quality of the calorific rays transmitted by the different screens, the constancy of this proportion between the rays which issue from the coloured and those which issue from the uncoloured glass clearly shows, as in the experiments of transmission, that the colouring matters do not affect the composition of the calorific stream transmitted by the glass. True, the green and the opake-black glasses furnish a very marked exception; but the experiments of transmission furnish an exception completely analogous§.

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* *Ann. de Chim. et de Phys.*, tom. lv. p. 384.

† *Id.*, p. 388.

‡ *Id.*, p. 381.

§ The same consequences are derived from the experiments of refraction. With this view, one of the faces of the refracting angle of a rock-salt prism is covered with a plate of coloured glass, and the distribution of temperature in the bands of the spectrum produced by exposing this system to the light of the sun is then observed. If we change the colour of the glass, we not only find the

We have found by transmission that the rays emerging from the green and opake-black glasses may be said to possess properties diametrically opposite to those of the rays issuing from alum*. The same antagonism of properties is manifested with respect to the apparent polarization which these two species of heat undergo in their passage through the tourmalines; for in the one the index of polarization increases three- or four-fold, while in the other it suffers a diminution of eight or nine tenths.

In fine, experiment has shown that the calorific rays immediately transmitted by alum approximate closely to the luminous rays, both in their abundant transmission through all uncoloured diaphanous substances

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form of the calorific spectrum preserved with great regularity, as we have seen elsewhere (*Ann. de Chim. et de Phys.*, tom. lx. p. 426.; Scientific Memoirs, vol. i. part i.), that is to say, possessing one maximum, and lower temperatures regularly decreasing on each side of it, but we see that the distances from this maximum and the surrounding bands to a given zone of the luminous normal spectrum remain sensibly invariable. As to the absolute quantity of heat, it varies considerably with the tint and nature of the glass; but this variation is always proportional to the value of the ordinates which represent the temperatures of the different zones for any one of the coloured plates; so that the intensities of the maximum and the adjacent bands are more or less affected in a constant ratio through the whole extent of each new spectrum produced by changing the glass. From these two facts it clearly follows that the quality of the calorific stream transmitted by the different plates of coloured glass does not vary in its passage from one plate to another. In this however, as in the other analyses that we have made of this phænomenon, the green glass possessing the qualities already mentioned presents a very striking exception: for this species of glass displaces the calorific spectrum and throws it, in the direction of the inferior refraction, almost totally beyond those limits that are common to the spectra produced by all the other species of coloured glass.

When several different methods (and the processes of absorption, polarization, and refraction described here and elsewhere are really such,) lead to one and the same conclusion, it seems to me that the conclusion is sufficiently secure to be ranked among truths firmly established by experiment.

Thus the colouring matters of the coloured glasses, while they so powerfully affect the relations of quantity which the different rays of ordinary light bear to each other, exercise no elective action on the concomitant calorific rays. This curious phænomenon is the more remarkable, as the same colouring matters absorb, almost always, a very considerable portion of the heat *naturally transmitted by the glass.* The following are, in fact, the calorific transmissions of the seven coloured glasses referred to the transmission of the colourless glass which is represented by 100: Red glass 82·5, Orange 72·5, Yellow 55, Bluish green 57·5, Blue 52·5, Indigo 30, Violet 85. The quantity of heat absorbed through the action of the colouring substances is therefore 17·5 in the red glass, 27·5 in the orange, 45 in the yellow, 42·5 in the green, 47·5 in the blue, 70 in the indigo, and 15 in the violet. Now, as these absorptions extinguish a proportional part of each of the rays which constitute the calorific stream transmitted by common glass, they may be compared, as we said before, with the absorbent action exercised on light by matters more or less deeply brown or dark when they are immersed in water or some other colourless liquid which dissolves but does not affect them chemically.

* *Ann. de Chim. et de Phys.*, tom. lv. p. 382.

and the feeble absorption which they suffer from white surfaces*; and this analogy is completed here by the almost total polarization of the same rays under the influence of the tourmalines.

It will now be easy to account for the differences between the indices of polarization produced by the different tourmalines. All the calorific rays emitted by the flame of the lamp do not indiscriminately pass through the tourmalines, each of which, according to its nature, is permeable to particular quantities and qualities of heat. This fact, which is observable in diathermanous substances in general, is so true in the particular instance under consideration, that each of the plates composing the polarizing system indicated in the first table by the numbers 1, 2, 3, 4, when combined with a plate of alum, fails to afford an appreciable transmission; an evident proof that the heat which alum is capable of transmitting is not to be found in the calorific stream emerging from these four systems. Now we have just seen that the different species of heat contained in the radiation of flame give very different indices of polarization. The calorific stream admitted by each polarizing pair will therefore necessarily have a mean index of polarization varying with the quality of the tourmalines.

If we place outside the polarizing system a screen indued with the same diathermancy as the plates composing this system, namely, a screen permeable to the same species and the same portions of calorific rays, then the effect of absolute transmission will, no doubt, be more or less diminished in proportion as this screen is more or less diathermanous, but the tourmalines will present no change in their index of polarization: such is the case with respect to the white, the red, the orange, the yellow, the blue, the indigo, and the violet glass. Water, oil, amber, alum, green, or opake-black glass affect this index in a greater or less degree, because their diathermancy differs from that of the tourmalines employed.

But let the polarizing system be changed. It is clear that, if the new system has not the same diathermancy, the order and the direction of the variations produced by the different screens in the value of the index of polarization will be no longer the same. The following is in fact a series of observations made on the pair of green tourmalines marked No. 5 in the first table:

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* *Ann. de Chim. et de Phys.*, p. 390.

TABLE III.

Source of heat, the flame of a Locatelli lamp.

NAMES of the substances interposed before the introduction of the calorific radiation into the tourmalines.	Thickness of the layers formed by these substances, reckoned in millimetres.	Calorific transmissions through each interposed layer and the same pair of tourmalines (No. 5 in the first table) in the position of the axes.				Indices of polarization in hundredth parts of the quantity of heat transmitted by the system of three plates when the axes of the tourmalines are parallel.
		Parallel.		Perpendicular.		
		Arcs of impulsion.	Forces.	Arcs of impulsion.	Forces.	
	mm	°		°		
No screen	0·00	17·11	14·84	15·15	13·15	11·35
Glass blueish green	1·93	17·65	15·30	15·54	13·49	11·83
—— opake black	1·98	17·10	14·83	15·06	13·05	11·94
Sulphate of Barytes	2·60	17·33	15·03	15·23	13·21	12·07
Oil of colza	8·49	17·52	15·19	12·95	12·80	15·65
Sulphate of Lime	2·71	17·76	15·39	12·74	12·63	17·91
Glass uncoloured	1·85	17·27	15·08	16·24	14·11	6·46
Ditto	8·27	17·81	15·43	17·05	14·79	4·17
Glass coloured (red)	1·80	17·49	15·16	16·32	14·17	6·53
———— (orange)	1·87	16·91	14·67	15·77	13·69	6·70
———— (yellow)	1·79	17·22	14·93	16·12	14·00	6·15
———— (blue)	1·83	16·87	14·64	15·81	13·73	6·20
———— (indigo)	1·78	16·98	14·73	15·86	13·78	6·44
———— (violet)	1·81	17·30	15·00	16·20	14·06	6·29

It is to be observed that the extreme limits of the variations produced in the index by the interposition of the screens differ considerably less than they did relatively to the pale yellow tourmalines; a circumstance which indicates a greater homogeneity in the calorific stream transmitted by the tourmalines actually employed. Moreover, the index of polarization undergoes but a very slight alteration under the influence of the green and the opake-black glasses, which produced a reduction of between twelve and nineteen twentieths in the direct index of the table that precedes the above. The diathermancy of these green tourmalines is therefore analogous to that of the green and the opake-black glasses.

As to the white, red, orange, yellow, blue, indigo, and violet glasses, they diminish the index of polarization instead of leaving it in its natural state, as in the instance of the pale yellow tourmalines. In this there is nothing that should surprise us, since the difference of diathermancy proper to the two polarizing systems causes these uncoloured and coloured glasses to act in this case like the green and opake-black glasses of the preceding table, and *vice versâ.*

Moreover, the white, red, orange, yellow, blue, indigo, and violet glasses all produce very nearly the same alteration in the value of the index of polarization of the green tourmalines: and we have just observed that these glasses have a very marked effect on the pale yellow tourmalines. This effect, varying in different systems, but constant in each particular system through the whole series of plates, is perfectly analogous to the uniformity of the ratios which, notwithstanding the changes of intensity, are observed to exist between the quantities of heat transmitted by the same glasses (coloured and uncoloured) when successively exposed to the rays emerging from the several kinds of screens*. We are thus brought back once more to one of the foregoing conclusions, namely, that the colouring substances have no power of elective absorption with respect to the rays of the calorific stream which passes through the glass.

Before we conclude our observations on the effect produced by the screens in the index of polarization of the tourmalines we shall make some remarks on the effects produced by the variation of thickness in the interposed substance and by the solution of salts in water.

On inspecting the second table it is easy to see that the influence of each substance in augmenting or diminishing the index of polarization is more powerful in proportion to the greater thickness of the substance. Thus, water reduced to a layer of $0^{mm}\cdot74$ in thickness causes the index of the pale yellow tourmalines to rise from 22 to 36, while a layer of 8^{mm} raises it to 67. On the other hand, a plate of dark glass $0^{mm}\cdot81$ in thickness, which causes the same index to descend from 22 to 4, would reduce it even to 1·5 if its thickness were about 2^{mm}. All this accords with the experiments of successive transmission, which show that the calorific stream emerging from a given substance becomes more simple, or, if the expression be preferred, more purified in proportion to the greater thickness of the substance through which it has passed. We find this to be the case with white light also in penetrating coloured media.

Tartaric acid, rock salt, and alum dissolved to saturation in water make no sensible change in its action on the index of polarization of the tourmalines. We have already shown, in a preceding memoir, that alum and rock salt (which, of all perfectly diaphanous and colourless substances, are those that possess the maximum and the minimum of diathermancy) do not by being dissolved in water affect the diathermanous power of this liquid*. The sensible equality of the action of these solutions and that of pure water on the natural index of polarization of the tour-

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* *Ann. de Chim. et de Phys.*, tom. lv. p. 55.

malines enables us to advance a step further; for this equality shows that not only the quantity but the *quality* of the heat transmitted by pure water is the same as the quality of that transmitted by water saturated with salt or alum. In short, if we receive on the pile the calorific streams which issue from distilled water, and the solutions of rock salt and alum, we shall find the deviations in the galvanometer very nearly equal in the three cases. It is to be recollected, however, that this will happen only when the three layers from which the rays issue are of the same thickness. Now this invariability of action through layers of equal thickness takes place also if we interpose the same plate of alum or any other substance behind each of the liquid layers in succession; for the common deviation is always diminished by a constant quantity, even when, by concentrating the calorific radiation with lenses, we have raised to 35° or 40° the arc of impulsion described solely under the action of the stream transmitted by each of the liquid layers.

In order to conclude the experimental study of the calorific polarization of the tourmalines, we have only to compare with one another the polarizing action of these crystallized substances on the radiations of different sources of heat. For this purpose I select the four systems marked Nos. 1, 5, 8, and 9 in the first table, and these being exposed, in the two principal directions of the axes, to the calorific radiations of an Argand lamp, a Locatelli lamp, a spiral of incandescent platina, and a plate of copper heated to 400°, give the indices of polarization contained in the following table.

TABLE IV.

Numbers indicating the order of the tourmalines in the first table.	Colour of each pair of tourmalines.	Indices of polarization for the direct radiations			
		of the Argand lamp with a glass funnel.	of the Locatelli lamp.	of the platina kept in a state of incandescence by the flame of alcohol.	of a plate of copper heated to about 400° C.
1	Deep green	0·37	3·71	5·27	0·59
5	Yellowish green......	5·33	11·30	13·89	3·22
8	Muddy violet	24·50	20·48	17·20	2·30
9	Pale yellow	26·21	21·89	18·16	2·98

N.B. In this table the arcs of impulsion are omitted in order to avoid excessive complexity. We think it necessary, however, to observe that these were often more extensive than those of the preceding tables; and this was indeed absolutely necessary in order to perceive those indices which in many instances are extremely feeble. No. 1, for example, being exposed to the radiation of the

Let us first direct our attention to the two last systems of tourmalines. Their indices undergo a gradual increase in passing from the copper to the Argand lamp. This shows that the radiation of each of the four sources contains a greater quantity of heat capable of being polarized by the tourmalines in proportion as the temperature of the source is more elevated.

Yet the indices of polarization of the systems 1 and 5 undergo, from the action of the first three sources, changes completely opposed to those which we have been considering. In order to account for such an anomaly, we must keep in view the very imperfect diathermancy of this sort of tourmalines. It is true that the calorific streams of the Argand and the Locatelli lamps contain rays more capable of being polarized by the tourmalines than any of those contained in the calorific streams from the inferior sources; but in the present case such rays scarcely contribute to increase the index of polarization; for we have seen that they cannot penetrate the plates which form the polarizing systems. The green tourmalines, however, are permeable to several species of heat; and, as in the radiation of each source there are several of these species, it is easy to see that if a certain group of rays, possessing an index of polarization inferior to those of the excluded but superior to those of the transmitted rays, is more abundant in the calorific stream of the incandescent platina than in that of the Locatelli, the index of polarization in the two systems of green tourmalines will, in this case, suffer a decrease in passing from the first to the second source. The same reasoning will apply to the Locatelli as compared with the Argand lamp; so that, notwithstanding the fact that the rays are more susceptible of polarization as we proceed from the incandescent platina to the higher sources, the two systems of green tourmalines will give lower indices of polarization.

Without a knowledge of the laws of calorific transmission and the analytical resources they afford, we should perhaps find it impossible to extricate ourselves from the perplexing difficulty presented by these singularly anomalous phænomena of polarization. We are now able to offer the following brief recapitulation of our observations on them.

"The different calorific rays coexisting in the radiation of the same

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Argand lamp, gives but a difference of 0°·1 in the two positions of the axes, the arcs of impulsion being from 26° to 27°; a difference which could not be rendered perceptible otherwise than by taking the mean of 10 observations; and I am not yet quite sure that it would not vanish altogether if the experiments were more numerous, for I frequently obtained a stronger transmission with the axes perpendicular. The fact is that in operating upon arcs of from 15° to 20° the transmission of this system of tourmalines seemed to undergo no variation whatsoever in consequence of the axes being crossed; and indeed, after the preceding experiments, there is nothing more surprising in the existence of tourmalines that give no sign of calorific polarization than there would be in the discovery of tourmalines capable of completely polarizing heat.

source of heat, or emitted by different sources, are very unequally affected by the cause through which the phænomena of polarization in the tourmalines are rendered sensible. Some of them appear to undergo no action of this kind, others produce indices of polarization more or less marked, and others are, like rays of light, completely polarized. Tourmalines in general, and the green tourmalines in particular, absorb the most polarizable rays, and transmit those species which seem totally or partially to escape polarizing action. The consequence is, that their apparent index of polarization is generally very feeble and sometimes even inappreciable. But it rises even to $\frac{22}{100}$, and perhaps higher, in those systems of plates which are permeable to a greater proportion of heat susceptible of a high degree of polarization, as we see in the plates of yellow, brown, or violet tourmalines. The index of apparent polarization in a given system varies considerably in passing from one source to the other, because a change takes place in the quality and the grouping of the rays constituting the calorific stream issuing from the focus of heat. In fine, this index varies, and in certain cases attains its two extreme limits, 0 and 100, when we introduce between the same source and the same system of tourmalines diathermanous plates of a different kind; because the particular absorption of these screens affects the relations of quantity existing between the several groups of rays composing the calorific stream naturally transmitted by the polarizing system."

In all these statements we have taken care to apply the qualifying term *apparent* to the signs of feeble polarization exhibited by the tourmalines; and, in fact, all the rays of heat, whether direct or transmitted through a screen, might be completely polarized, as light is, in the interior of these crystallized bodies, and yet the polarization not be rendered perceptible by any diminution in the quantity of heat transmitted by the plates when the parallel is exchanged for the perpendicular position of the axes.

In order to understand this proposition it is necessary to recollect the phænomena which take place in the polarization of light by tourmalines.

When a ray of natural light falls perpendicularly on a plate of tourmaline cut parallel to the axis of the needles, the double refraction first divides the ray into pencils possessing sensibly equal intensities and polarized at right angles; but, in proportion as these pencils penetrate into the substance of the tourmaline, they suffer very different degrees of absorption, that of the pencil which has undergone the ordinary refraction being much the greater; so that, beyond a depth often very inconsiderable, one of the pencils is entirely absorbed, while the other pursues its path, emerges from the plate, and shows itself in its proper direction of polarization. This inequality of absorption is proved by the following experiment, for which we are indebted to M. Biot. We take a sufficiently thick plate of tourmaline cut parallel to the axis, and attenuate it obliquely at one side, so that the planes of its two faces may intersect each other exactly on one of the edges, and thus form a certain angle. A very narrow slip of white paper, or any other object equally minute, if viewed across this angle and in the direction of the edge, will give two images neither lying one upon the other nor confounded together, as they are when the faces are parallel, but separated by the double refraction of the tourmaline. If we achromatize the refringent angle by means of a glass prism in order to have a clearer view, we find that these two images, viewed through the thinnest part of the tourmaline, are nearly of the same intensity; but by passing the thicker parts successively before the eye, we perceive the image formed by the ordinary refraction becoming gradually weaker and weaker until it is finally extinguished.

Thus we see that it is in consequence of the *unequal* absorption of the two pencils formed by double refraction that the polarization in a plate of tourmaline becomes perceptible. If the absorbency of the matter of which the plate is composed acted with the same intensity on each of them, the two pencils would emerge intermixed, and exhibit all the properties of ordinary light; so that a second plate of tourmaline would no longer, by having its axis placed transversely to that of the first, produce any diminution of intensity in the light transmitted.

Let us now apply these notions to calorific polarization. Let us suppose that all the rays of heat, like those of light, undergo a complete polarization as soon as they enter a plate of tourmaline, and that each of them is consequently divided into two pencils or bundles possessing equal intensities and polarized at right angles. Let us admit, besides, that the inequality of absorption effected by the matter of the tourmaline in the two pencils varies with the different calorific rays; that it is very great with respect to some rays, and little or none with respect to others: it is evident that the former will issue from the tourmaline entirely polarized in one plane, while the latter will be more or less polarized in the two planes standing at right angles to one another, and will therefore present little or no appearance of polarization.

All the facts which we have stated may then be explained on the hypothesis of a complete polarization of the calorific rays; and we shall see, indeed, that this hypothesis is rendered more and more probable, nay, certain, by the following experiments. But before we conclude our observations on this part of the subject, it will not, perhaps, be useless to set in contrast, by means of an example easily to be comprehended, the two different effects which the tourmalines have on the rays of light and the rays of heat.

Let us imagine a series of alcohol flames coloured by different salts, and several common flames masked by glasses of different colours. If we look at those lights through our systems of tourmalines with their axes first in the parallel and then in the perpendicular position, all the coloured images which present themselves with sufficient vividness and brilliancy in the first case, completely disappear in the second; or if any of them remains, it is but an excessively feeble gleam*. The rays of platina in a state of incandescence, and of copper heated to 400°, represent, relatively to radiant heat, the coloured alcohol flames; and the rays of a Locatelli lamp, transmitted through water, glass, or alum, are, in respect to this heat, no more than the lights of different colours which we perceive through the coloured glasses. Now we have seen that the effect of the tourmalines on the several species of rays, so far from being equal as in the case of light, presents differences so strongly marked, that sometimes the heat passes in a sensibly equal quantity in all positions of the axes of crystallization, and at others it is almost completely intercepted when the axes of the two plates are perpendicular to each other.

The phænomena of calorific polarization, in a variable proportion, produced by a system of tourmalines which polarizes the luminous rays of all colours equally, are analogous to the extremely marked differences of absorption which the various species of calorific rays undergo in their passage through a sufficiently thin plate of glass, rock crystal, water, alcohol, and almost all perfectly diaphanous substances, the absorbent force of which, within these limits of thickness, if there be any, is the same for all sorts of luminous rays†. Among those actions which vary

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* M. Biot possesses a prism cut out of a tourmaline of a light violet-red colour, which not only does not completely extinguish the common sheaf, as is done by the tourmalines that are too thin or too lightly coloured, but colours the sheaf while it weakens it; so that the two images of a minute object seen through this prism are sensibly white and of equal intensity near the apex of the refringent angle; but, in proportion as the eye moves towards the thickest part, the ordinary image is observed to decrease in its intensity and to take at the same time a red tint which becomes gradually deeper, while the extraordinary image never presents itself in any other colour than a slight tinge of the shade which belongs to the tourmaline. It appears then that two flakes of this particular kind of tourmaline would not act alike on all the coloured rays, and that in the perpendicular position of the axes they would still be permeable to red light. Perhaps the red rays themselves would be finally extinguished if the two flakes were of a certain thickness. However, this case is but an exception; for all the other tourmalines constantly produce the effect we have just announced; that is to say, that they extinguish indiscriminately and equally all the luminous rays whatever be their colour, provided they are made to pass through the flakes when their axes are at right angles to each other.

† The experiments on the different degrees in which coloured and uncoloured media absorb the light and heat of the solar spectrum (*Ann. de Chim. et de Phys.*, Dec. 1835, p. 402; Scientific Memoirs, vol. i. part i.) afford a still more striking example of the same kind; for the differences then present themselves in pairs of calorific and luminous rays, which being isolated and, so to speak, purified by by the force of refraction, seem to constitute that species of light and heat which are most identical. And here I shall take the opportunity to correct a strange

considerably with respect to different calorific rays, while they remain sensibly constant with respect to light, we may class the variable absorbent power exercised on the different species of heat by the white surfaces of opake bodies which reflect the same proportion of coloured rays. Such, then, are the conditions to be satisfied henceforth by every theory that will refer the phænomena of light and radiant heat to a single principle.

mistake into which the young and able successor of Leslie has fallen with regard to these experiments. I find in a letter of his addressed to the Editors of the London and Edinburgh Philosophical Magazine (March 1836, p. 245), that the special aim of my labour was to raise objections against the undulatory theory of heat: "*M. Melloni lately read a paper to the Academy of Sciences stating certain objections to the undulatory theory of heat.*") Now such was most assuredly not my intention; and I have, I think, expressed myself to that effect with sufficient clearness in a note at the end of the memoir. In publishing those facts my only object was to show that which I announced at the head of the memoir presented to the Academy, namely, the nonidentity of light and radiant heat, a proposition which is evidently independent of every theory. Thus, whether the system adopted be that of emanation or that of undulations, I do not think it possible to maintain at this day that the same molecule or the same wave that gives, for instance, yellow light, at a determinate part of the solar spectrum, produces the concomitant heat also. Such is the only conclusion that I have drawn from the experiments contained in my memoir. The author of the letter has been probably misled by the enunciation of the proposition, (which I made in terms applicable only to one of the two systems,) as well as by the arguments, which should in my opinion be brought, on the supposition of their identity, to answer the objections derived from the previously known differences between the action of diaphanous and diathermanous media on the light and heat of terrestrial sources; arguments which must necessarily be given in the language of one or the other of the hypotheses on which calorific phænomena are explained. I chose the language of the undulatory system; but I might as well have employed that of the system of emanation. It is besides so true that the arguments contained in the memoir do not apply particularly to the theory of undulations, that by suppressing certain expressions proper to this theory, and changing the words *wave* and *length* into *molecule* and *species*, the arguments are still good, and we thus arrive at the same general conclusion expressed in the language of the system of emission; that is, that in the interior of the solar spectrum the same molecules cannot produce the two effects of light and heat simultaneously. In short, my only aim in pursuing my researches with respect to radiant heat is to study the laws and properties of this agent. I have not the vanity to think that by any new discovery I shall make the partisans of either of the systems tremble. I shall rather tremble myself lest, in consequence of preadopted notions, I may mistake the truth of the phænomena; and I suppose there is no one who will censure a timidity so salutary ... but I will frankly avow that I consider any other fear scarcely philosophical.

SCIENTIFIC MEMOIRS.

VOL. I.—PART III.

Article XV.

Memoir on the Motive Power of Heat; by E. Clapeyron, *Mining Engineer.*

From the *Journal de l'Ecole Royale Polytechnique*; Paris; vol. xiv. p. 153 *et seq.*

§ 1.

Few questions are more worthy of fixing the attention of geometers and natural philosophers than those which relate to the constitution of gases and vapours: the functions they exercise in nature, and the advantages which industry derives from them, account sufficiently for the numerous and important labours of which they have been the object: this vast question, however, is far from being exhausted. The law of Mariotte and that of Gay-Lussac, which establish the relations existing between the volume, the pressure, and the temperature of a given quantity of any gas, have both long since obtained the assent of scientific men. The experiments recently made by MM. Arago and Dulong leave no doubt of the accuracy of the first of those laws within very extended limits of pressure; but these important results give no information respecting the quantity of heat which the gases contain, and which is disengaged by pressure or diminution of temperature,—they do not give the law of the specific calorics when the pressure and the volume are constant. This part of the theory of heat, however, has been the object of profound researches, among which we may cite those of MM. La Roche and Bérard on the specific caloric of gases. Lastly, M. Dulong, in a memoir which he published under the title of *Recherches sur la Chaleur Spécifique des Fluides Elastiques*, has established by experiments free from all objection, that *equal volumes of all elastic fluids at the same temperature and under the same pressure, being suddenly compressed or dilated by the same fraction of their volume, disengage or absorb the same absolute quantity of heat.*

Laplace, and subsequently M. Poisson, have made public some very remarkable theoretical researches on this subject; but they rest upon hypothetical data which appear liable to objection. It is admitted in them that the ratio of the specific caloric when the volume remains constant to the specific caloric under a constant pressure, is invariable, and that the quantities of heat absorbed by gases are proportional to their temperatures.

I will finally quote among the works which have appeared on the theory of heat, one by M. S. Carnot, published in 1824, under the title of *Reflexions sur la Puissance Motrice du Feu.* The idea taken for the basis of his researches appears to me fertile and incontestible: his demonstrations are founded on the absurdity which arises from admitting the possibility of producing absolutely either the motive force or the heat.

The various theorems to which this new method of reasoning conducts us may be enunciated as follows:

1. *When a gas without change of temperature passes from a determined volume and pressure to another volume and pressure equally determined, the quantity of caloric absorbed or lost is always the same, whatever be the nature of the gas subjected to experiment.*

2. *The difference between the specific heat under a constant pressure and the specific heat at a constant volume is the same for all gases.*

3. *When a gas varies in volume without change of temperature, the quantities of heat absorbed or disengaged by that gas are in arithmetical progression, if the increments or diminutions of volume are in geometrical progression.*

This new mode of demonstration appears to me worthy of fixing the attention of geometers; it is, in my opinion, free from every objection, and it has acquired additional importance since its verification by the labours of M. Dulong, in which the truth of the first theorem which I have recited is demonstrated by experiment.

I think it will be of some interest to revive this theory: M. S. Carnot, dispensing with mathematical analysis, arrives, by a series of delicate reasonings difficult to apprehend, at results easily deducible from a more general law, which I shall endeavour to establish. But before entering upon the subject, it will be useful to return to the fundamental axiom upon which the researches of M. Carnot are founded, and which will be my starting point also.

§ II.

It has long been remarked that heat may be employed to develop motive force, and reciprocally that by motive force we may develop heat. In the first case we should observe that there is always a passage of a determinate quantity of caloric from a body at a given temperature to another body at a lower temperature; thus in the steam-engine, the production of the mechanical force is attended by the passage of a part of the heat which is developed by combustion in the furnace, the temperature of which is very high, into the water in the condenser, the temperature of which is much lower.

Reciprocally, it is always possible to render the passage of caloric from a hot to a cold body useful for the production of a mechanical force: to obtain this it is sufficient to construct a machine resembling an ordinary steam-engine, in which the heated body serves to produce steam and the cold one to condense it. It results from this that there is a loss of *vis viva*, of mechanical force, or of quantity of action, whenever immediate contact takes place between two bodies of different temperatures, and heat passes from the one into the other without traversing an intermediate body; therefore in every machine intended to make efficient the motive force developed by heat, there is a loss of power whenever a direct communication of heat takes place between bodies of different temperatures, and consequently the *maximum* of the effect produced cannot be obtained but by means of a machine in which only bodies of equal temperature are brought into contact. Now the knowledge we possess of the theory of gases and vapours shows the possibility of attaining this object.

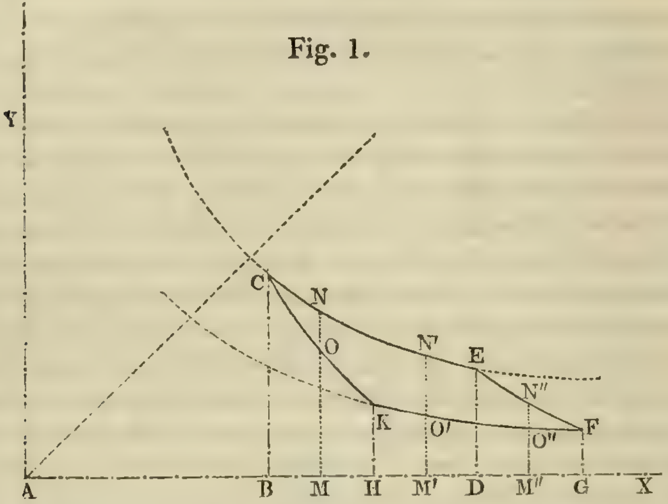
Let us, then, suppose two bodies retained, one at a temperature T, and another at an inferior temperature t; such, for example, as the sides of a steam-boiler, in which the heat developed by combustion constantly supplies the place of that which the steam produced carries away; and the condenser of the common atmospheric engine, in which a current of cold water removes, every moment, both the heat which the steam loses in condensing, and that which belongs to its proper temperature. For the sake of simplicity we will call the first body A and the second B.

Let us now take any gas whatever, at the temperature T, and bring it into contact with the source of heat A, representing its volume v_0 by the absciss A B, and its pressure by the ordinate C B (fig 1).

If the gas is inclosed in an extensible vessel, and which is allowed to extend in a void space in which it cannot lose heat either by radiation or by contact, the source of heat A will supply it, from moment to moment, with the quantity of caloric which its increase of volume renders latent, and it will preserve the same temperature T. Its pressure, on the contrary, will diminish according to the law of Mariotte. The law of this variation may be represented by a curve C E, of which the volumes will be the abscisses, and the corresponding pressures the ordinates.

Supposing the dilatation of the gas to continue until the volume A B has become A D, and that the pressure corresponding to this new volume is D E, the gas during its dilatation will have developed a quantity of mechanical action, which will have for its value the integral of the product of the pressure by the differential of the volume, and which will be represented geometrically by the area comprised between the axis of the abscisses, the two coordinates C B, D E, and the portion of a hyperbola C E.

Fig. 1.



Supposing, again, that the body A is removed and that the dilatation of the gas continues in an inclosure impermeable to heat; then a part of its sensible caloric becoming latent, its temperature will diminish and its pressure will continue to decrease in a more rapid manner and according to an unknown law, which law might be represented geometrically by a curve E F, the abscissæ of which would be the volumes of the gas, and the ordinates the corresponding pressures: we will suppose that the dilatation of the gas has continued until the successive reductions which its sensible caloric experiences have reduced the temperature T of the body A to the temperature *t* of the body B; its volume will then be A G, and the corresponding pressure F G. It will also be evident from the same reasoning, that the gas during this second part of its dilatation will develop a quantity of mechanical action represented by the area of the mixtilinear trapezium D E F G.

Now that the gas is brought to the temperature *t* of the body B, let us bring them together: if we compress the gas in an inclosure impermeable to heat, but in contact with the body B, the temperature of the gas will tend to rise by the evolution of latent heat rendered sensible by compression, but will be absorbed in proportion by the body B, so that the temperature of the gas will remain equal to *t*. The pressure will increase according to the law of Mariotte: it will be represented geometrically by the ordinates of a hyperbola K F, and the corresponding abscisses will represent the corresponding volumes. Suppose the compression to be increased until the heat disengaged and absorbed by the body B is precisely equal to the heat communicated to the gas by the source A during its dilatation in contact with it in the first part of the process. Let then the volume of gas be A H, and the corresponding pressure H K: the gas in this state contains the same absolute quantity of heat that it did at the moment of commencing the process, when it occupied the volume A B under the pressure C B. If therefore we remove the body B and continue to compress the gas in an inclosure impermeable to heat, until the volume A H is reduced to the volume A B, its temperature will successively increase by the evolution of the latent caloric, which the compression converts into sensible caloric. The pressure will increase in a corresponding ratio; and when the volume shall be reduced to A B, the temperature will become T, and the pressure B C. In fact, the successive states which the same weight of gas experiences are characterized by the volume, the pressure, the temperature, and the absolute quantity of caloric which it contains: two of these four quantities being known, the other two become known as consequences of the former; thus in the case in question the absolute quantity of heat and the volume having become what they were at the beginning of the process, we may be certain that the temperature and pressure will also be the same as before. Consequently, the unknown law according to which the pressure will vary when the volume of gas is reduced in its inclosure impermeable to heat, will be represented by a curve K C, which will pass through the point C, and in which the abscisses always represent the volumes, and the ordinates the pressures.

However, the reduction of the gaseous volume from A G to A B will have consumed a quantity of mechanical action which, for the reasons we have stated above, will be represented by the two mixtilinear trapeziums F G H K and K H B C. If we subtract from these two trapeziums the two first, C B D E and E D G F, which represent the quantity of action during the dilatation of the gas, the difference, which will be equal to the sort of curvilinear parallelogram C E F K, will represent the quantity of action developed in the circle of operations which we have just described, and after the completion of which the gas will be precisely in the same state in which it was originally. Still, however, the entire quantity of heat furnished by the body A to the gas during its dilatation by contact with it, passes into the body B during the condensation of the gas, which takes place by contact with it.

Here, then, we have mechanical force developed by the passage of caloric from a hot to a cold body, and this transfer is effected without the contact of bodies of different temperatures.

The inverse operation is equally possible: thus, we take the same volume of gas A B at the temperature T and under the pressure B C, inclose it in an envelope impermeable to heat, and dilate it until its temperature, gradually diminishing, becomes equal to *t*; we continue the dilatation in the same envelope,—but after having introduced the body B, which is at the same temperature,—and carry on the operation until the body B has restored to the gas the heat which it had received in the preceding operation. We next remove the body B, and condense the gas in an inclosure impermeable to heat until its temperature again becomes equal to T. We then introduce the body A, which possesses the same temperature, and continue the reduction of volume until all the heat taken from the body B is transferred to the body A. The gas will then be found to have the same temperature and to contain the same absolute quantity of heat as at the beginning of the operation, whence we may conclude that it occupies the same volume and is subjected to the same pressure.

Here the gas passes successively, but in an inverse order, through all the states of temperature and pressure through which it had passed in the first series of operations; consequently the dilatations become compressions, and reciprocally, but they follow the same law. Further, the quantities of action developed in the first case are absorbed in the second, and reciprocally; but they retain the same numerical values, for the elements of the integrals which compose them are the same.

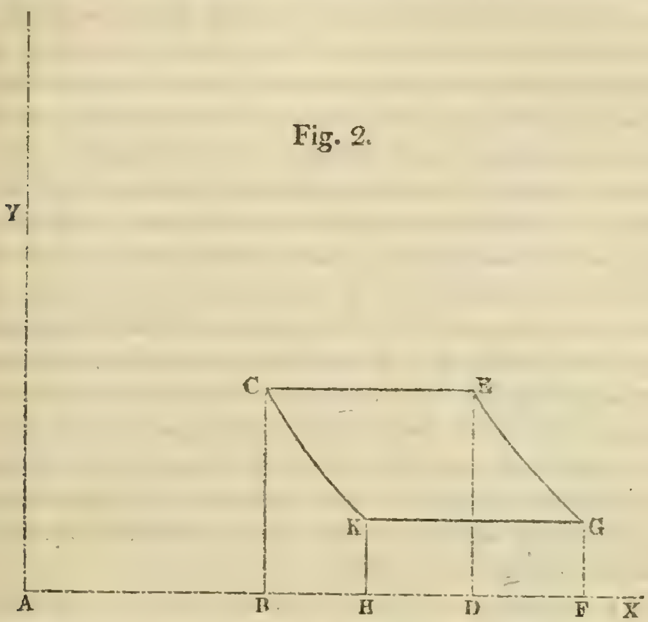
We thus see that by causing heat to pass, in the manner first indicated, from a body retained at a determinate temperature, into a body retained at an inferior temperature, we develop a certain quantity of mechanical action, which is equal to the quantity which must be consumed in order to cause the same quantity of heat to pass from a cold to a hot body, by the inverse process we have subsequently described.

We may arrive at a similar result by converting any liquid into vapour. We take the liquid and bring it into contact with the body A in an extensible envelope impermeable to heat, and suppose the temperature of the liquid to be equal to the temperature T of the body A. Upon the axis of the abscisses A X (fig. 2.) we describe a quantity A B equal to the volume of the liquid, and upon a line parallel to the axis of the ordinates A Y, a quantity B C equal to the pressure of the vapour of the liquid, which corresponds to the temperature T.

If we increase the volume of the liquid, a portion of it will pass into the state of vapour; and as the source of heat A furnishes the latent caloric necessary to its formation, the temperature will remain constant and equal to T. Then if quantities representing the successive volumes occupied by the mixture of liquid and vapour are described upon the axis of the abscisses, and the corresponding values of the pressures are taken for ordinates, as the pressure remains constant, the curve of the pressures will here be reduced to a right line C E parallel to the axis of the abscisses.

When a certain quantity of vapour has been formed, and the mixture of liquid and vapour occupies a volume A D, the body A may be withdrawn and the dilatation continued. A fresh quantity of liquid will then pass into the gaseous state, and a part of the sensible caloric becoming latent, the temperature of the mixture will diminish as well as

Fig. 2.



the pressure. Suppose the dilatation to be continued until the temperature diminishing gradually becomes equal to the temperature *t* of the body B; let A F be the volume and F G the pressure corresponding to it. The law of the variation of the pressure will be given by a curve E G, which will pass through the points E and G.

During this first part of the operation which we are describing, a quantity of action will be developed represented by the surface of the rectangle B C E D, and that of the mixtilinear trapezium E G F D.

We will now bring forward the body B, put it in contact with the mixture of liquid and vapour, and successively reduce its volume; a part of the vapour will pass into the liquid state, and as the latent heat disengaged in its condensation will be absorbed by the body B, the temperature will remain constant and equal to *t*. We shall thus continue to reduce the volume until all the heat furnished by the body A in the first part of the operation has been conveyed to the body B.

Let A H then be the volume occupied by the mixture of vapour and liquid; the corresponding pressure will be K H equal to G F: the temperature remaining equal to *t*, during the reduction of the volume from A F to A H, the law of the pressure between these two limits will be represented by the line K G parallel to the axis of the abscisses.

Arrived at this point, the mixture of vapour and liquid upon which we are operating, which occupies the volume A H under a pressure K H, at a temperature *t*, possesses the same absolute quantity of heat that the liquid possessed at the commencement of the operation; if, therefore, we remove the body B, and continue the condensation, in a vessel impermeable to heat, until the volume again becomes equal to A B, we shall have the same quantity of matter occupying the same volume, and possessing the same quantity of heat as at the commencement of the operation: its temperature and its pressure ought, therefore, also to be the same as at that epoch; the temperature will thus again become equal to T, and the pressure equal to C B. The law of the pressures during this last part of the operation, will therefore be given by a curve passing through the points K and C; and the quantity of action absorbed during the reduction of the volume from A F to A B, will be represented by the rectangle F H K G and the mixtilinear trapezium B C K H. If, then, we deduct from the quantity of action developed during the dilatation, that which is absorbed during the compression, we shall have for the difference the surface of the mixtilinear parallelogram C E G K, which will represent the quantity of action developed during the entire series of the operations that we have described, and at the conclusion of which the liquid employed will be found in its primitive state.

But it is necessary to remark that all the caloric communicated by the body A has passed to the body B, and that this transmission has taken place without there having been any other contact than that between bodies of the same temperature.

It might be proved in the same manner as for the gases, that by repeating the same operation in an inverse order, the heat of the body B may be made to pass to the body A, but that this result will only be obtained by the absorption of a quantity of mechanical action, equal to that which has been developed in the passage of the same quantity of caloric from the body A to the body B.

From what precedes, it results that a quantity of mechanical action and a quantity of heat passing from a hot to a cold body, are quantities of the same nature, and that it is possible to substitute the one for the other reciprocally; in the same manner as in mechanics a body falling from a certain height, and a mass endowed with a certain velocity, are quantities of the same order, and can be transformed one into the other by physical agents.

Hence also it follows that the quantity of action F developed by the passage of a certain quantity of heat C, from a body A maintained at a temperature T, to a body B maintained at a temperature *t*, by one of the processes that we have just indicated, is the same, whatever be the gas or the liquid employed, and is the greatest that it is possible to realize.

Suppose that by causing the quantity of heat C of the body A to pass to the body B, by some other process, it was possible to realize a larger quantity of mechanical action F′; we should employ one part of it F, to restore to the body A from the body B the quantity of heat C, by one of the two means that we have just described. The *vis viva* F employed for this purpose would be equal, as we have seen, to that which would be developed in the passage of the same quantity of heat C, from the body A to the body B; it is therefore, according to the hypothesis, smaller than F′; a quantity of action F′ — F, would therefore be produced, which would be created absolutely and without consumption of heat; an absurd result, which would imply the possibility of creating either force or heat in a gratuitous and indefinite manner. It appears to me that the impossibility of such a result might be accepted as a fundamental axiom of mechanics: the demonstration by pulleys, that Lagrange has given, of the principle of virtual velocities, against which no one has attempted to raise an objection, rests upon an analogous principle. In the same manner it may be proved that no gas or vapour exists which, employed in the processes described to transmit the heat of a hot body to a cold one, is capable of developing a larger quantity of action than any other gas or vapour.

We shall therefore admit the following principles as the basis of our researches.

Caloric passing from one body to another maintained at a lower temperature may cause the production of a certain quantity of mechanical action; there is a loss of *vis viva* whenever bodies of different temperature come into contact. The *maximum* effect will be produced when the passage of the caloric from the hot to the cold body takes place by one of the methods which we have just described. We may add, that the effect will be found to be independent of the chemical nature, of the quantity, and of the pressure of the gas or liquid employed; so that the maximum quantity of action, which the passage of a determinate quantity of heat from a hot to a cold body can develop, is independent of the nature of the agents which serve to realize it.

§ III.

We shall now translate analytically the various operations that have been described in the preceding paragraph; we shall deduce from them the expression of the *maximum* quantity of action produced by the passage of a given quantity of heat from a body maintained at a determinate temperature, to another body maintained at a lower temperature, and we shall arrive at new relations between the volume, the pressure, the temperature, and the absolute quantity of heat or latent caloric of solid, liquid, or gaseous bodies.

Let us return to the two bodies A and B, and suppose that the temperature t of the body B is lower by the infinitely small quantity dt, than the temperature t of the body A. We shall suppose in the first instance that a gas serves for the transmission to the body B, of the caloric of the body A. Let v_0 be the volume of the gas under the pressure p_0 at a temperature of t_0; let p and v be the volume and the pressure of the same weight of gas at the temperature t of the body A. The law enunciated by Mariotte, combined with that of Gay-Lussac, establishes between these different quantities the relation

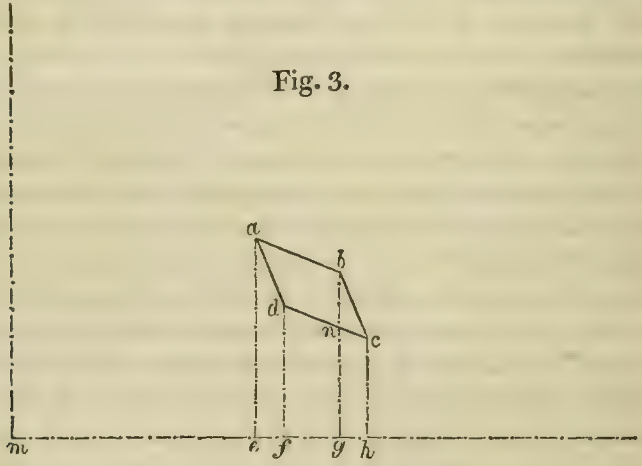
$$pv = \frac{p_0 v_0}{267 + t_0} (267 + t),$$

or, for simplicity, $\frac{p_0 v_0}{267 + t_0} = \mathrm{R}$:

$$p\,v = \mathrm{R}\,(267 + t).$$

The body A is brought into contact with the gas. Let $me = v$, $ae = p$ (fig. 3.). If the gas be allowed to expand by the infinitely small quantity $dv = eg$, the temperature will remain constant, in consequence of the presence of the source of heat A; the pressure will diminish, and become equal to the ordinate bg. We now remove the body A, and allow the gas to expand, in an inclosure impermeable to heat, by the infinitely small quantity $g\,h$, until the heat becomes latent, reduces the temperature of the gas by the infinitely small quantity dt, and thus brings it to the temperature $t - dt$ of the body B. In consequence of this reduction of temperature, the pressure will diminish more rapidly than in the first part of the operation, and will become ch. We now take the body B, and reduce the volume mh by the infinitely small quantity fh, calculated in such a manner that during this compression the gas may transmit to the body B all the heat it has derived from the body A during the first part of the operation. Let fd be the corresponding pressure; that done, we remove

Fig. 3.



the body B, and continue to compress the gas until it is again reduced to the volume me. The pressure will then again be equal to ae, as we have shown in the preceding paragraph; and in the same manner also it will be proved, that the quadrilateral figure $a\,b\,c\,d$ will be the measure of the quantity of action produced by the transmission to the body B, of the heat derived from the body A, during the expansion of the gas.

Now it is easy to show that this quadrilateral figure is a parallelogram; this results from the infinitely small values assigned to the variations of the volume and pressure: let us conceive that perpendiculars are erected upon each point of the plane upon which the quadrilateral figure $a\,b\,c\,d$ is traced, and that on each of them, commencing at their foot, are described two quantities T and Q, the first equal to the temperature, the second to the absolute quantity of heat possessed by the gas, when the volume and the pressure have the value assigned to them by the absciss v and the ordinate p which correspond to each point.

The lines ab and cd belong to the projections of two curves of equality of temperature, passing through two points infinitely near, taken upon the surface of temperatures; ab and cd are therefore parallel: ad and bc will be also projections from two curves, for which $\mathrm{Q} = \text{const.}$, and which would also pass through two points infinitely near, taken upon the surface $\mathrm{Q} = f\,(p\,v)$; these two elements are therefore also parallel. The quadrilateral figure $abcd$ is therefore a parallelogram, and it is easy to see that its area may be obtained by multiplying the variation of the volume during the contact of the gas with the body A or the body B, that is to say, eg, or its equal fh, by bn, the difference of the pressures supported during these two operations, and corresponding to the same value of the volume v. Now, eg, or fh, being the differentials of the volume, are equal to dv; bn will be obtained by differentiating the equation $pv = \mathrm{R}\,(267 + v)$, supposing v constant; we shall then have $bn = dp = \mathrm{R}\,\frac{dt}{v}$. The expression of the quantity of action developed will therefore be $\mathrm{R}\,\frac{dt\,dv}{v}$.

It remains to determine the quantity of heat necessary to produce this effect: it is equal to that which the gas has derived from the body A, whilst its volume has increased by dv, at the same time preserving the same temperature t. Now Q being the absolute quantity of heat possessed by the gas, ought to be a certain function of p and of v, considered as independent variables; the quantity of heat absorbed by the gas will therefore be

$$d\mathrm{Q} = \frac{d\mathrm{Q}}{dv}\,dv + \frac{d\mathrm{Q}}{dp}\,dp;$$

but the temperature remaining constant during the variation of the volume, we have

$$v\,dp + p\,dv = 0, \text{ whence } dp = -\frac{p}{v}\,dv,$$

and consequently

$$d\,\mathrm{Q} = \left(\frac{d\mathrm{Q}}{dv} - \frac{p}{v}\frac{d\mathrm{Q}}{dp}\right) dv.$$

If we divide the effect produced by this value of $d\mathrm{Q}$, we shall have

$$\frac{R\,dt}{v\frac{d\mathrm{Q}}{dv} - p\frac{d\mathrm{Q}}{dp}}$$

for the expression of the *maximum* effect which can be developed by the passage of a quantity of heat equal to unity, from a body maintained at the temperature t to a body maintained at the temperature $t - dt$.

We have shown that this quantity of action developed is independent of the agent which has served to transmit the heat; it is therefore the same for all the gases, and is equally independent of the ponderable quantity of the body employed: but there is nothing that proves it to be independent of the temperature; $v\frac{d\mathrm{Q}}{dv} - p\frac{d\mathrm{Q}}{dp}$ ought therefore to be equal to an unknown function of t, which is the same for all the gases.

Now by the equation $pv = \mathrm{R}\,(267 + t)$, t is itself the function of the product $p\,v$; the partial differential equation is therefore

$$v\frac{d\mathrm{Q}}{dv} - p\frac{d\mathrm{Q}}{dp} = \mathrm{F}\,(p\,.\,v),$$

having for its integral

$$\mathrm{Q} = f\,(p\,.\,v) - \mathrm{F}\,(p\,.\,v)\log\,[\,(hyp)p\,].$$

No change is effected in the generality of this formula by substituting for these two arbitrary functions of the product $p\,v$, the functions B and C of the temperature, multiplied by the coefficient R; we shall thus have

$$\mathrm{Q} = \mathrm{R}\,(\mathrm{B} - \mathrm{C}\log p).$$

That this value of Q satisfies all the conditions to which it is subject may be easily verified; in fact we have

$$\frac{d\mathrm{Q}}{dv} = \mathrm{R}\left(\frac{d\mathrm{B}}{dt}\frac{p}{\mathrm{R}} - \log p\,\frac{d\mathrm{C}}{dt}\frac{p}{\mathrm{R}}\right)$$

$$\frac{d\mathrm{Q}}{dp} = \mathrm{R}\left(\frac{d\mathrm{B}}{dt}\frac{v}{\mathrm{R}} - \log p\,\frac{d\mathrm{C}}{dt}\frac{v}{\mathrm{R}} - \mathrm{C}\,\frac{1}{p}\right);$$

whence

$$v\frac{dQ}{dv} - p\frac{dQ}{dp} = C\,R,$$

and consequently

$$\frac{R\,dt}{v\frac{dQ}{dv} - p\frac{dQ}{dp}} = \frac{dt}{C}.$$

The function C by which the logarithm of the pressure in the value of Q is multiplied is, as we see, of great importance; it is independent of the nature of the gases, and is a function of the temperature alone; it is essentially positive, and serves as a measure of the *maximum* quantity of action developed by the heat.

We have seen that of the four quantities Q, t, p, and v, two being known, the other two follow from them; they ought therefore to be united together by two equations; one of them,

$$p\,v = R\,(267 + t),$$

results from the combined laws of Mariotte and Gay-Lussac. The equation

$$Q = R\,(B - C \log p),$$

deduced from our theory, is the second. However, the numerical determination of the alterations produced in the gases, when the volume and the pressure are varied in an arbitrary manner, requires a knowledge of the functions B and C.

We shall see upon another occasion that a value approaching to the function C may be obtained through a considerable extent of the thermometrical scale; besides, being determined for one gas it will be determined for all. As to the function B, it may vary in different gases; however, it is probable that it is the same for all the simple gases: that they all have the same capacity for heat, is at least the apparent result of the indications of experiment.

Let us return to the equation

$$Q = R\,(B - C \log p).$$

We will compress a gas occupying the volume v, under the pressure p, until the volume becomes v', and allow it to cool till the temperature sinks to the same point. Let p' be the new value of the pressure; let Q′ be the new value of Q; we shall have

$$Q - Q' = R\,C \log \frac{p'}{p} = R\,C \log \frac{v}{v'}.$$

The function C being the same for all the gases, it is evident that *equal volumes of all the elastic fluids, taken at the same temperature and under the same pressure, being compressed or expanded by the same fraction of their volume, disengage or absorb the same absolute quantity of heat.* This law M. Dulong has deduced from direct experiment.

This equation shows also that *when a gas varies in volume without change of temperature, the quantities of heat absorbed or disengaged by this gas are in arithmetical progression, if the increments or reductions of volume are in geometrical progression.* M. Carnot enunciates this result in the work already cited.

The equation

$$Q - Q' = R\,C \log \left(\frac{v}{v'}\right)$$

expresses a more general law; it includes all the circumstances by which the phænomenon can be affected, such as the pressure, the volume, and the temperature.

In fact, since

$$R = \frac{p_0\, v_0}{267 + t_0} = \frac{p\, v}{267 + t},$$

we have

$$Q - Q' = \frac{267 + t}{p\, v}\, C \log \frac{v}{v'}.$$

This equation exhibits the influence of the pressure; it shows that *equal volumes of all the gases, taken at the same temperature, being compressed or expanded by the same fraction of their volume, disengage or absorb quantities of heat proportionate to the pressure.*

This result explains why the sudden entrance of the air into the vacuum of the air-pump does not disengage a sensible quantity of heat. The vacuum of the air-pump is nothing but a volume of gas v, of which the pressure p is very small; if atmospheric air be admitted, its pressure p will suddenly become equal to the pressure of p' of the atmosphere, its volume v will be reduced to v', and the expression of the heat disengaged will be

$$C \frac{p\, v}{267 + t} \log \frac{v}{v'} = C \frac{p\, v}{267 + t} \log \frac{p'}{p}.$$

The heat disengaged by the reentrance of atmospheric air into the vacuum will therefore be what this expression becomes when p is there made very small; it is then found that $\log \frac{p'}{p}$ becomes very great, but the product of p by $\log \frac{p'}{p}$ is not the less small on that account; in fact we have

$$p \log \frac{p'}{p} = p \log p' - p \log p = p\,(\log p' - \log p),$$

a quantity which converges towards zero when p diminishes.

The quantity of heat disengaged will therefore be small in proportion to the feebleness of pressure in the recipient, and it will be reduced to zero when the vacuum is perfect.

We shall add that the equation

$$Q = R\,(B - C \log p)$$

gives the law of the specific calorics at a constant pressure and volume. The expression of the first is

$$R\left(\frac{dB}{dt} - \frac{dC}{dt}\log p\right);$$

of the second,

$$R\left(\frac{dB}{dt} - \frac{dC}{dt}\log p - C\,\frac{1}{p}\,\frac{dp}{dt}\right),$$

equal to

$$R\left(\frac{dB}{dt} - \frac{dC}{dt}\log p - \frac{C}{267 + t}\right).$$

The first may be obtained by differentiating Q with relation to t, supposing p constant; the second, by supposing v constant. If we take equal volumes of different gases at the same temperature and under the same pressure, the quantity R will be the same for all; and accordingly we see that the excess of specific caloric at a constant pressure, over the specific caloric of a constant volume, is the same for all, and equal to $\frac{R}{267 + t}$ C.

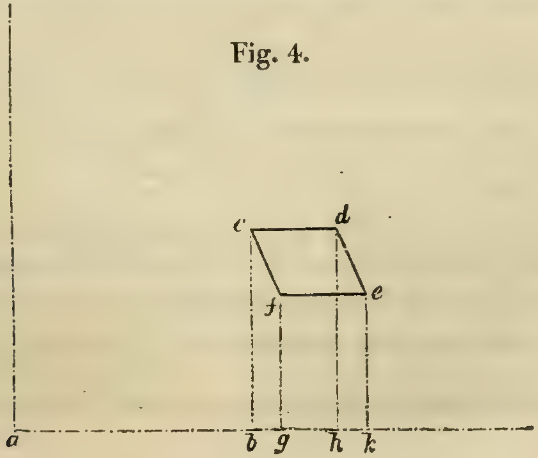
§ IV.

The same method of reasoning applied to vapours enables us to establish a new relation between their latent caloric, their volume, and their pressure.

We have shown in the second paragraph how a liquid passing into the state of vapour may serve to transmit the caloric from a body maintained at a temperature T, to a body maintained at a lower temperature t, and how this transmission develops the motive force.

Let us suppose that the temperature of the body B is lower by the infinitely small quantity dt than the temperature of the body A. We have seen that if $c\,b$ (fig. 4.) represents the pressure of the vapour

Fig. 4.



of the liquid corresponding to the temperature t of the body A, and fg that which corresponds to the temperature $t - dt$ of the body B; bh the increase of volume due to the vapour formed in contact with the body A, hk that which is due to the vapour formed after the body A has been removed, the formation of which has reduced the temperature by the quantity dt; we have seen, I say, that the quantity of action developed by the transmission of the latent caloric furnished by the body A, [and transmitted] from that body to the body B, is measured by the quadrilateral figure $cdef$. Now this surface is equal, if we neglect the infinitely small quantities of the second order, to the product of the volume cd by the differential of the pressure $dh - ek$. Naming p the pressure of the vapour of the liquid corresponding to the temperature t, p will be a function of t, and we shall have $dh - ek = \frac{dp}{dt} dt$.

cd will be equal to the increase of volume produced in water when it passes from the liquid into the gaseous state, under the pressure p, at a corresponding temperature. If we call ρ the density of the liquid, δ that of the vapour, and v the volume of the vapour formed, δv will be its weight, and $\frac{\delta v}{\rho}$ will be the volume of the liquid evaporated. The increase of volume owing to the formation of a volume v of vapour will therefore be

$$v\left(1 - \frac{\delta}{\rho}\right).$$

The effect produced will therefore be

$$\left(1 - \frac{\delta}{\rho}\right) v \frac{dp}{dt} dt.$$

The heat, by means of which this quantity of action has been produced, is the latent caloric of the volume v of vapour formed; let k be a function of t representing the latent caloric contained in the unity of volume of the vapour furnished by the liquid subjected to experiment, at a temperature t, and under a corresponding pressure, the latent caloric of the volume v will be kv, and the ratio of the effect produced to the heat expended will be expressed by

$$\frac{\left(1 - \frac{\delta}{\rho}\right) \frac{dp}{dt} dt}{k}.$$

We have demonstrated that it is the greatest which can possibly be obtained; that it is independent of the nature of the liquid employed, and the same as that obtained by the employment of the permanent gases: now we have seen that this is expressed by $\frac{dt}{C}$, C being a function of t independent of the nature of the gases; we shall therefore also have

$$\frac{\left(1-\frac{\delta}{\rho}\right)\frac{dp}{dt}}{k}=\frac{1}{C},\quad\text{whence } k=\left(1-\frac{\delta}{\rho}\right)\frac{dp}{dt}C.$$

With regard to the generality of vapours, the ratio $\frac{\delta}{\rho}$ of the density of the vapour to that of the liquid from which it is formed may be neglected before it arrives at unity, so long as the temperature is not very high; we shall have therefore, sensibly,

$$k=C\frac{dp}{dt}.$$

This equation expresses that *the latent caloric contained in equal volumes of the vapour of different liquids at the same temperature, and under the corresponding pressure, is proportional to the coefficient* $\frac{dp}{dt}$ *of the pressure with regard to the temperature.*

Whence it results, that the latent caloric contained in the vapours of liquids which commence boiling only at high temperatures, as mercury for example, is very feeble, since for these vapours the quantity $\frac{dp}{dt}$ is very small.

We shall not insist upon the consequences which result from the equation

$$k=\left(1-\frac{\delta}{\rho}\right)\frac{dp}{dt}C.$$

We shall simply remark that if, as everything leads us to believe, C and $\frac{dp}{dt}$ do not become infinite for any value of the temperature, k will become null when we have $\delta=\rho$, that is, that when the pressure is strong enough, and the temperature sufficiently elevated to render the density of the vapour equal to that of the liquid, the latent caloric is reduced to zero.

§ V.

Variation is produced in the volume of all the substances of nature by changes in the temperature and pressure to which they are subjected; liquids and solids are amenable to this law, and serve equally to develop the motive power of heat; it has been proposed to substitute them for the vapour of water, in order to render this motive force available; they have even occasionally been advantageously employed, particularly when it was necessary to develop a very considerable momentaneous effort, exerted within narrow limits.

In bodies of these kinds, as in the gases, it may be remarked, that of the four quantities, the volume v, the pressure p, the temperature T, and the absolute quantity of heat Q, two being determined, the others are deducible from them; if then we take two of them, p and v for example, as independent variables, the two others T and Q may be considered as functions of the former two.

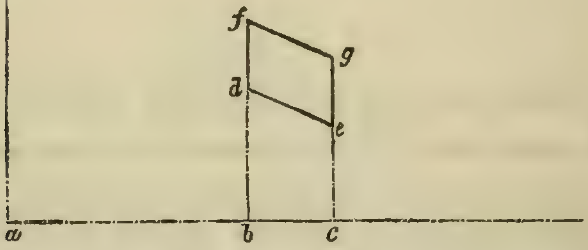
In what manner the quantities T, p, and v, vary with respect to each other may be ascertained by direct experiments upon the elasticity and dilatability of bodies; it is thus that Mariotte's law relative to the elasticity of the gases, and Gay Lussac's relative to their dilatability, lead to the equation

$$p\,v = R\,(267 + t);$$

all that remains is to determine Q in functions of p and v.

A relation exists between the functions T and Q, which may be deduced from principles analogous to those which we have just established. Let us increase the temperature of the body by the infinitely small quantity dT, and at the same time prevent the increase of the volume; the pressure will then be augmented; if we represent the volume v by the absciss $a\,b$ (fig. 5), and the primitive pressure by the ordinate $b\,d$, this augmentation of pressure may be represented by the quantity $d\,f$, which will be of the same order as the increase of temperature dT to which it is owing, that is infinitely small.

Fig. 5.



Now we will take a source of heat A, maintained at the temperature T + dT, and allow the volume v to increase by the quantity $b\,c$; the presence of the source A, maintained at the temperature T + dT, prevents the reduction of the temperature. During this contact, the quantity Q of heat that the body possesses will increase by the quantity dQ, which will be derived from the source A. We will afterwards remove the source A, and the given body will become cool by the quantity dT, at the same time retaining the volume $a\,c$. The pressure will then diminish by the infinitely small quantity $g\,e$.

The temperature of the body being thus reduced to T, which is that of the source of heat B, we will take B, and reduce the volume of the body by a quantity $b c$, in such a manner that all the heat developed by the diminution of volume may be absorbed by the body B, and the temperature remain equal to its primitive value T. The volume V also again becoming the same as it was at the commencement of the operation, it is certain that the pressure will return to its primitive value $b d$, as will also the absolute quantity of heat Q.

If we now connect the four points f, g, e, d by right lines we shall form a quadrilateral figure, the area of which will measure the quantity of action developed during the operation described. Now it is easy to see that $f g$ and $d c$ are two elements infinitely near, described upon two curves infinitely near, the equations of which will be $\mathrm{T} + d\mathrm{T} = \text{const.}$, and $\mathrm{T} = \text{const.}$ They ought therefore to be considered as parallel; the two ordinates which terminate the quadrilateral figure in the other direction being also parallel, the figure is parallelogrammical, and measures $b c \times d f$.

Now $f d$ is nothing but the increase experienced by the pressure p, the volume v remaining constant, and T becoming $\mathrm{T} + d\mathrm{T}$. We have therefore

$$d f = \frac{d p}{d \mathrm{T}} d \mathrm{T},$$

whence

$$f d = \frac{1}{\frac{d \mathrm{T}}{d p}} d \mathrm{T}.$$

And $b c$ being the increase of volume $d v$

$$f d \times b c = \frac{d v . d \mathrm{T}}{\frac{d \mathrm{T}}{d p}}.$$

It only remains to determine the heat consumed in the production of this quantity of mechanical action.

We have first raised the temperature of the body subjected to experiment by the quantity $d\mathrm{T}$ without changing its primitive volume v; afterwards, when it had become $v + d v$, we have lowered its temperature by the same quantity $d\mathrm{T}$ without varying its primitive volume $v + d v$. Now it may easily be seen that this double operation can be effected without loss of heat; let us suppose that n being a number indefinitely great, the interval of temperature $d\mathrm{T}$ be divided into a number n of new intervals $\frac{d\mathrm{T}}{n}$, and that we have $n + 1$ sources of heat maintained at the temperatures $\mathrm{T}, \mathrm{T} + \frac{d\mathrm{T}}{n}, \ \mathrm{T} + \frac{2 d\mathrm{T}}{n}, \ldots\ldots$ $\mathrm{T} + \frac{(n-1) d\mathrm{T}}{n}$ and $\mathrm{T} + d\mathrm{T}$.

To raise the temperature of the body upon which we are operating from T to $\mathrm{T} + d\mathrm{T}$, we bring it successively into contact with the second, the third, and the $(n+1)$th of these sources, until it has acquired the temperature of each of them. When, on the contrary, the volume v of the body being increased by dv, we wish to give it the temperature T, we bring it successively into contact with the nth, the $(n-1)$th, and the first of these sources, until it has acquired the temperature of each of them. We then return to these sources the heat that has been borrowed from them in the first part of the operation; for it is not necessary to attend to the differences of an order of inferior magnitude, arising from changes that may have been produced in the specific caloric of the body, in consequence of the variations of v and Q.

Nothing therefore will have been lost or gained by any of these sources, excepting always the source of which the temperature is $\mathrm{T} + d\mathrm{T}$, which will have lost the heat necessary to elevate the temperature of the body upon which we are operating from $\mathrm{T} + \frac{(n-1)\,d\mathrm{T}}{n}$ to $\mathrm{T} + d\mathrm{T}$, and the source maintained at the temperature T, which will have acquired the heat necessary to reduce the temperature of the same body from $\mathrm{T} + \frac{d\mathrm{T}}{n}$ to T. If we suppose n to be infinitely great, these quantities of heat may be neglected.

We see, therefore, that when the body in question, (its temperature being thus reduced to T,) is brought into contact with the source of heat B, the heat communicated to it from the source A will be all it has gained from the commencement of the operation. In consequence of the reduction of its volume in contact with the body B, it will be found at its original volume and temperature; the quantities Q and P will therefore have re-assumed their primitive value; it is therefore certain that all the heat borrowed from the source A, and nothing but that heat, will have been given to the body B.

Whence it results that the effect produced,

$$\frac{dv\,d\mathrm{T}}{\frac{d\mathrm{T}}{dp}}$$

is owing to the transmission of the heat absorbed by the body subjected to the experiment during its contact with the source A, and which has afterwards flowed into the source B.

The temperature having remained constant during the contact with the source A, it follows that the variations dp and dv of the pressure and the volume are connected by the relation

$$\frac{d\mathrm{T}}{dp}dp + \frac{d\mathrm{T}}{dv}dv = 0$$

These variations dp and dv occasion a variation in the absolute quantity of heat Q, the expression of which is

$$dQ = \frac{dQ}{dp}dp + \frac{dQ}{dv}\,dv = dv\left[\frac{dQ}{dv} - \frac{dQ}{dp}\frac{\left(\frac{dT}{dv}\right)}{\left(\frac{dT}{dp}\right)}\right];$$

such is the quantity of heat consumed in the production of the effect that we have just calculated. The effect produced by a quantity of heat equal to unity will therefore be

$$\frac{dT}{\frac{dQ}{dv}.\frac{dT}{dp} - \frac{dQ}{dp}.\frac{dT}{dv}}.$$

It will be shown, as in the case of the gases, that this effect produced, is the largest which it is possible to realize; and as all the substances of nature may be employed, in the manner that has just been indicated, to produce this *maximum* effect, it is necessarily the same for all.

When this theory has been applied specially to the gases, we have called $\frac{1}{C}$ the coefficient of dT in the expression of this maximum quantity of action; the equation therefore of all the substances of nature, solid, liquid, or gaseous, will be

$$\frac{dQ}{dv}.\frac{dT}{dp} - \frac{dQ}{dp}.\frac{dT}{dv} = C$$

in which C is a function of the temperature which is the same for all.

For the gases we have

$$T = -267 + \frac{1}{R}pv,$$

whence we deduce

$$\frac{dT}{dp} = \frac{v}{R} \text{ and } \frac{dT}{dv} = \frac{p}{R}.$$

The preceding equation applied to the gases takes therefore the form

$$v\frac{dQ}{dv} - p\frac{dQ}{dp} = RC = F(p,v):$$

it is the equation at which we have already arrived, and of which the integral is

$$Q = R(B - C\log p);$$

that of the general equation

$$\frac{dQ}{dv}\frac{dT}{dp} - \frac{dQ}{dp}\frac{dT}{dv} = C$$

is of the form

$$Q = F(T) - C\phi(p,v);$$

F(T) is an arbitrary function of the temperature, and $\phi(p,v)$ a particular function satisfying the equation

$$\frac{d\,\mathrm{T}}{d\,v}\,\frac{d\,\phi}{d\,p} - \frac{d\,\mathrm{T}}{d\,p}\,\frac{d\,\phi}{d\,v} = 1$$

(See the note appended to this Memoir.)

We shall now deduce various consequences from the general equation at which we have arrived.

We have previously seen that when we compress a body by the quantity $d\,v$, the temperature remaining constant, the heat disengaged by the condensation is equal to

$$d\,\mathrm{Q} = d\,v \left[\frac{d\,\mathrm{Q}}{d\,v} - \frac{d\,\mathrm{Q}}{d\,p}\,\frac{\left(\frac{d\,\mathrm{T}}{d\,v}\right)}{\left(\frac{d\,\mathrm{T}}{d\,p}\right)}\right];$$

and as

$$\frac{d\,\mathrm{Q}}{d\,v}\,\frac{d\,\mathrm{T}}{d\,p} - \frac{d\,\mathrm{Q}}{d\,p}\,\frac{d\,\mathrm{T}}{d\,v} = \mathrm{C},$$

the preceding expression takes the form

$$d\,\mathrm{Q} = d\,v\,\frac{\mathrm{C}}{\left(\frac{d\,\mathrm{T}}{d\,p}\right)} = -\,d\,p\,\frac{\mathrm{C}}{\left(\frac{d\,\mathrm{T}}{d\,v}\right)}$$

This last equation may be put under the form

$$d\,\mathrm{Q} = -\,d\,p\,\mathrm{C}\,\frac{d\,v}{d\,\mathrm{T}};$$

$\frac{d\,v}{d\,\mathrm{T}}$ is the differential coefficient of the volume with regard to the temperature, the pressure remaining constant.

We thus arrive at this general law, which is applicable to all the substances of nature, solid, liquid, or gaseous: *If the pressure supported by different bodies, taken at the same temperature, be augmented by a small quantity, quantities of heat will be disengaged from it, which will be proportional to their dilatability by heat.*

This result is the most general consequence deducible from this axiom: that it is absurd to suppose that motive force or heat can be created gratuitously and absolutely.

§ VI.

The function of the temperature C is, as we see, of great importance, in consequence of the part it sustains in the theory of heat: it enters into the expression of the latent caloric which is contained in all substances, and which is disengaged from them by pressure. Unfortunately no experiments have been made from which we can determine the values of this function, corresponding to all the values of the temperature. To obtain $t = 0$ we must proceed in the following manner.

M. Dulong has shown that the air, and all the other gases taken at the temperature of 0°, and under the pressure $0^{m}\cdot 76$ of mercury, when compressed by $\frac{1}{267}$ of their volume, disengage a quantity of heat, capable of elevating the same volume of atmospheric air by 0·421.

Suppose that we operate upon a kilogramme of air occupying a volume $v = 0\cdot 770$ of a cubic metre, under the pressure of the atmosphere p, equivalent to 10230 kilogrammes upon a square metre; we have

$$p\,v = \mathrm{R}\,(267 + t),$$

and

$$\mathrm{Q} = \mathrm{R}\,(\mathrm{B} - \mathrm{C}\log p).$$

If a variation be suddenly effected in v by an infinitely small quantity $d\,v$, without there being any variation in the absolute quantity of heat Q, we shall have

$$p\,d\,v + v\,d\,p = \mathrm{R}\,d\,t,$$

and

$$0 = \mathrm{R}\left(\frac{d\,\mathrm{B}}{d\,t} - \log p\,\frac{d\,\mathrm{C}}{d\,t}\right)d\,t - \mathrm{R}\,\mathrm{C}\,\frac{d\,p}{p},$$

or preferably

$$\frac{d\,t}{\mathrm{C}}\,\mathrm{R}\left(\frac{d\,\mathrm{B}}{d\,t} - \log p\,\frac{d\,\mathrm{C}}{d\,t}\right) = \mathrm{R}\,\frac{d\,p}{p} = \frac{\mathrm{R}}{p}\left(\frac{\mathrm{R}\,d\,t - p\,d\,v}{v}\right) = \frac{\mathrm{R}\,d\,t - p\,d\,v}{267 + t}.$$

Now $\mathrm{R}\left(\frac{d\,\mathrm{B}}{d\,t} - \frac{d\,\mathrm{C}}{d\,t}\log p\right)$ being the partial differential of Q in respect of t, p remaining constant, is nothing else than the specific caloric of the air at a constant pressure; it is the number of unities of heat necessary to elevate a kilogramme of air under atmospheric pressure by one degree; we have therefore

$$\mathrm{R}\left(\frac{d\,\mathrm{B}}{d\,t} - \frac{d\,\mathrm{C}}{d\,t}\log p\right) = 0\cdot 267.$$

Then substituting $-\frac{v}{267}$ for $d\,v$, and 0·421 for $d\,t$, we arrive lastly at

$$\frac{1}{\mathrm{C}} = 1\cdot 41.$$

This is the *maximum* effect producible by a quantity of heat, equal to that which would elevate by 1° a kilogramme of water taken at zero, passing from a body maintained at 1° to a body maintained at 0°. It is expressed in kilogrammes raised one metre high.

Having the value of C, which corresponds to $t = 0$, it is interesting to know, setting out from this point, whether C increases or decreases, and in what proportion. An experiment of MM. De Laroche and Bérard upon the variations experienced by the specific caloric of the air, when the pressure is varied, enables us to calculate the value of the differential coefficient $\frac{dC}{dt}$.

In fact, according to our formulas, the specific caloric of the air under two pressures p and p' differs by $R\frac{dC}{dt} - \log\frac{p}{p'}$; rendering this quantity equal to the difference of the specific calorics, as it has been deduced from the results of MM. De Laroche and Bérard; taking the mean of two experiments, we find

$$\frac{dC}{dt} = 0{\cdot}002565.$$

In these experiments the air entered into the calorimeter at the temperature of 96°·90, and quitted it at that of 22°·83; 0·002565 is therefore the mean value of the differential coefficient $\frac{dC}{dt}$ between these two temperatures.

From this result we learn, that between these two limits the function C increases, though very slowly; consequently the quantity $\frac{1}{C}$ diminishes; whence it follows that the effect produced by the heat diminishes at high temperatures, though very slowly.

The theory of vapours will furnish us with new values of the function C at other temperatures. Let us return to the formula

$$\frac{1}{C} = \frac{\left(1 - \frac{\delta}{\rho}\right)\frac{dp}{dt}}{k},$$

which we have demonstrated in paragraph IV. If we neglect the density of the vapour before that of the fluid, this formula will be reduced to

$$\frac{1}{C} = \frac{\frac{dp}{dt}}{k}.$$

We may remark in passing, that at the temperature of ebullition $\frac{dp}{dt}$ is nearly the same for all vapours; C itself varies little with the temperature, so that k is nearly constant. This explains the observations of certain philosophers, who have remarked that at the boiling point, equal volumes of all vapours contain the same quantity of latent caloric; but we see at the same time that we are only approximating to this law, since it supposes that C and $\frac{dp}{dt}$ are the same for all vapours at the boiling point.

From the experiments made by several philosophers we are enabled to calculate the values corresponding to the boiling point of k and $\frac{p}{d\,t}$, for different liquids; we can therefore deduce from them the corresponding values of $\frac{1}{\text{C}}$.

NAMES of Liquids.	Value in atmospheres of $\frac{d\,p}{d\,t}$ at the temperature of ebullition.	Density of the vapour at the temp. of ebullition, the density of the air being 1.	Quantity of latent heat contained in a kilogramme of vapour.	Temperature of ebullition.	Corresponding values of $\frac{1}{\text{C}}$.
Sulphuric Ether...	$\frac{1}{28{\cdot}12}$	2·280	90·8	35·5	1·365
Alcohol............	$\frac{1}{25{\cdot}19}$	1·258	207·7	78·8	1·208
Water	$\frac{1}{29{\cdot}1}$	0·451	543·0	100·	1·115
Essence of Turpentine	$\frac{1}{30}$	3·207	76·8	156·8	1·076

These results confirm, in a striking manner, the theory that we are explaining; they show that C is slowly augmented with the temperature, as has been already stated: we have seen that for $t = o$, $\frac{1}{\text{C}} = 1{\cdot}41$, whence $\text{C} = 0{\cdot}7092$; this result is deduced from experiments upon the velocity of sound.

We here find, starting from experiments upon the vapour of water, for $t = 100°$, $\frac{1}{\text{C}} = 1{\cdot}115$, whence $\text{C} = 0{\cdot}8969$; C is therefore increased from 0 at 100° to 0·187, which gives as the mean of the differential coefficient between these two limits

$$\frac{d\text{C}}{d\,t} = 0{\cdot}00187.$$

The mean of the two experiments performed by MM. De Laroche and Bérard gives us, between the limits 22°·83 and 96·90, for the mean value of $\frac{d\text{C}}{d\,t}$ the quantity 0·002565.

These two results differ little from each other, and their divergence will be sufficiently explained, by reflecting on the number and the variety of the experiments whence the data on which they are founded are derived.

There is another means of calculating the values of $\frac{1}{C}$, between extended limits of the temperature in an approximative manner; for this it is necessary to admit, that the quantity of caloric contained in a given weight of the vapour of water is the same whatever be the temperature and the corresponding pressure; and still further, that the laws relative to the compression and the dilatation of the permanent gases are equally applicable to vapours: adopting these laws, towards which we have only approximated, the formula

$$\frac{1}{C} = \frac{\frac{dp}{dt}}{K}$$

will express K in function of t; $\frac{dp}{dt}$ may be deduced from 0 to 100° from experiments long since made by several philosophers, and from 100° to 224° from recent experiments of MM. Arago and Dulong.

Thus we find for			Values of $\frac{1}{C}$.
$t =$ 0°	$\frac{1}{C}$ = 1·586	We have already found for the values of $\frac{1}{C}$ corresponding to the same values of t.	1·410
$t =$ 35·5	$\frac{1}{C}$ = 1·292		1·365
$t =$ 78·8	$\frac{1}{C}$ = 1·142		1·208
$t =$ 100°	$\frac{1}{C}$ = 1·102		1·106
$t =$ 156·8	$\frac{1}{C}$ = 1·072		1·078

These last, deduced from experiments upon sound, the vapours of sulphuric ether, alcohol, water, and essence of turpentine, accord with the first in a satisfactory manner.

These remarkable coincidences, obtained by numerical operations performed upon a great variety of data, furnished by phænomena of many different kinds, appear to us to add greatly to the evidence of our theory.

§ VII.

The function C is, as we have seen, of great importance: it is the connecting link of all the phænomena produced by heat upon solid, liquid, or gaseous bodies. It is greatly to be desired that experiments of the most precise exactitude, such as the researches upon the propagation of sound in gases taken at different temperatures, were instituted, in order to establish this function with all the requisite certainty. It would conduce to the determination of several other important elements of the theory of heat, with regard to which we know nothing, or have arrived by our experiments at very insufficient approximations only. In this number may be included the heat disengaged by the compression of solid or liquid bodies; the theory that we have enunciated enables us to determine it numerically for all the values of the temperature for which the function C is known in a manner sufficiently exact, that is to say, from $t = 0$ to $t = 224°$.

We have seen that the heat disengaged by the augmentation of pressure dp is equal to the dilatation by the heat of the body subjected to experiment, multiplied by C. With regard to the air taken at zero, the quantity of heat disengaged may be directly deduced from the experiments upon sound in the following manner.

M. Dulong has shown that a compression of $\frac{1}{267}$ raises the temperature of a volume of air taken at zero by 0°·421. Now the 0·267 unity of heat necessary to elevate a kilogramme of air taken at zero under a constant pressure by 1°, are equal to the heat necessary to maintain the temperature of the gas dilated by $\frac{1}{267}$ of its volume at zero, above the heat necessary to elevate the dilated volume, maintained constant, by 1°; the last is equal to $\frac{1}{0·421}$ of the first; their sum is therefore equal to the first multiplied by $1 + \frac{0}{0·421}$; the former therefore, that is the heat necessary to maintain the temperature of 1 kil. of air, dilated by $\frac{1}{267}$ of its volume, at zero, is equal to $(0·267) : \left(1 + \frac{1}{0·421}\right)$, or to 0·07911.

We arrive at the same results by the application of the formula

$$Q = R\,(B - C \log p),$$

whence

$$dQ = R\,C\,\frac{dv}{v},$$

putting $C = \frac{1}{410}$, and observing that a diminution of volume of $\frac{1}{267}$ corresponds to an increase of pressure equal to $\frac{1}{267}$ of an atmosphere.

Knowing the quantity of heat disengaged from gases by compression, to ascertain that which a similar pressure would disengage from any substance whatever, from iron for example, we write the proportion: 0·07911 of heat disengaged by a volume of air equal to 0·77 of a cubic metre, subjected to an increase of pressure equal to $\frac{1}{267}$ of an atmosphere, is to that disengaged from the same volume of iron under the same circumstances as 0·00375, the cubic dilatability of the air, is to 0·00003663, the cubic dilatability of iron. For the second term of the proportion we find the number 0·0007718. Now a volume of $0^{m}\cdot77$ of iron weighs 5996 kilogrammes; the heat disengaged by one kilogramme will therefore be $\frac{0\cdot007718}{5996}$; for the pressure of an atmosphere it will be 267 times more considerable, or equal to 0·00003436; the division of this number by the specific caloric of the iron referred to that of water gives the quantity of the elevation of the temperature of the iron by the pressure of an atmosphere; it is, we see, too feeble to be appreciated by our thermometrical instruments.

§ VIII.

We shall not further insist upon the consequences to the theory of heat of the results enunciated in this Memoir; but it may, perhaps, be useful to add a few words upon the employment of heat as a motive force. M. S. Carnot, in the work already cited, appears to have established the true basis of this important part of practical mechanics.

High or low pressure engines without detent (*détente*) bring into use the *vis viva* developed by the caloric contained in the vapour, in its passage from the temperature of the boiler to that of the condenser; the high pressure engines without condenser act as if provided with a condenser at a temperature of 100°. In the latter, therefore, all that is brought into use is the passage of the latent caloric contained in the vapour, from the temperature of the boiler to the temperature 100°. As to the sensible caloric of the vapour, it is entirely lost in all the engines without detent.

The sensible caloric is in part utilized in the engines with detent, in which the temperature of the vapour is allowed to sink: the cylindrical envelope, the use of which in Woulf's engines with two cylinders is to maintain the vapour at a constant temperature, though very useful to diminish the limits of the variation of the motive force acting upon the pistons, has an injurious influence as to the quantity of effect produced compared to the consumption of fuel.

To render useful all the motive force at our disposal, the detent should be continued until the temperature of the vapour be reduced to that of the condenser; but practical considerations, suggested by the manner in which the motive force of fire is employed in the arts, prevent the attainment of this limit.

We have elsewhere shown that the employment of gases, or of any other liquid than water, between the same limits of temperature, could add nothing to the results already obtained; but from the preceding considerations it follows, that the temperature of the fire being from

1000° to 2000° higher than that of the boilers, there is an enormous loss of *vis viva* in the passage of the heat from the furnace into the boiler. It is therefore only from the employment of caloric at high temperatures, and from the discovery of agents proper to realize its motive force, that important improvements may be expected in the art of utilizing the mechanical power of heat.

NOTE.

The integral of the general equation

$$\frac{dQ}{dv} \cdot \frac{dT}{dp} - \frac{dQ}{dp}\,\frac{dT}{dv} = C,$$

is, as we have seen,

$$Q = F(T) - C\,\phi\,(p, z) . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

F (T) is an arbitrary function of the temperature T, varying from one body to another; C is a function of the temperature which is the same for all the substances of nature, and $\phi\ (p, v)$ is a particular function of p, and of v satisfying the equation

$$\frac{dT}{dv} \cdot \frac{d\phi}{dp} - \frac{dT}{dp}\,\frac{d\phi}{dv} = 1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

This function ϕ may be determined in the following manner. Let

$$\varphi = \int \frac{dp}{\frac{dT}{dv}} + \varphi',$$

be substituted in the equation (2), it will be

$$\frac{dT}{dv}\,\frac{d\varphi'}{dp} - \frac{dT}{dp}\,\frac{d\varphi'}{dv} = \frac{dT}{dp}\,\frac{d}{dv} \int \frac{dp}{\frac{dT}{dv}};$$

this equation is satisfied by putting

$$\varphi' = \int dp\,\frac{\frac{dT}{dp}}{\frac{dT}{dv}}\,\frac{d}{dv} \int \frac{dp}{\frac{dT}{dv}} + \varphi'';$$

ϕ'' satisfying the equation

$$\frac{dT}{dv}\,\frac{d\varphi''}{dp} - \frac{dT}{dp}\,\frac{d\varphi''}{dv} = \frac{dT}{dp}\,\frac{d}{dv} \int dp\,\frac{\frac{dT}{dp}}{\frac{dT}{dv}}\,\frac{d}{dv} \int \frac{dp}{\frac{dT}{dv}},$$

we shall have equally

$$\varphi'' = \int dp\,\frac{\frac{dT}{dp}}{\frac{dT}{dv}}\,\frac{d}{dv} \int dp\,\frac{\frac{dT}{dp}}{\frac{dT}{dv}}\,\frac{d}{dv} \int \frac{dp}{\frac{dT}{dv}} + \varphi'''.$$

We thus see that $\phi\ (p\, v)$ is given by a series of terms, each of which is obtained by means of the preceding one, by differentiating it in respect to v, multiplying by the ratio $\frac{\frac{d\,T}{d\,p}}{\frac{d\,T}{d\,v}}$, and integrating the result in respect of p. The first term of this series being $\int \frac{d\,p}{\frac{d\,T}{d\,v}}$, it is evident that the value of ϕ may be easily obtained; substituting this value in the equation (1), we have for the expression of the general integral of the partial differential equation

$$\frac{d\,Q}{d\,v}\frac{d\,T}{d\,p} - \frac{d\,Q}{d\,p}\frac{d\,T}{d\,v} = C$$

the formula

$$Q = F\,(T) - C \left| \begin{array}{l} \displaystyle\int \frac{d\,p}{\frac{d\,T}{d\,v}} \\[2ex] \displaystyle + \int d\,p\ \frac{\frac{d\,T}{d\,p}}{\frac{d\,T}{d\,v}}\, \frac{d}{d\,v} \int \frac{d\,p}{\frac{d\,T}{d\,v}} \\[2ex] \displaystyle + \int d\,p\ \frac{\frac{d\,T}{d\,p}}{\frac{d\,T}{d\,v}}\, \frac{d}{d\,v} \int d\,p\ \frac{\frac{d\,T}{d\,p}}{\frac{d\,T}{d\,v}}\, \frac{d}{d\,v} \int \frac{d\,p}{\frac{d\,T}{d\,v}} \\[2ex] + \ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot \end{array} \right.$$

This equation gives the law of the specific calorics, and of the heat disengaged by the variations of the volume and of the pressure of all the substances of nature, when the relation which exists between the temperature, the volume, and the pressure is known.

ARTICLE XVI.

Remarks on the cause of the Sound produced by Insects in flying; by DR. HERMANN BURMEISTER, *of the University of Berlin.*

From Poggendorff's *Annalen derPhysik und Chemie*, vol. xxxviii. No. 6. p. 283.*

IT is an opinion generally entertained by natural philosophers, that the sound which insects produce during their flight arises from a vibration of the wings. This notion can have had its origin only in the circumstance that no one had taken the trouble to examine sufficiently into the mechanism which produces the sound. I feel myself the more justified in making this assertion, as I find merely incidental remarks on the buzzing of insects recorded by naturalists, who notice the phænomenon only for the purpose of illustration. Baumgærtner in his "Naturlèhre," (3rd edit. 1829, p. 229,) expresses himself thus: "Therefore an insect can produce a sound through the rapid vibration of the wings:"—and Wilh. Weber says in his essay on tubes with tongues (Zungenpfeifen), (*Leges Oscillationis oriundæ, si duo corpora diversa celeritate oscillantia ita conjunguntur ut oscillare non possunt nisi simul et synchronice, exemplo illustratæ* tubulorum linguatorum.—Halæ 1827, 4to,) Page 1: "*Insecta v.c. quædam volantia motu alarum sonum certæ altitudinis proferunt: alæ vero neutiquam in ipsis insita earumque partes ad æquilibrium repellente agitantur, sed vi extra alas posita, musculorum nimirum et nervorum.*" In both cases, therefore, the cause of the sound is referred to the motion of the wing as a vibrating body.

In the course of an investigation of the different methods by which insects produce sound, with the view of communicating them in my Manual of Entomology (vol. i. p. 509, Berlin, 1832), my attention was first directed to this subject, and I soon found that the wings have no part whatever in the formation of the sound, for the hum of the insect continues even when its wings are entirely cut away. I perceived, however, a different pitch of the sound; and remarked that the more of the wing was taken away the higher this became. The insect on which I made my experiments was *Eristalis tenax.* I have at this time no living specimen of this species at hand, but have now another dipterous insect still larger, the *Tabanus bovinus,* on which I have repeated all my experiments, and obtained precisely the same result. It appeared to

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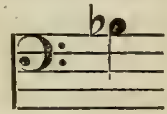
* The Editor is indebted for the translation to Mr. W. Francis of Berlin.

me, therefore, not improper to call the attention of natural philosophers to this subject by a special memoir, and in particular to give a sketch of the mechanism which forms the sound. I must next remark, that the sound which the insect emits is capable of considerable variations. It may be that it maintains an equality of pitch and strength during a uniform motion of the wings, for so in fact it appears; but every change in the velocity of the flight, every disturbance of the ordinary motion, generally causes also an alteration in the tone. An idea of the origin of the tone is however only to be obtained when the insect is held by the legs, and excited by pressure or other means to go through all its motions of the wing, and thus to produce a sound. I found in this manner that the tone of the common gad-fly (*Tabanus bovinus*) varied from  to  as the effort to extricate itself from the hands of the troublesome observer was shown with greater or less energy. Such a difference might be explained, it is true, upon the supposition that the agitation of the wing produces the tone by the varying rapidity with which the vibrations are made; but this explanation is untenable, as the same phænomenon continues when the wings are entirely cut away; an operation which produces only a variation of the tone, but does not render its formation impossible.

Before I proceed to assign the true cause of the sound, I think it necessary to give a short description of that part of the insect by which alone the sound is produced. This part is the breast or *thorax*. This consists, in two-winged insects (*Diptera*, Linn.) of a simple cavity, covered by a thin elastic parchment-like envelope, which exhibits on its surface various symmetrically arranged elevations and depressions (fig. 7. Plate V.), but is notwithstanding perfectly continuous. These elevations, the relative magnitude and form of which differ very much in different diptera, originate either in the muscles attaching themselves to the internal surface of the cavity, or in air-bladders forming continuations of the tracheæ, which stretch in these parts the coriaceous skin and make it vesicular. The largest of these elevations is the vaulted partition which forms the limit between the thorax and the abdomen (Kirby's *metaphragma*. Fig. 7. B), and to which the great dorsal muscle, of which a horizontal section is represented in fig. 8. A Plate V., is attached in the direction A B. On the middle of the back the further point of connection of this muscle forms a broad longitudinal stripe. Near to this, on each side, appear two elevations, a front one lesser (fig. 7. C), and a hinder one greater (fig. 7. E, where it appears partly covered by the wing): both originate in the lateral muscles, which are extended in the direction C D and E F through the cavity of the thorax. In fig. 8. a section of them is also exhibited, the front pair between CD and BB, the hinder pair between EF and CC. Besides these lateral muscles, there is a pair situated behind EF, and another pair in the intermediate space between CD and EF, which has a slanting position, and the whole course of which may easily be perceived in fig. 8. at DD. These muscles, namely the single great dorsal muscle and the four pair of lateral muscles, are capable of contracting the cavity of the thorax in different directions; the first only from the fore to the hind part, so as to increase the convexity of the back, the other four pair from the upper to the inferior part, so that the sides become more arched, and the back flatter. The other elevations are less interesting to us, and contain the muscles for the movement of individual organs. Thus, for example, in the elevations G at fig. 7. lie the muscles which serve to expand and elevate the wings: they are represented in fig. 8. by E E and FF: in the elevation behind A lies the muscle which serves to close the air-hole fig. 7*; in the elevations *a*, *b*, *c*, which are the hips of the three pair of thighs, are the muscles for the motion of the legs, &c. The depressions between the elevations are immaterial: there are however three of them, of which we must take a more particular notice; namely, the one wherein the further air- or respiration-hole (stigma) is situated (fig. 7*.), the second in which the hinder air-hole is perceived (fig. 7**), and the third in which the wing is placed, and which we perceive (fig. 8) in the section at the point of insertion of each wing (*dd*). Having thus far considered the structure of the thorax, we have now only to observe that the spaces between the muscles are filled up by the tubes which carry air to the tracheæ, and that the chain of nerves and the aorta, which are supported by the forked prolongation of the covering represented at fig. 8, traverse the larger space beneath the dorsal muscle of the intestinal canal.

We find as the external organs of the thorax, the wings, the poisers (*halteres*), the scales, and the legs. The *wings*, *d*, are placed in a depression on the side of the thorax, which is formed by a very thin prolongation of the exterior envelope in a slanting direction, as may be seen in fig. 7. They consist of a pocket-like folding of the epidermis, supported and extended by the horny veins. The muscles by which the wings are expanded and elevated commence at the base of those veins. Behind the wing is the *scale*, a circular pellicle (fig. 7. *g*), which is placed perpendicularly, connected with the basis of the wings, and moved by muscles of its own. Behind this, quite at the end of the thorax, are situated the *poisers* (fig. 8. *f*.), which are the rudiments of the undeveloped hinder wings, and are likewise capable of independent movement by their proper muscles. We have already mentioned that the *legs* are attached to the hips (fig. 8. *a*, *b*, *c*,); their structure is unimportant to our investigations.

The movement of the wing, on which the sound in the next place depends, is produced by the contraction of the lateral muscles; for, the covering of the back being drawn down presses on the base of the wing, and thereby the short base end of the wing, projecting freely into the cavity of the thorax, is forced downwards beneath the wing about the more outward point of support; in consequence of which the opposite end rises. This raising of the wing is then accelerated by its proper muscles of elevation E E and F F, but ceases as soon as these and the simultaneously contracted lateral muscles are relaxed. The wing now falls again, which falling is accelerated by the contraction of the dorsal muscle and the concomitant curvature of the back. Such then is the mechanism which produces the action of the wing. But at the same time this mechanism also causes rhythmical contractions and expansions of the whole thorax and of the numerous air-canals of its interior. Now the contraction drives out a part of the air; the expansion, on the contrary, causes an equal quantity of fresh air to stream through the air-pipes above described; and there is, therefore, connected with the motion of the wings, a constant, proportionally rapid, and intensive breathing; *and this breathing is the cause of the sound.* It produces the sound by the alternate efflux and influx of the air, just as the current of air sounds the æolian harp when forced at short intervals through the small holes of the sound-board, or (to take a more familiar example) by a mechanism similar to that of the mouth in whistling. The sound of the æolian harp bears a wonderful resemblance to that of many insects; and the observation that the sound is no longer heard when the air-holes of the thorax are closed, without however injuring the insect in any other manner, is decisive. It is true that the insect dies of suffocation soon after this experiment, but not directly, because the respiration through the air-holes of the abdomen continues for some time; but these emit no sound during the flight of the insect, for they are then inactive, as other observations have proved; the insect breathes through the air-holes of the abdominal part when it sits and crawls, but through the air-holes of the thorax when on the wing. According to this the hum of insects is in reality a whistle. The variation which the mutilation of the wings causes in the tone is easily to be accounted for, if we consider that by this action the moveable part becomes lighter, and also that the motion of the same, by the continued equal exertion, is quicker; but this causes a quicker current of air, which must produce a higher tone. On the contrary, a mutilation of the moveable apparatus produces a slower motion, a slower current of air, a deeper tone. I believe that the foregoing facts and observations offer sufficient evidence, but the reader can satisfy himself as to the truth of the phænomenon by performing the same simple experiments.

I have now only to add, that I have, in my Manual of Entomology, made mention of small moveable plates which are found behind the aperture of the stigma. Their presence however is not general: in the *Tabanus bovinus* they do not appear, but are found in *Eristalis tenax*. I have given an engraving of the hinder air-hole, magnified forty times, in which it is shown on the inner side. We find an oval band-like claudent muscle or sphincter, to both ends of which other muscular bands are attached: perpendicular to the inner surface of this claudent muscle stand sixteen to eighteen small horny lamellæ, which are of the same breadth as the muscle, and connected in the middle by another longitudinal horny band. On the other side, which is directed outwards, the claudent muscle is clothed with skin, upon which are feathery hairs, which cover the entrance of the air-pipe like a sieve and exclude foreign bodies. I am at present inclined to consider these small horny lamellæ rather as a mere scaffolding serving to support the claudent muscle, but leave it to naturalists to decide whether, and how far, they contribute to the formation of the sound. In either case however this can be but of little consequence, as many insects possess no such plates.

I have further to combat some objections which have already been publicly urged against the correctness of my theory of the production of the sound. M. Silbermann has given publicity to this in France by a translation of the chapter of my work which treats on this subject in his *Revue Entomologique*, by which also M. Goureau was led to perform similar experiments. The latter finds* all that I have said perfectly exact, except that the sound is suspended when the stigma is closed, and this is precisely the main point of the question. On the contrary, however, I must maintain that, although a sound may be heard as long as the gum employed to close the stigma is moist, and the air can make its way through it, none is audible when the gum is perfectly dry. The animal indeed dies of suffocation soon after. M. Goureau has not taken this circumstance into consideration, but has even hazarded the opinion that the sound originates through the friction of the edges of those plates which compose the thorax. This can however be the case in those insects only in which we find on the thorax really distinct plates connected by fibres; and as the diptera are not in possession of such, his theory cannot be applied to them. But even in respect to the other orders with skeleton hips his theory is untenable; first, because the mobility of these hips, on account of their intimate connection, must be very trifling; and secondly, because the sound is far too strong to be produced by the friction of such minute surfaces. That the feeble sound of the chirping Capricorn beetle (*Cerambycina*), when the body is in a quiet position, is really produced by such friction is well known; but it is also obvious, from the feebleness of this sound, that the loud buzzing of the flying insects, which besides sounds quite differently, cannot arise from such friction.

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* *Revue Entomologique*, by G. Silbermann, Strasb. 1835, vol. iii. p. 107.

To conclude, I have to mention in addition to the above, that MM. J. F. Schelver*, Dumeril†, and Chabrier‡, are quite of my opinion, even though Schelver differs from me in some unimportant points; however, neither their observation, nor the theory of the phænomenon, has been communicated by them with sufficient detail.

Explanation of the Figures.

Plate V. Fig. 7. A side view of the thorax of *Tabanus bovinus*, magnified eight times in linear dimension.

Fig. 8. Horizontal section of the same, in the direction between E D (Fig. 7.), at the same power.

Fig. 9. Air-hole of *Eristalis tenax*, magnified forty times.

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* *Wiedemann's Archiv*, vol. ii. part 2. p. 210.

† *Essai sur le Vol des Insects.* Paris, 1822, p. 40 *seq.*

‡ *Dictionnaire des Sciences Naturelles*, vol. i. p. 15.

[The reader may be amused by referring to The Clouds of Aristophanes, where Socrates is described as engaged in an investigation of this subject.—Edit.]

ARTICLE XVII.

Note on the Reflection of Radiant Heat; by M. MELLONI.

(Read before the Academy of Sciences of Paris, November 2nd, 1835.)

From the *Annales de Chimie et de Physique*, vol. LX. p. 402.

ALTHOUGH the researches of Leslie and Rumford established the fact that the quantities of calorific rays reflected by different bodies depend on the nature of those bodies and the polish of their surfaces, the proportion of the reflected to the incident heat in each particular case remained yet undetermined. The results presented by my experiments on the immediate transmission of radiant heat enable us to solve this question with considerable exactness.

When the calorific rays fall perpendicularly on the anterior surface of a diathermanous plate with parallel faces, they undergo a certain reflection, then penetrate into the interior of the plate, reach its further surface, and, after there undergoing a second reflection, issue forth into the air, pursuing their original direction. In certain cases there is no internal absorption, and consequently the difference between the incident heat and that transmitted, will in such cases be the value of the reflections produced at both surfaces. The substance in which this fact is most distinctly perceived is rock salt. We know that plates of this substance, when very pure and well polished, transmit 0·923 of the incident heat, whatever may be the thickness of the plates, the nature of the rays, or the modifications which these may have previously suffered in their passage through other plates.

Let us suppose, for example, two plates of rock salt, the one measuring one millimetre*, and the other ten millimetres† in thickness. According to what we have just stated, the transmissions of these two plates will be equal; and if we imagine the thick plate divided into ten layers, each one millimetre thick, the absorbent power of the nine layers that succeed the first will be of no appreciable value. Hence we may infer that, if the rays suffer any absorption, it must be in the first layer. Let us suppose for a moment that this takes place. On this hypothesis, the molecules which constitute the first layer of one millimetre in thickness will retain all the heat that is not completely transmissible by rock salt, and the quantity of heat lost in the passage through either of the two plates, that is, 1 — 0·923, or 0·077, will be but the sum of the rays absorbed or retained, and of those reflected at the two surfaces. Let the

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* 0·03937 in.

† 0·394 in.

heat, as it issues from the source, be received on one of the two plates, the thinner, for instance, and as it emerges from this let it be transmitted through the other. The supposed absorption or purification will have taken place in the first plate; and allowance being made for the rays reflected at the two surfaces, none will reach the second plate but such as are completely transmissible by its substance, so that the loss suffered by these rays in their passage through the second plate will be necessarily less than 0·077. But experiment shows that even in passing through the second plate the quantity of heat transmitted is exactly 0·923, and the quantity lost 0·077. It is clear therefore that no absorption can have taken place in the passage through the first plate, and that 0·077 is precisely the amount of loss produced by reflection at the first and the second surface of each plate.

As the nature of the source of heat has no influence on the transmission of rock salt, the calorific rays must evidently suffer the same loss (0·077) in the sum of the reflections which they undergo when entering and leaving the plate of rock salt. The same may be said of the different rays emitted by the same source, for the loss 0·077 is constant with respect to heat emerging from all sorts of screens exposed to the action of any calorific radiation whatsoever.

We may now, with the greatest facility, ascertain the proper value of each of the reflections. Let 1 represent the incident heat, and R the amount of reflection at the first surface, then 1 — R will be the quantity that penetrates the plate, and R (1 — R) the amount of reflection at the further surface; for as the rock salt absorbs none, the whole quantity 1 — R arrives at the further surface, and is there reflected in the ratio of R : 1. Now as the sum of the two reflections added to 0·923 (the quantity transmitted) must reproduce the quantity of incident heat, which we represent by unity, we have the equation

$$R + R\,(1 - R) + 0{\cdot}923 = 1\,^{*};$$

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[*] The heat reflected at the second is evidently returned to the first surface, where it undergoes in the interior of the plate a third reflection, by which it is again returned to the second surface, and a partial reciprocation of the heat between the two surfaces is thus continued for some time. It is obvious that when the plate is perpendicular to the direction of the rays, there is always a portion of heat issuing from the surface, in combination with the transmitted rays, after having undergone three, five, seven, or more reflections. Although the portions thus added to the transmission are of very little value, yet, as their number is infinite, their sum may be supposed to constitute a sensible part of the calorific effect indicated by the thermomultiplier placed in the direction of the rays. Hence it might be reasonably objected that the equation $R + R\,(1 - R) + 0{\cdot}923 = 1$ is not perfectly true, as it rests on the assumption that 0·923 is the exact value of the direct transmission. Fortunately, however, a very simple experiment, already described, furnishes a sufficient answer to the objection. Let the rock-salt lens be inclined at an angle of about 25° or 30° to the incident rays: the portions of heat which undergo the reflections represented by the odd numbers 3, 5, 7, &c., will not, in issuing from the plate,

whence we derive

$$R = 1 \pm \sqrt{0\cdot923} = 1 \pm 0\cdot9607.$$

As the first sign of the radical would lead to an absurd result in the case under consideration, it must be rejected. The reflection at the surface of the plate will then be $1 - \cdot09607 = 0\cdot0393$ to the unit of incidence, and such also will be the ratio of the second reflection to the quantity of heat which reaches the further surface of the rock salt. But if it be desired to have the absolute value of the second reflection, it will be found by substituting 0·0393 for R in the expression $R\,(1-R)$, or yet more simply by taking the difference between the numbers 0·077 and 0·0393, which gives in each case 0·0377.

It now remains to be seen whether the quantities of heat reflected by other transparent substances be equal or not equal to those produced by the surfaces of rock salt. For the solution of this question it is sufficient to observe, that a thick plate of glass, of rock crystal, or any other diaphanous substance gives a calorific transmission sensibly equal to that of another plate, the same in substance though different in thickness. If we take, for instance, one plate of glass 8 millimetres * thick, and another $8\frac{1}{2}$, and expose them separately to the radiation of a Locatelli lamp, we shall find no sensible difference between the two quantities of heat transmitted. From this experiment it is obvious that the layer of half a millimetre, which constitutes the difference of thickness between the two plates, causes no appreciable absorption of the calorific rays, which have already traversed 8 millimetres of the same substance. Let us now detach this thin layer from the thicker plate and expose it (thus separated) to the rays emerging from the plate of 8 millimetres; it will reflect part of them and transmit *all* the rest. The quantity lost will therefore represent the effect, and *only the effect*, of the two reflections. If this experiment be made with care, the number found as the quantity transmitted will be very nearly 0·923 †, and therefore the number representing the part reflected will still be 0·077. This is true not only with

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mingle with the bundle of calorific rays directly transmitted, but will be thrown laterally. If their action on the thermomultiplier be appreciable, it must be perceived in the diminution of the effect. But the galvanometer always exhibits the same deviation, both when the plate stands perpendicularly and when it stands obliquely. These multiplied reflections have, therefore, no sensible influence whatsoever on the measure of the transmission which is represented exactly by the number 0·923.

* 0·315 in.

† Are these small variations, which do not amount even to hundredth parts, to be attributed to a difference of polish in the surfaces, or to a difference of energy in their reflecting powers? This question seems not to admit of an easy solution by experiment. However, if we were to judge by the complete analogy which these phænomena bear to those that occur in the reflection of light, we should think it highly probable that the trifling differences observed partly depend on the indices of refraction in the different substances of which the plates are composed.

respect to glass, but also with respect to rock crystal, alum, fluate of lime, topaz, sulphate of barytes, &c., so that a thin plate of any of these substances, if very pure and well polished, will, when placed behind a thick plate of the same substance, always transmit 0·923 and lose 0·077.

The same numbers will be found also when the thin plate is placed behind a thick plate of a different substance, provided the latter be less permeable to the direct rays of the source. Thus a thin plate of rock crystal transmits 0·923 of the radiation from thick glass; and a thin plate of glass transmits the same proportion of the heat emerging from water or alum. The heat thus transmitted is so *purified*, that although it issues from a very thin layer, it is still capable of traversing considerable depths of glass or rock crystal without suffering any absorption. Hence it is that plates of glass or rock crystal measuring 7 or 8 millimetres in thickness will, when exposed to the rays emerging from a layer of water or alum measuring 1 or 2 millimetres in thickness, transmit 0·923, as is done by plates only half a millimetre thick.

Concluding from all this that radiant heat undergoes a reflection of about four hundredths of the incident heat which falls perpendicularly on the surface of a diathermanous substance, we perceive at once the method that is to be pursued in order to determine the quantity of calorific rays reflected by athermanous bodies. We first observe the effect of the calorific transmission through a plate of rock salt when the radiation, emitted by a constant source, is perpendicular to its faces: the plate is afterwards inclined towards the incident rays. In the quantity of heat transmitted, there appears no sensible diminution so long as the inclination does not exceed 25° or 30° around the normal. The reflection of the perpendicular rays is then sensibly equal to that of the rays forming an angle of between 60° and 65° with the reflecting plane. Now, this being supposed, let us bring a bundle of radiant heat to fall on the well-polished surface of glass or rock crystal at an incidence of between 60° and 65°, and receive the reflected portion in the interior of the tube which envelopes the thermomultiplier. After having marked the calorific force indicated by the galvanometer, let us repeat the same experiment on the polished surface of the athermanous body without making any change in the respective positions of the several parts of the apparatus. We shall thus obtain a second calorific force, differing from the first. The reflection of the athermanous body will evidently be 0·0393, which represents the value of the reflection at the surface of the rock crystal multiplied by the ratio of the observed forces.

The following are the mean results of several comparisons made between the quantities of heat reflected by rock crystal and yellow copper.

Reflection of Rock Crystal.	Reflection of Yellow Copper.	Ratios of the two reflections.	Product of the two numbers (0·0393 and 11·3).
3·15	35·63	11·3	0·4441.

By diminishing the angle of incidence which the calorific rays form with the surface of the rock crystal, an increase of reflection is obtained, especially in the small incidences. But this effect is nearly imperceptible on the metallic surface, for, in passing from 80° to 20°, I have been able to verify with the plate of brass (*laiton*) no more than a difference of 4 or 5 hundredth parts. The concentration of radiant heat by the action of metallic mirrors of any form will therefore be always inferior to that produced by rock-salt lenses of the same breadth. Thus, for example, the conical mirrors of polished brass which are applied to one face of the pile of the thermomultiplier will never give more than $\frac{444}{923}$ or about half the effect given by a rock-salt lens having its diameter equal to that of the opening of these cones.

ARTICLE XVIII.

Observations and Experiments on the Theory of the Identity of the Agents which produce Light and Radiant Heat; by M. MELLONI.

(Communicated to the Academy of Sciences of Paris, Dec. 21, 1835.)
From the *Annales de Chimie et de Physique*, vol. L. p. 418.

AMONG the hypotheses on which it has been proposed to explain the radiation of heat, there is a remarkably simple one which M. Ampère has lately modified and developed with great ingenuity. It consists in regarding radiant heat as a series of undulations produced in the æther by the vibrations of bodies possessing heat. Those undulations should be longer than the undulations which constitute light, if the calorific source were dark; but when the source is at the same time calorific and luminous, there must always be a group of waves simultaneously possessing the property of heating and that of illuminating. Viewed in this way, there would be no essential difference between radiant heat and light. A very extensive series of æthereal undulations coming into contact with the different parts of our body would produce the sensation of heat: a more limited number of these would also possess the power of exciting in the retina of the eye a vibratory movement calculated to produce the sensation of light.

No cause had been yet assigned for the quick transition of the purely calorific to the shorter waves, which are at the same time calorific and luminous. M. Ampère has found a very plausible explanation in the phænomena presented by the immediate transmission of terrestrial heat through water.

If an iron ball be heated at different times to different temperatures, and brought each time to act on a thermoscope placed behind a layer of water (either pure or charged with salt) measuring from three to four millimetres in thickness, the thermoscope exhibits no indication of heat so long as the metallic mass remains obscure, but as soon as the ball becomes decidedly red, it indicates a slight calorific transmission. Now, as the eye contains a certain quantity of watery humour, an absorption and transmission similar to that exhibited by the layer of water will take place in the interior of this organ also, which will therefore suffer none but the undulations producing luminous heat to arrive at the retina.

On the supposition that both agents are identical, it is needless to show that the calorific rays are propagated in a straight line, and that their angle of reflection is equal to their angle of incidence.

It is true that there appears a striking disparity in their manner of propagating themselves when the calorific and the luminous radiations fall upon the surface of diaphanous bodies, whether solid or liquid. In this case the medium is immediately traversed by one portion only of the radiant heat, while the other portion is slowly transmitted from layer to layer. But to a certain extent this phænomenon may be accounted for by supposing the ordinary conductible heat to be produced by a vibratory motion communicated by the æthereal undulations of every length to the anterior molecules of the medium, and then gradually propagated to its further surface*.

Considerations derived from the different lengths of the æthereal undulations will enable us to account for the two very distinct species of transparence observable in diaphanous bodies relatively to the rays of heat and those of light. Thus it will be easy to conceive why certain substances possess but very little diathermaneity, though they are perfectly clear, if it be admitted that they intercept all the obscure waves, the sum of whose intensities is greater than that of the luminous waves even in the radiation of the most brilliant flame. On the other hand, we shall perceive the cause of the diathermaneity of certain perfectly opake media, in the supposition that they allow themselves to be penetrated by particular groups of obscure undulations.

The hypothesis of identity is sufficient, no doubt, to explain a great number of general facts. It would not, however, embrace all particular cases, and, if we should proceed to a numerical examination of experiments, would even give rise to some serious objections. But I deem it useless to enter into detail on this subject, as the phænomena which I

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* The propagation of ordinary conductible heat considered as the effect of molecular vibration is essentially different from the vibratory motion produced by sound in ponderable matter, or that produced by light in the æther; for, in the slow propagation of ordinary heat, the points first heated lose their temperature only by little and little, and this temperature is always higher than that which is gradually transmitted to the rest of the body, unless it be lowered by other causes. But in the propagation of sound and light, the points first agitated immediately communicate their motion to the adjacent points, and then return to a state of repose, in which they remain until they are again set in motion by a subsequent impulse. A wave is thus formed which propagates itself with great velocity, and at any given instant there is no motion except in the point which the sonorous or luminous wave has attained. The cause of this difference, which seems inexplicable by the undulatory theory, has been the object of M. Ampère's inquiry in a memoir published for the first time in the *Bibliothèque Universelle* of Geneva (May, 1832), and subsequently reprinted in the *Annales de Chimie* and in the *London and Edinburgh Philosophical Magazine.* M. Ampère finds that the cause of the difference between the slow propagation of heat and other undulatory motions is to be assigned to the distinction which he establishes between the vibrations of the *molecules* of bodies with respect to one another, and the vibrations of the *atoms* which constitute each molecule; inasmuch as these two species of vibration may take place not only separately but simultaneously in the same points of a body.

am about to submit to the consideration of the Academy, seem to me to leave no doubt whatever that light and radiant heat are the direct effects of two different causes.

If we decompose a bundle of solar rays by means of a rock-salt prism, and measure the degree of heat proper to each band of the spectrum (proceeding from that band in which the refraction is greatest to that in which it is least), we find that the temperature increases from the violet to the red, and that even in the dark space on the other side this increase continues until it has reached a point midway between the red and the yellow. At this point a pretty rapid decrease of temperature takes place, and a total cessation of calorific action is perceptible in the obscure band, the distance between which and the red is about one third of the length of the luminous spectrum.

It is known that the refraction of solar undulations is greater in proportion as they are shorter. In the obscure part of the spectrum, we have none but calorific waves, which become shorter and shorter as we approach more nearly to the red limit. When we enter the luminous part, the shortening of the undulations still continues from the red to the violet. But it is to be recollected that according to the theory of identity each simple colour is the effect of an undulation which produces, at the same time and by the same species of vibration, both heat and light.

Now if all the parts of the spectrum are made to pass through a layer of water (from two to three millimetres in thickness) contained between two plates of glass, and the temperatures of the emergent rays are ascertained, we shall find that the maximum of temperature and the last obscure limit are both close to the red limit. These effects will be more decidedly marked if the layer of water be thicker. With a layer of about four millimetres, the maximum will be found in the red band. If we continue to increase the thickness of the interposed layer, we shall find the maximum continually take the same direction and pass successively over the different parts of the red, the orange, and the yellow. When the rays have passed through a layer of 300 millimetres, the maximum becomes stationary at the commencement of the green.

The obscure limit is found in that case much more close to the red than it is in the normal spectrum; but there is still an appreciable interval between the two, and this interval is necessarily greater when the thickness of the layer of water is from eight to ten millimetres; whence we conclude that a portion of the obscure heat emitted by the sun penetrates through a considerable depth of this liquid, and passes, no doubt, through the watery humour of the eye into the retina, without exciting the sensation of light.

But (to continue the explanation of the changes produced in the calorific and luminous constitution of the solar spectrum by the interposition of diaphanous substances,) if, instead of water, a plate of glass be employed, the same variations will take place, though on a more extended scale; in other words, the last obscure limit of the normal spectrum and the maximum of temperature approaches the most refracted part more nearly than it does when the medium is a layer of water of equal thickness*. In all cases, the ratios of luminous intensity between the several parts of the spectrum remain invariable, because of the perfect transparence of the media traversed by the solar rays.

But if the plate of uncoloured glass be removed and coloured glass substituted for it, the luminous spectrum will be entirely altered. If, for example, a blue cobalt glass be employed, not only does the orange disappear, but a great part of the green, a little of the blue, and the middle of the red band, so that the spectrum then looks like a series of zones, more or less broad and luminous, mixed with obscure bands. A finely violet-coloured glass usually effaces the orange and the yellow, and leaves but the red on one side and the blue and indigo on the other. In fine, a red glass, as it intercepts the other rays almost entirely, may be said to afford a passage to none but rays of its own colour.

Now, in examining the distribution of the heat of the obscure and the luminous bands, so capriciously coupled together in these several modifications of the spectrum, we find the calorific energy more or less diminished according to the nature of the glass interposed; but the *maximum* always remains nearly in the same position, and the temperatures

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* I have shown in my first Memoir on the transmission of heat through solid and liquid bodies how we may account for the different positions taken by the maximum of temperature in the solar spectra produced by prisms of different substances.

The above-mentioned experiments decisively prove that the position of this maximum must depend not only on the matter, but also on the mean thickness of the prism. To convince ourselves of this, we have only to take a large hollow prism, filled with water, and partly cover one of its lateral faces with an opake plate laid in the direction of its length, so as to leave the side situated towards the refringent angle perfectly free. Upon measuring the temperatures of the different zones of the spectrum, we shall see that the maximum of heat, which, when the prism is entirely free, is found in the yellow, now that one face is partially covered by the plate, approaches the last red limit and that more closely in proportion as the portion which remains uncovered in the direction of the edge is smaller. These variations are reproduced with more or less energy by employing for the construction of the prisms solid diaphanous bodies or liquids different from water; but there are no variations when we employ rock salt. Hence it is clear that this substance, which transmits all the calorific radiations of terrestrial sources with the same intensity, transmits solar heat also without producing any change in the relative intensities of the different rays. It is for this reason that I have thought it advisable to make use of a rock-salt prism in the dispersion of solar heat, and afterwards to consider the alterations produced in the relative intensities of the refracted rays by the interposition of transparent bodies.

of the successive zones constantly decrease on each side with the greatest regularity. Thus, notwithstanding the interposition of the coloured glasses, the intensity of the heat uniformly increases from the violet to the red, while the intensity of the light undergoes very irregular variations, which render a given zone sometimes stronger and sometimes more feeble than the succeeding zone.

Let us disregard that which takes place in the obscure part, and fix our attention on the alterations produced in the visible part of the normal spectrum, in which each luminous band is accompanied by a calorific band possessing the same refrangibility. On the one hand we see uncoloured media which have no influence on the luminous rays and totally alter the ratios of intensity in the accompanying calorific rays; on the other, coloured media which totally change the relative energies of the luminous, without affecting the regularity of the proportions existing between the corresponding calorific rays.

But if heat and light were both produced by the same movement of the æthereal molecules, it is evident that each reduction of force in a given ray of pure light should be accompanied by an exactly proportionate reduction in the ray of heat possessing the same refrangibility. Now the variations of intensity produced in each of the two agents by the interposition of uncoloured or coloured media, so far from corresponding through the whole of the luminous part of the spectrum, exhibit the most striking diversity. Light and radiant heat, therefore, proceed from two distinct causes*.

This being admitted, the complete separation of light from heat becomes intelligible; and such is the conclusion at which I have arrived, with respect both to terrestrial fire and the solar rays. The process of separation is exceedingly simple: it consists in causing the radiation from the luminous sources to pass through a system of diaphanous bodies which absorb the whole of the calorific, while they extinguish but a part of the luminous rays. The only substances hitherto employed by me are water, and a peculiar species of green glass coloured by means of the oxide of copper. The *pure light* emerging from this system contains much yellow, and possesses at the same time a tinge of bluish green: *it exhibits no calorific action capable of being rendered perceptible by the most delicate thermoscopes, even when it is so concentrated by lenses as to rival the direct rays of the sun in brilliancy.*

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* These two causes themselves are, perhaps, but different effects of a single cause. The conclusion which appears to me to follow so clearly from my experiments is therefore by no means opposed to the general theory of undulations, according to which light and radiant heat arise from the motions communicated to the æther by the molecular vibrations of luminous bodies and bodies possessing heat. It will only be necessary to admit that the luminous and the calorific rays are two essentially distinct modifications which the æthereal fluid suffers in its mode of existence.

Article XIX.

On the Constitution of the Superior Regions of the Earth's Atmosphere; by M. Biot.

(Read Nov. 21, 1836.)

From the *Compte Rendu des Séances de l'Académie des Sciences.*

In the application of mathematics to the phænomena of nature, there is nothing more satisfactory than to see how analysis discovers the hidden links of the chain which unites facts so widely distant from each other, that ordinary reasoning, far from being able to demonstrate, could not even suspect their connection. It is thus that the sun's parallax and the ellipticity of the flattening of the earth,—two elements which it cost arduous labours and long voyages to determine,—were found by the genius of Laplace to be results so intimately connected with the lunar motions, that their measure is to be most exactly deduced from the diligent observation of those motions. When the same geometer had improved the theory of astronomical refractions, by connecting therewith the real constitution of the terrestrial atmosphere more exactly than they had previously been, it was naturally to be expected that these two classes of phænomena would thenceforth serve to throw such light on each other, that the constitution of those layers of the atmosphere which are rendered inaccessible to us by their elevation, might be discovered by means of the measure of the refractions. Such an expectation must have been yet more confidently entertained, after Mr. Ivory had theoretically established atmospheric forms, which, by representing the refractions still better than they had been by Laplace, likewise reproduced with greater fidelity the decrease of the densities, and the temperatures near the earth's surface, where we have it in our power to observe their law. But in order that the agreement thus obtained between the refractions and the supposed constitution of the atmosphere might afford a rigorous proof that this was its real constitution, it was necessary to determine the degree of influence that any different state whatsoever assigned to the superior layers would exercise on the absolute quantity of the refraction observed here on earth. That could not, however, be accomplished by means of the differential equations, until then applied to the motion of light in the atmosphere, because they assign to the gas of the atmosphere a composition uniform throughout, and a refractive power constantly proportional to its density,—two conditions which already hypothetically limit the problem. Besides, as the analytical integrals deduced from those equations embraced the whole extent of the supposed atmosphere, they rendered it impossible to distinguish the share which the superior layers had in the total refraction obtained, and more particularly those shares which should *necessarily* be ascribed to them.

The first of these difficulties has been solved in the memoir on *Astronomical Refractions* presented by me some months ago to the Academy. The differential equations of the motion of light in a spherical atmosphere, however constituted, are there established. But the terrestrial atmosphere presents certain general phænomena which should be introduced into the equations, and which serve to limit them. Thus, the absolute smallness of the refractions at every distance from the zenith, excludes the possibility of the luminous trajectories which re-enter into themselves, and shows that those trajectories are all but very slightly curved.

Again, at the elevations which are accessible to us, we find by experiment that the refringent power decreases in proportion as we ascend, and that the depression of the visible horizon increases. Hence, it is concluded that as we ascend from the surface of the earth, the angles formed by the elements of each trajectory, with their central radius vector, gradually diminish. This species of inflection is virtually proved, at the height even of Chimborazo, where Bourguer observed refractions near the horizon; for, by a general theorem, given in my former memoir, the direction in which these refractions vary is found geometrically connected with the mode of inflection in question. Analysis now shows that this geometrical phænomenon results from the feebleness of the refringent power as compared with the density; and since, in the whole of that part of the atmosphere which is placed within our reach, the relation of these two elements is far from having attained the limit at which the phænomenon should cease, it is a necessary consequence of the law of the diffusion of the gases that it should exist at much greater elevations. In fine, even when we can no longer prove that it exists, we are able at least to assign limits beyond which it cannot extend: these limits are the result of the small altitude of the terrestrial atmosphere. The observation of the twilights proves that the height of the last particles of air which afford us a perceptible reflection of light does not exceed, even if it equals, $\frac{12}{1000}$ of the earth's semidiameter*, and the phænomena of the tails of comets show with what

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* *Note by M. Arago.*—All the determinations of the height of the atmosphere hitherto effected by observing the duration of twilight, rest on the supposition that all the solar rays which mark the limit of the phænomenon have been but once reflected, and that, after being twice reflected on laminæ of air, solar light is too feeble to produce more than an inappreciable glimmering. At

an excessive tenuity of matter the reflection is perceptible. Thus at that distance from the earth, the refringent power of the atmosphere ceases; and, this being granted, we may calculate the angle that each luminous trajectory forms at this point of emergence, with its radius vector. We then see that this angle continues to decrease from the greatest elevations at which it had ceased to be perceptible by the senses. However the extreme value of it is but slightly different from the last that can be observed; a fact which, when coupled with the free communication of the layers of the air with one another, does not allow us to suppose that the intermediate values depart abruptly, or in any considerable degree, from the order of magnitude assigned by these two limits. If we then consider any luminous trajectory whatever reaching an observer placed at the level of the sea, under a certain zenith distance, which will also be the angle formed by the trajectory with its radius vector at the point, we shall be obliged to admit that while the former ascends to a very great height in the atmosphere, its element becomes more and more inclined to the latter. The subsequent variations of this inclination, of whatever kind they may be, are always very inconsiderable, and lead to an ultimate value which, though lower, does not differ much from the values found at an inferior elevation.

This condition, which is proper to the terrestrial atmosphere, being introduced into the general differential equations, conjointly with the values of the refringent power, the temperature, the pressure and the hygrometrical state of the inferior layer, I rigorously deduce from them for each zenith distance two values of the total refraction; the one necessarily too high, and the other too low: so that the mean error is always less than half their difference. Now when the latter becomes inappreciable by observation, the total refraction is found independently of every hypothesis respecting the uniformity of constitution and constancy of the refringent power of the superior layers, which are virtually placed beyond the reach of our examination; for then all possible diversities of condition, compatible with the phænomena which we have been just now considering, can produce no change in the mean

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present these bases of calculation would be inadmissible. Experiments on polarization have shown in fact that multiple reflections contribute materially to the dissemination of solar light in the atmosphere; and that, in each direction, rays reflected several times form a considerable part of the whole bundle that reaches the eye. As to the rest, it is evident, that by introducing this new datum into the calculation, we should find the different heights of the atmosphere less than they had been found by the old method.

M. Biot's Remarks on this Note.—The less the thickness of the atmosphere, the more rapidly convergent are the developments of the refractions, and the more confined the definite limits which I have found for the refraction. According to the result announced by M. Arago, those limits will become more narrow than they are given by the numbers which I adopted from the evaluation of Delambre, and consequently the zenith distances to which those limits may be applied can be extended still further.

value, but one so minute that its amount can neither be measured, nor the fact of its existence established by our instruments.

By applying this reasoning to the meteorological circumstances which present themselves at the level of the sea, when the pressure is 0^{m}, 76 and the temperature 10° of the centesimal thermometer, I find that all the varieties of constitution that can be assigned to the atmosphere of the earth do not cause in our mean value of the total refraction a variation amounting to the following quantities, namely, at 45° zenith distance, 0″, 001; at 74°, 0″ 277; at 80°, 2″ 243. These limits increase in proportion as we descend toward the horizon; but so long as the trajectory is not excessively low, the shortness of its passage through the atmosphere, together with the smallness of its curvature, causes them to deviate nearly in the same degree from the true refraction, which is then found to differ but little from their mean. The zenith distance being, for instance, 86° 30′, the mean error is only 1″ 32, if we take as our term of comparison the very perfect table of Mr. Ivory.

We may calculate in a similar manner the refractions observable in every other layer of the atmosphere, their meteorological elements being given, and shall find analogous limits of their values. It is necessary to observe, however, that in proportion as the station of the observer is more elevated, these limits approach each other more nearly, for equal zenith distances; and their deviation may in that case be disregarded, though under the same zenith distance, they are by no means to be neglected, when the observer is at the level of the sea. It is by these means that I intend to effect the solution of the problem which I have proposed to myself. For if we consider, for example, the trajectory which arrives horizontally at the level of the sea, and cause it to re-ascend into the layers of the air, according to a law of decrease sufficiently exact to bring it back, without any supposable error, to the height at which the density is reduced to the hundredth part of its primitive value (about $\frac{5}{1000}$ of the earth's semidiameter), the angle which it then forms with its radius vector has become so small that the part of the refraction produced on the remainder of its course may be so exactly appreciated by means of our limits, that it may safely be included among the observations made at the earth's surface, for the error cannot amount to 0° 15″ for the whole refraction. The superior layers, from which this portion is derived, might therefore be constituted in any imaginable manner as to their densities and temperatures, and in a certain degree even as to their physical nature, without our ever perceiving any appreciable effect of these differences in the total refractions observed; and thus, reciprocally, the observed refractions afford no idea of those elevated regions of the atmosphere.

All that remains then is, that we endeavour to discover a law of decrease in the densities and the temperatures, such as may represent with sufficient exactness the lower part of the atmosphere, which is within reach of observation and experiment, and whose physical bases are such that this law may be mathematically extended, as an approximation, to about two-fifths of its whole height. Now this is easily accomplished, and we are led to it by the refractions themselves. For, if they are calculated on the supposition that the pressures are proportional to the first power of the densities, the value obtained is too great; but if the second power of the densities be employed, the value obtained will be too small. The true law lies, therefore, between these two limits, and an approximation to it may be obtained by taking an indeterminate expression consisting of two terms affected to each of those two powers. If this expression be subjected, as it should be, to the general conditions of equilibrium in the layers, as well as to the particular circumstances of pressure and temperature which take place in the inferior layer, and lastly to the decrease of temperature observed near the earth's surface, we obtain precisely Mr. Ivory's atmospheric constitution, with all its numerical constants determined, and identical with those which he obtained by means of other considerations*. Now Mr. Ivory has proved that this law being applied to the inferior layers perceptibly agrees with the barometrical formula which, with respect to these layers, is the faithful expression of facts immediately resulting from the decrease of the densities. We are therefore justified by this combination of identical results, in extending its application to the limits of elevation already indicated as being equal to about $\frac{5}{1000}$ of the earth's semidiameter. The rest of the refraction is then obtained independently of every hypothesis respecting the constitution of the superior layers, with a limit of error less than $0''\,15$ even for the horizontal refraction, as has been already asserted.

And it is not only advantageous, but theoretically necessary, that we should have to form no hypothesis in respect to the state of these last layers, which are unknown to us. For the law of decrease founded on the two first powers of the densities, though it adapts itself to all the

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* The expressions thus obtained differ from those of Mr. Ivory only in their including the decrease of the weight which Mr. Ivory has neglected in consideration of the small height of the earth's atmosphere. But theoretically speaking, this consideration is no longer applicable to the atmospherical system which he employs in the integration of the differential element of the refraction, because it still gives an infinite extent to the atmosphere. Mr. Ivory, no doubt, found it necessary to proceed in that manner, in order to render the analytical integrations, on which the refraction depends, practicable: but this restriction is no longer necessary when we employ the numerical quadratures, and then any expression whatsoever that represents the real state of the atmosphere may be employed without limitation. As to the equations of equilibrium which determine the ratios which the pressures and the densities bear to the height, they are always integrable, and with the same facility when the pressure is expressed by any number of terms, containing any powers whatever of the densities.

phænomena observed in the lower layers, becomes unquestionably defective toward its limits, inasmuch as it would give the atmosphere an infinite altitude, while its real altitude is certainly limited and very inconsiderable. There is here also that condition which is always introduced into differential equations, and by means of which they are limited, before the law of decrease of the densities is introduced as a function of the height. So that there is an evident contradiction in afterwards integrating them analytically, by extending this decrement even to infinity, as the law derived from the first two powers of the densities requires. Fortunately, however, the effect of this contradiction is little or none as to the total refractions, because the rapidity of the decrease, depending on the conditions of the lower layers, soon renders the refringent power insensible at a height which is yet very inconsiderable, so that the observable result is the same as it would be in an atmosphere sensibly limited. But this approximation, which is produced spontaneously, without affording any means of ascertaining its exactness, is attended also with the inconvenience of leading us to suppose that the physical state of the most elevated layers of the atmosphere is really the same as that which has been hypothetically assigned to them; while the observable results, being determined almost entirely by the total pressure of that remainder of the atmosphere, and by the conditions of its contact with the lower layers which support its weight, depend in no sensible degree on that state, and are consequently incapable of even indicating it.

Though the foregoing considerations establish the utter impossibility of some inductions which might have proved highly valuable in reference to terrestrial physics, they show us how the tables of refraction may be made more perfect, and above all more general, than they are at present. In fact, those tables have been hitherto constructed with reference to a certain given constitution of the atmosphere, and by merely changing the pressure and the temperature conformably to the indications of the barometer and thermometer in the inferior layer, they are supposed to be made applicable to all climates and seasons. But such an identity is in direct opposition to the observed physical phænomena: for instance, the decrease of temperature near the earth's surface appears to vary considerably at the same place in the different seasons of the year, and it is very unlikely that its absolute amount is the same in all situations. Now this element affects one of the most important constants of the tables; and according to a theorem which I have demonstrated, it is on this that the differences of the refractions near the horizon principally depend. It is therefore necessary to determine its variations experimentally, at different times and places, for the heights that are accessible to us; and instead of supposing, as has been done hitherto, that it is constant and everywhere the same, to take those variations into account in the calculation of the tables. The next thing to be done would be to observe the hygrometrical state of the inferior layers, and in particular to measure the amount of its diminution in proportion to the height to which we ascend; for these elements also, though in a very inferior degree, affect the same constants. The real constitution of the atmosphere up to very considerable degrees of elevation being known, the methods which I have given in my Memoir will therefore enable us to deduce the refraction numerically for the layers to which they are applied. When we have arrived at elevations at which the valuation by limits becomes sufficient, the remainder of the refraction will then be obtained by this process without any hypothesis. We shall then have tables of refraction adapted to circumstances which are really variable, though supposed in the present tables to be uniform. And, should we be thus led to discover, (a thing not at all improbable,) that these variations, or at least the most considerable of them, take place chiefly in those atmospheric layers which are not at a great elevation, it might be possible in the great observatories of Europe to observe regularly the constants of those troubled regions of the air by means of small *captive* balloons, carrying with them instruments with indicators, and the results obtained in this way might be applied as correctives to permanent tables constructed for the untroubled region. The only errors then to be apprehended would be those which might arise from an accidental alteration in the supposed sphericity of the refringent layers; such an alteration, for instance, as might be produced by a violent agitation continued for a long time in one direction. But the effects of these disturbances being thus isolated, and their extent being known, it would perhaps be not impossible to give them due attention, if they should be found to possess any constancy, and analysis would have then done for the theory of astronomical refractions all that it is allowed us to expect.

ARTICLE XX.

Remarks on the real Occurrence of Fossil Infusoria, and their extensive Diffusion; by Prof. EHRENBERG.

From J. C. Poggendorff's *Annalen der Physik und Chemie,* vol. xxxviii. No. 5, p. 213 *; with a Plate.

IN the month of April of this year I communicated to the Academy † a remarkable fact relative to the infusoria of the mineral springs of Carlsbad, namely that they appeared to be the same species as those met with on the French coasts of the Atlantic and in the Baltic. For the knowledge of this fact I was indebted to the kindness of the proprietor of the porcelain manufactory in Pirkenhammer, near Carlsbad, M. Fischer, who, at my request, brought for me to Berlin some of the water containing living animalcules. In order to follow up the examination more closely and more extensively, I requested another supply, which I received a fortnight ago in good condition. At the same time M. Fischer informed me, in a letter dated 20th June, that he himself had made a curious observation. He had remarked that the Kieselguhr ‡ (announced by M. Radig in the Jahrbücher für Deutschlands Heilquellen, &c., edited by MM. von Græfe and Dr. Kalisch, 1836, p. 193.), which occurs in the peat-bog of Franzensbad, near Eger in Bohemia, consists almost entirely of the shields of Naviculæ, and appears to owe its origin to the action of volcanic heat on the bottom of the sea. M. Fischer sent me, together with this information, a piece of this fossil siliceous body, originally rather more than 2 inches long, 1 inch broad, and ¾ inch high, which I have presented to the Royal Mineralogical Cabinet; he requested me at the same to determine the forms of the animalculæ, and to publish his observations together with my results. Microscopical examination directly confirmed the observation of M. Fischer, that the Kieselguhr of Franzensbad consisted almost entirely of Naviculæ; and the great transparency and clear-

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[*] This paper was read in the Royal Academy of Sciences of Berlin on the 7th July, 1836. [The translation is by Mr. W. FRANCIS.]

[†] Compare the Report of the Proceedings of the Royal Academy of Berlin, 1836, pp. 36, 50, and 55; and Wiegmann's Archiv. for Nat. Hist. 1836, p. 240.

[‡] [A kind of siliceous paste; from Kiesel, *silex,* and Guhr, a term used in mining for water carrying dissolved minerals when in a thick liquid state.—W. F.]

ness of the little siliceous shields made it indeed probable that an intense heat had caused their accumulation from a more voluminous combustible substance. But the opinion that they have belonged to the bottom of a sea is improbable, since the chief part of the forms, both from their figure and size, as well as from the number of their inner stripes, agree very exactly with the *Navicula viridis* now living in all the fresh water around Berlin, and widely diffused in other parts. In the sample of the peat-bog there were also to be perceived Naviculæ, which, though mostly different from those of the Kieselguhr, were still living species, and in quite a different proportion to one another, and generally in a smaller proportionate quantity in the same space.

After this the original specimens of the Kieselguhr from the Isle of France, and the Bergmehl from San Fiore in Tuscany, in the Museum of Berlin, which had been chemically analysed by Klaproth, and to which were still attached the descriptions in his handwriting, were microscopically examined. It was found that these substances also consisted almost wholly of several different forms of fossil infusoria, so that the whole siliceous contents given by Klaproth are to be assigned to the infusoria shells.

As early as the year 1834 I announced to the Academy, in the appendix to my third paper on Organization, that, after having examined with M. Henry Rose the discovery made by M. Kützing, that the shields of the Bacillaria consist of silex, this fact was fully established, not only for these, but also for other living forms; a fact which the observations of M. Fischer, and my examination of the Kieselguhr analysed by Klaproth, confirm anew.

As the interest of this phænomenon appeared to be great, I compared several other siliceous and earthy substances from the Royal Mineralogical Cabinet, which Professor Weiss had the kindness to place at my disposal, without however being able to forward the object of the research. At a fortunate moment it occurred to me that such siliceous shields might be in use in the arts as polish, like the siliceous shavegrass, *Equisetum*. I purchased therefore in Berlin several kinds of tripoli and polishing earths for examination. I examined first the common or leaf tripoli, and found at once that this also consisted entirely of the shells of infusoria. All the others were of a different inorganic nature. A comparison of this tripoli of the shops (which, as I was informed, comes from the Harz and Dresden) with the scientifically arranged species of tripoli in the Royal Mineralogical Museum, showed that this so-called leaf-tripoli is evidently the same stone which was received by Werner as a new species in mineralogy under the name of Polirschiefer (polishing slate), which it has ever since retained. The specimens at hand from the Kritschelberg, near Bilin, exhibited so perfect a similarity, as well outwardly as in the forms of the infusoria of which it consisted, that it is evident that the leaf-tripoli sold at Berlin comes from Bilin in Bohemia, through Dresden. A similar stone to this is the Polirschiefer found at Planitz near Zwickau, if indeed the locality of the specimen examined by me be correct. But the Klebschiefer from Montmartre, which Klaproth has analysed, exhibited only doubtful traces of infusoria shields. The appearance of the fossil infusoria in the form of the Polirschiefer of Bilin is plainly of great importance to our further researches into geognostical relations. In the same slate are found the impressions of an extinct fish, the *Leuciscus papyraceus* of Bronn, (according to Agassiz,) and several impressions of plants, probably belonging to the tertiary formation.

I had been inclined, even before these researches, to assign a great influence in the origin of the Raseneisen (bog-iron-ore) to an infusorium discovered by me in 1834, and of which I have, in April 1835, given an engraving in Plate X. of my Codex of Infusoria, under the name of *Gaillonella ferruginea*, which is perhaps the same as the *Hygrocrocis ochracea* of botanists. The minuteness of these corpuscles deterred me however from publishing this important circumstance; but since the discovery of so many and various shield-infusoria as stone masses, and since I have found that even the animalculæ which almost entirely form the Polirschiefer of Bilin are also a species of the genus Gaillonella, I no longer hesitate to add this observation to the rest. That the formation of the Raseneisen, or of the Wiesenerz (meadow-earth), as a continual phænomenon excites great attention, and has given rise to many but not sufficiently explanatory theories, is well known. I have every spring observed in the marshes, particularly in the turf districts about Berlin, large quantities of a substance of a very deep ochre yellow, sometimes passing into flesh red, often covering to a great extent the bottom of the ditches from one to several feet deep, and much developed in small holes and in the footsteps of animals grazing. This mass is extremely delicate, and without any consistency, dividing itself at the least touch into an indefinite number of parts. Where it has become dry, after the evaporation of the water, it appears exactly like oxide of iron, for which it has been formerly often mistaken. We perceive however under the microscope, with a moderately high magnifying power, extremely slender articulated threads, the members of which measure only $\frac{1}{1000}$ of a line, and in which the yellow colour is inherent. At the beginning of last summer I satisfied myself that these slender articulated threads do not lose their form in a strong red heat, but the colour changes to a red-brown, which is exactly that of iron-ochre. It was found that by the application of muriatic acid the colour was dissolved, without the articulated threads being changed: in the solution precipitated iron was clearly visible. There is also one of the genus Gaillonella, very similar to the Bacillaria, but a very minute organic being, containing a yellow ochre colour, in which there is probably a great proportion of iron, in the same manner as phosphate of lime is contained in the bones. By extraction of the lime, the gelatine of the bones retains, as is well known, its form: in the same manner the *Gaillonella ferruginea* possesses a siliceous shield, which retains its form unchanged after the extraction of the iron.

I have already examined with the microscope various specimens of the Raseneisen from Berlin, from the Ural, from New York, and other places, and find the extremely voluminous yellow iron oxide which is attached to them, and which perhaps has originally served to form them, to consist also of similar connected threads in rows, which resemble the Gaillonella in size, form, and colour, and which are not destroyed by the action of heat or muriatic acid, but no longer form such evident articulated threads as in the living animal. If I compare it, when its fibres are disjointed, with the *Gaillonella distans* in the Polirschiefer, I find no reason to consider the phænomenon in the Wiesenerz-ochre as a different one. I received, through the kindness of M. Karsten, the vegetable products of the mineral water of the salt-works of Colberg, in which there is a yellow earthy substance, in great quantity, formed on the surface. At first it collects at the surface of the stagnant water, as I was informed, in a greenish mass, similar therefore to the protoxide of iron. Dried and exposed to the air it remains of a beautiful ochre yellow, and on being heated it becomes of a red-brown blood-stone colour. On dissolving it in muriatic acid I found a great quantity of iron, with remains of silex. This substance consists, like the marsh-ochre, of articulated threads, which separate into single members: it resembles also very much the *Gaillonella ferruginea*. These Gaillonellæ are used in Colberg for iron-colour in house-painting. The circumstance that this production of the salt-spring collects on the surface of a yellowish green colour, and afterwards sinks to the bottom and changes into yellow, determines perhaps a special and not otherwise characterized species of the same genus*. Thus the siliceous contents of the Raseneisen, and the incombustible organic form of the minute bodies constituting the ochre which surrounds it, make it highly probable that here also an organic relation exists through infusorial formation, though only so far as to form after death, by the large proportion of iron they contain, a central point or nucleus, to which all other iron in solution immediately around it is attracted.

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* Another quantity of this mass sent from the Dürrenberg salt-works has determined this question, since it appears in this that these living animals (?) also are always yellow; that in dying they rise to the surface of a grayish green colour (protoxide of iron), and in sinking to the bottom they again take the yellow colour.

The animals which I found in the above-mentioned fossils are the following species.

I. Nine species in the stone from Franzensbad.

1. *Navicula viridis,* as chief mass; 2. *N. gibba*; 3. *N. fulva*; 4. *N. Librile,* all freshwater animalcules, very common in the neighbourhood of Berlin;—5. *N. viridula*; 6. *N. striatula*; both sea animalcules now living: the first I know of only from the Baltic, near Wismar; the second from near Havre in France, and in the mineral water of Carlsbad;—7. *Gomphonema paradoxum*; 8. *G. clavatum*: both species now common near Berlin;—9. A species of Gaillonella, *G. varians*? of which I have hitherto seen only fragments.

II. In the peat-bog of Franzensbad I found, around the Kieselguhr, five species:

1. *Navicula granulata,* as the most usual form, not occurring in the Kieselguhr; 2. *N. viridis,* rare; 3. *Bacillaria vulgaris*?; 4. *Cocconeïs undulata*; both sea animals;—5. *Gomphonema paradoxum* (*clavatum*?), still found near Berlin.

Only two forms are common to the turf and the Kieselguhr, which is found in it, and which thence probably owes its origin to a different period.

III. I found in the Kieselguhr of the Isle of France several species:

1. *Bacillaria vulgaris*? as chief mass; 2. *B. major,* an unknown species, but perhaps allied to the former, which is a well-known sea animalcule; 3. A small *Navicula,* perhaps the infant state of *N. fulva*; 4. *N. gibba*; 5. *N. bifrons,* a still living species, occurring rarely near Berlin.

IV. The Bergmehl of Santa Fiora, or San Fiore, of Klaproth's collection contains nineteen different species:

1. *Synedra capitata,* new species, as chief mass, between which 2. *S. Ulna,* an animalcule living both in fresh and sea water;—3. *Navicula inæqualis*; 4. *N. capitata*; 5. *N. viridis*; 6. *N. gibba*; 7. *N. phœnicenteron*; 8. *N. Librile*; 9. *N. Zebra*; all freshwater animalcules;—10. *N. viridula,* a sea animalcule from the Baltic;—11. *N. granulata*; 12. *N. Follis*; two yet unknown or extinct species;—13. *Cocconeïs undulata,* a sea animalcule;—14. *Gomphonema paradoxum*; 15. *G. clavatum*; 16. *G. acuminatum*; freshwater animals from Berlin;—17. *Cocconema cymbiforme,* a freshwater animalcule; 18. *Gaillonella italica,* new species; 19. Siliceous needles of a sea *Spongia,* or freshwater *Spongilla.*

V. In the Polirschiefer of Bilin, specimens of which M. Weiss had himself collected there, I found four species:

1. *Podosphenia nana*, new species, as chief mass; 2. *Gaillonella distans*, new species; 3. *Navicula Scalprum*?; 4. *Bacillaria vulgaris*? probably all sea animals.

VI. In the leaf-tripoli of the shops at Berlin, probably received through Dresden or from the Harz, were found three precisely corresponding species:

1. *Gaillonella distans*, as chief mass; 2. *Podosphenia nana*, new species; 2. *Bacillaria vulgaris*?.

VII. In the Klebschiefer from Menilmontant I in two instances found fragments of *Gaillonella distans*, but am doubtful whether they may not have been derived from the Schiefer of Bilin.

It deserves particular notice, that by far the greater number of these twenty-eight fossil species of infusoria, which all belong to the family of the Bacillariæ, and indeed to eight different genera now existing,—namely the genera Navicula, Cocconeïs, Synedra, Gomphonema, Cocconema, Podosphenia, Bacillaria, Gaillonella,—that of these twenty-eight species, fourteen were undistinguishable from existing freshwater infusoria, and five species from existing marine animals. The other nine species, therefore not quite one third, are either as yet undiscovered but existing forms, or extinct ones. It however appears to me more probable, from a comparison of my extended observations of these natural bodies, and bearing in mind the circumstance that no extinct species appear exclusively in the above-mentioned fossil relations, that the new fossil species, among which is no new genus, are not extinct, but still existing ones which have not yet been discovered.

The great mass of the specimens of these animal forms is in very good preservation: many of them are so beautifully preserved, that I have even been able to determine from them the characters of the living species more precisely; for a direct comparison of the latter showed that certain apparent characteristical distinctions are very difficult to be observed in the living ones, and have hitherto been overlooked by me. I first discovered the apertures of the Gaillonellæ in the Polirschiefer, and I now perceive them in all the species of the genus: I have never before seen the six apertures of *Navicula viridis* so beautifully*.

The great sharpness and clearness of all the outlines of all these siliceous shields plainly appears to have been produced by an extra-

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* As botanists have often regarded these forms as plants, the following reasons why they are considered as animals, which I have already often pointed out, are deserving of remark: 1. Many Naviculæ and other Bacillariæ have quite a distinct, powerful, active, crawling motion, by which they move and push aside other bodies much greater than themselves. 2. The projection of an organ similar to the foot of a snail, and whose action assists in crawling,

ordinary red heat, which has evaporated all organic (particularly vegetable) carbon; for the animals then lived, as at the present day, on plants: at a later period the soluble earths may have become separated, while the silex has better resisted all action. Werner, indeed, was of opinion that subterranean fire had formed the Polirschiefer, an opinion which has much in its favour.

There is a certain remarkable preponderance in quantity of individual species in most of the fossil infusoria whose localities have been mentioned. Thus the Kieselguhr from Franzensbad consists almost entirely of *Navicula viridis*; the mass from the Isle of France of *Bacillaria vulgaris*; that of San Fiore of *Synedra capitata*; and that of Bilin is so entirely formed of *Gaillonella distans* that the other species of animalcules are only scattered through it.

Finally, the proportion of these animals merits a passing attention. The millions of the tribe of infusoria have often been mentioned, and spoken of almost without consideration of their number, perhaps because little belief is entertained of their corporeality. They have often been regarded as drops of oil and appearances of various kinds; but since the Polirschiefer of Bilin must be acknowledged to consist almost entirely of an aggregation of infusoria in layers, without any connecting medium, these infusoria begin to acquire a greater importance, not only for science, but for mankind at large. The Kieselguhrs occur, it is said, only in nests about the size of a fist or a head, and probably may be of comparatively recent origin. With the Polirschiefer it is different; this forms widely extended layers, containing fossil plants and fishes. A single druggist's shop in Berlin consumes yearly more than 20 cwt.: the consumption therefore of infusoria as tripoli and for casting-moulds in Berlin and the environs may be perhaps estimated at 50 to 60 cwt. yearly, and thence we may in some measure infer the sale in Bilin. I hope to receive in a short time more extensive details on this subject: it is sufficient at present to say, that the infusoria supply all the requisite demands for purposes of practical utility. Passing over the share they have in the Raseneisen, the soldier cleans his arms with tripoli; the worker in metal, the locksmith and the engraver polish with

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may be directly recognised in many forms. 3. By a close examination all the apertures may be seen, which may be considered as apertures of nutrition, of generation, and of motion. 4. Internal organs may be distinguished, which may be compared with the polygastric bladders of the infusoria, and others with the crowned ovary. 5. The infusoria are propagated, besides the highly probable egg-formation, not by buds as in plants, but also distinctly by separation, a method of propagation which is wanting in all decided plant-formations, but which is observed in many decided animals. 6. Some forms, whose motion is very slow, or which attach themselves like oysters, afford no reason why they are therefore to be considered as plants. Compare the Report of the Academy of Berlin, 1836, p. 34.

infusoria, which serve also for moulds in founderies. These animals which are so useful after death, and form entire rocks, have at present a more special interest in their individuality. The size of a single one of these infusoria, which form the Polirschiefer, amounts upon an average and in the greater part to $\frac{1}{288}$ of a line, which equals $\frac{1}{6}$ of the thickness of a human hair, reckoning its average size at $\frac{1}{48}$ of a line. The globule of the human blood, considered at $\frac{1}{300}$, is not much smaller. The blood globules of a frog are twice as large as one of these animalcules. As the Polirschiefer of Bilin is slaty, but without cavities, these animalcules lie closely compressed. In round numbers, about twenty-three millions of animals would make up a cubic line, and would in fact be contained in it. There are 1728 cubic lines in a cubic inch, and therefore a cubic inch would contain on an average about 41,000 millions of these animals. On weighing a cubic inch of this mass, I found it to be about 220 grains. Of the 41,000 millions of animals, 187 millions go to a grain, or the siliceous shield of each animalcule weighs about the $\frac{1}{187}$ millionth part of a grain.

The animalcules of the Raseneisen are only $\frac{1}{1000}$ line in diameter, or the $\frac{1}{21}$ part of the thickness of a human hair, $\frac{1}{3}$ of the diameter of a globule of the human blood, $\frac{1}{8}$ of the blood globule of a frog. A cubic line of such animal iron-ochre would thus, in the same relation, contain one thousand millions, one cubic inch one billion, and one cube of nine feet diameter one drillion, of living beings. If we suppose only one fourth of this multitude to be really present, and take no notice of the other three fourths, there yet remain such enormous numbers as to merit the greatest attention.

Further Notices of Fossil Infusoria; by Prof. Ehrenberg.

From Poggendorff's *Annalen der Physik und Chemie*, vol. xxxviii. No. 6, p. 455, 1836.

It has been announced as a well-ascertained fact, that the Polirschiefer of Bilin in Bohemia, which is a member of the tertiary formation, consists almost entirely of the siliceous shields of *Gaillonella distans* and other infusoria, without any foreign cement. The recent Kieselguhr and the Bergmehl from San Fiore, which are of less geological interest indeed, consisting of larger infusoria shells, are better adapted than the Polirschiefer (whose minute animalcules require a high and clear magnifying power) to make these organic relations more apparent and convincing. The kind exertions and reports of M. Alexander von Humboldt, who lately visited the district of Bilin in his journey to Teplitz, and sent me two very rich collections of the mineral products of that district, in various states, have furnished new materials for the furtherance of my observations.

Before I speak of this valuable addition to our subjects for investigation, I may mention, that an examination of the Polirschiefer of Planitz (of which, by the friendly intervention of M. Weiss and the liberality of M. Freiesleben in Freiberg, I have been enabled to examine a specimen whose locality was quite certain,) has shown with certainty that this layer also is a conglomeration of infusoria shells. The specimen examined resembled the Saugschiefer of Bilin, and the infusoria shells of the *Gaillonella distans* are here filled with and connected by a siliceous cement, which somewhat mars the distinctness of their form; I have, however, seen some so plainly that I am convinced of the identity of these two formations. There is probably also in Planitz a more earthy form of this stone, similar to the loose Polirschiefer, which is chiefly formed of the unchanged *Gaillonella distans.*

A specimen of the Polirschiefer from Cassel, which M. Carus of Dresden had the kindness to send me, and in which he had also recognized organic forms, was particularly interesting.

I found in the Royal Mineralogical Cabinet some specimens with petrifactions of fish, the *Leuciscus papyraceus*, from the same locality. I have also lately been able, through the kindness of M. Keferstein of Halle, to examine specimens of the stone from the Habichtswald near Cassel. This Polirschiefer of Cassel contains seven different species of shield-infusoria, between which is a loose and, for the most part, siliceous cement, which cannot be plainly reduced to organic fragments. It is worthy of notice that most of the forms in the Polirschiefer from Bilin and Planitz are either extinct or as yet undiscovered; while at the same time those forms which resemble existing species, belong to such as are not very striking, and therefore less sure for the detection of their identity; but in the Polirschiefer of Cassel two of the most remarkable existing forms occur, namely *Gaillonella varians* and *Navicula viridis*: *Navicula striatula* appears also to occur in this Polirschiefer. *Gaillonella varians* and *Navicula viridis* appear both in the tertiary formation of Cassel and in the Bergmehl of San Fiore, and these have a form related to that of *Navicula Follis.* Besides 1. *Gaillonella varians*, 2. *Navicula viridis*, 3. *Navicula striatula*? 4. *Navicula Crux* (comp. *Navicula Follis* adulta), I have also found in the stone from Cassel, 5. *Navicula fulva* juv.? 6. *Navicula gracilis*? and 7. *Navicula Cari*, n. species,—three less clearly defined species: the last however is very numerous and is unknown to me. Besides these ascertained relations of the distribution of the Infusoria-schiefer as Polirschiefer, the rich parcel sent by M. von Humboldt from Bilin and the valley of Luschitz has given rise to very important observations. It consists of a small collection of minerals from Bilin, made by Dr. Stolz of Bilin, of a larger one by Dr. Reuss, and also of a great number of specimens collected by M. von Humboldt. A careful geognostical drawing by Dr. Reuss explains the position of the rock-masses of that district.

The infusoria rock of Bilin forms the upper layer (fourteen feet deep) of the Tripelberg, which (differing from the Kritschelberg, with which it was formerly confounded) is elevated about 300 feet above the level of the brook Biela. It lies on a bed of clay, which is superincumbent to the chalkmarl. Beneath these gneiss is found, as the base of all the minerals of that district. The upper masses of stone lie west of the Tripelberg on a projected mass of basalt, which forms the Spitalberg, and on the other side of which (west) Grobkalk, with many discernible petrifactions of small chalk sea animals (many Crinoideæ) lie on the gneiss. The firmer masses (Saugschiefer and Semi-opal) lie in the Polirschiefer towards the exterior upper part, the earthy below, disposed often without order in layers, the inferior ones being almost horizontal.

The particular attention paid to the Saugschiefer and semi-opal, whose numerous transitions were exposed to view, has now given the scarcely unexpected result that these also are in the closest connection with the infusoria.—The Saugschiefer is, upon microscopical observation, plainly only a Polirschiefer, whose infusoria shells are cemented by and filled with a formless siliceous matter, just as there are fossil shells both filled and empty: this produces its greater specific weight, and all its other characters. In the gradual transitions to the semi-opal we see how the cement has increased at the expense of the infusoria shells, while the small shells have decreased in quantity and in sharpness of outline.

The formation of the semi-opal in the Polirschiefer appears to be this, that it lies imbedded in it in nodules, in the most minute transitions from the Saugschiefer. A close microscopical analysis of the most varying semi-opals from Bilin, and the neighbouring valley of Luschitz, has shown that all these stone nodules, which sometimes equal flint in hardness and give sparks, consist partly of infusorial forms held together by a small quantity of transparent siliceous cement, and partly contain inclosed within them single infusoria, but of a larger size, just as amber contains insects. It is often very plainly to be seen, that the disposition of the Polirschiefer has not otherwise been altered, either by its change into Saugschiefer (cemented and permeated by amorphous siliceous matter), or by its change into semi-opal, than that by some means a part of the infusoria shells, particularly the more delicate ones, have been eaten away or dissolved, with which another part, especially of the larger forms, has been covered in an unaltered state. In this process the stratified structure remains as fully visible in the Polirschiefer as it had before been, and forms the stripes of the semi-opal. The white and less transparent stripes are mostly well-preserved layers of infusoria. It is not improbable that a dissolving medium may have acted upon the siliceous shells as drops of water or steam act on meal. The parts in contact with it were gradually penetrated, and partly dissolved and changed into opal; or the penetrating matter, producing the opal, and which occupies but a small space, has assimilated to itself a greater or less part of the empty siliceous shells. The true wood-opal, in which the woody substance is changed into opal, renders the opinion probable that a peculiar opaline mass has supplanted the decayed and dissolved parts of the woody substance, retaining however its form. We cannot easily imagine the expulsion of the siliceous shield-mass by the opal-mass, and of the latter filling its space: therefore it appears conceivable that the opal may be probably formed from the infusoria shells, simply by water or any other dissolving medium except fluoric acid, just as dough is formed of meal. Unkneaded dough contains stripes of meal,—semi-opal has often stripes of infusoria: both are hydrates.

In the semi-opal of Bilin and of the valley of Luschitz were visible, inclosed like insects in amber, 1. *Gaillonella distans*; 2. *Gaillonella varians*, particularly the larger individuals; 3. *Gaillonella ferruginea*; 4. siliceous needles of sponges. The first is mostly dissolved, at times preserved as principal mass, with the outline rather rounded off, although the connecting medium has quite a glassy appearance. The second is mostly well preserved, but rather rounded off; the third is sometimes well preserved in the buff-coloured specimens, but on account of its minuteness does not admit of a determining character. The latter however is not unimportant with regard to the question of the action of volcanic agency: it may perhaps have been deposited in the moist parts of the previously formed Polirschiefer. Upon heating this yellow semi-opal, it became red and acted as iron. The red was the articulated fibres of the Gaillonella: they could not therefore possibly have been heated in the air. The tranquil horizontal stratification of the Polirschiefer (exhibiting perhaps the yearly or periodical deposition of the layers) speaks also for a neptunian action. Hot vapours of the volcanic neighbourhood might have much contributed to the purifying of the mass, without actual fire. The semi-opal of Bilin removes all doubt as to these organic relations.

Very similar formations, with inclosed forms of organic origin, were also apparent in the semi-opal from Champigny, that out of the Dolerit from Steinheim near Hanau, and that from the serpentine formation of Kosemitz in Silesia. The microscopical bodies inclosed in this stone, very apparently of a spherical form, and *never occurring larger*, which are also attached externally to the semi-opal or hornstone from Kosemitz as a white meal, and filling out its internal cavities, might partly belong to the still existing genus *Pyxidicula.* They are quite different from the stalactitic columns which produce the round eyes in agate.

It was natural for me now to test again the flint of the chalk, which I had before often examined: and this time I employed a higher power, and therefore with more success. The black flint, which broken into small pieces is transparent, showed no evident traces of an inclosure of microscopic organic bodies, but such are easily perceptible in the whitish and yellowish opake pieces. The more rare horizontally striped specimens are very similar to the striped semi-opals. They all contain spherical and often needle-shaped bodies, at times with apertures, which can scarcely be an optical phænomenon, and which are covered by a transparent siliceous matter. There are sometimes seen in the latter, as in the *Gaillonella varians* of Cassel, radial stripes proceeding from a pierced centre to the periphery, and also somewhat plainly a separate defined shell. The chalk-like envelope and white covering of the flint does not effervesce with acids, and is therefore not chalk, but silica, as I have convinced myself; it does not appear to originate in decomposition, but is like the meally covering of a lump of dough; that is to say, it is that layer of siliceous meal (of evident organisms) which at the formation of the flint has only been touched by the dissolving or metamorphosing matter, but not completely penetrated by it. According to this the flint would be formed nearly in the same manner as the semi-opal of the Polirschiefer. The siliceous parts of the chalk would, from their specific gravity, accumulate in certain places, and form layers of siliceous Bergmehl in the chalk; in the same manner as we see in high perpendicularly cut heaps of rubbish, things of the same specific gravity, mortar, pieces of porcelain, bones, &c., arranged separately in stratified horizontal layers. If now a dissolving elastic or other fluid forced its way into the heap, those nodules must also be formed in horizontal layers and nests, which have already attracted the special attention of geologists, and of which some at times take the form of Holothuriæ and corals; the greatest number however, partly on account of their enormous volume and partly from their wholly undetermined forms, present great difficulties to this hypothesis. In the Menilite the nodule formation of a penetrating substance, itself occupying scarcely any space, and not changing the layers of the primitive mass, is particularly well seen.

I have finally to mention the examination of the precious opal of Kaschau. In some fragments both of the common serpentine opal of Kosemitz and of the precious porphyry opal of Kaschau, I saw also inclosed round bodies like those in the flint; the greatest mass was however in the interior homogeneous. I examined the matrix of the precious opal, and found that a mass similar to Steinmark (lithomarge) always immediately surrounds the nodules. This Steinmark of Kaschau exhibits however, under the microscope, a great resemblance to the *Gaillonella distans*, as it appears in the Saugschiefer of Bilin. I have, from the remarkable character of the primary formation, repeatedly examined and compared these and similar phænomena, and prefer to declare them openly than to keep them secret. I shall however continue my observations with close examination, and publish the results if they lead to any discovery, when they are sufficiently matured.

The more probable appears the proverb, partly old and partly new, *Omnis calx e vermibus*, *Omnis silex e vermibus*, *Omne ferrum e vermibus*, the more necessary it is, by continual and close examination, which cannot be the work of a day, to separate facts from opinions, and not to envelope them in mystery, but by careful observation to confine them within the probable and attainable limits which nature has assigned. We may regard as hitherto ascertained facts that

1. Bergmehl } Newest formation
2. Kieselguhr }
3. Polirschiefer .... }
4. Saugschiefer .... } Tertiary formations
5. The semi-opal of the Polirschiefer.. }

consist entirely or partly of the shells of shield-infusoria.

The following species of stone are *very probably* of the same nature:

6. The semi-opal of the Dolerit .... } Secondary and primary formations.
7. The (precious) opal of the porphyry }
8. The flint of the chalk .... }
9. The Gelberde (yellow earth) } Newest formation.
10. The Raseneisenstein .... }
11. Certain kinds of Steinmark*.

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* The examination of a boulder from the Mark (Brandenburg) which has been regarded as Schwimmstein (compare Klöden, Geognost. Mem. 1834, p. 30.) has lately proved to me that its chief mass consists of just the same detached siliceous spindles of sponges and of the minute globules (infusoria *Pyxidicula?*) which the flint boulders of the Mark inclose in great numbers. These bodies also lie in the meally covering of the flint. This Schwimmstein therefore bears the same relation to the flint as the Polirschiefer to the semi-opal, and it belongs to the chalk.

Explanation of the Figures (*Plate* V).

Fig. 1. *Navicula* (*Surirella*) *viridis*, $\frac{1}{9}$ lin. magnitude, in the Kieselguhr of Franzensbad: *a*, seen from the side surface, where the mouths of the three apertures are apparent; *b*, the same individual from the dorsal or ventral side, in which are seen all the six apertures. The stripes are internal raised bands, between which were situated the ovaries of the living animal.

Fig. 2. *Navicula* (*Surirella*) *granulata*, from the peat-bog of Franzensbad; *a*, side view; *b*, the under surface.

Fig. 3. 1. *Synedra capitata*, the chief form of the Kieselguhr of San Fiore; *a*, side surface; *b*, ventral surface. 2. *Navicula inæqualis*, side view.

Fig. 4. *Bacillaria vulgaris*? chief form of the Kieselguhr of the Isle of France.

Fig. 5. *Gaillonella distans*, $\frac{1}{384}$ to $\frac{1}{192}$ lin. thick: chief form of the Polirschiefer of Bilin (the leaf-tripoli); *a*, *b*, *c*, seen from the side; *d*, *e*, cross surfaces; *f*, apertures.

Fig. 6. *Gaillonella ferruginea*, $\frac{1}{1000}$ lin. thick; the animalcule of the iron-ochre; *a*, with the same magnifying power; *b*, two thousand times magnified. Lyngbye has regarded this animalcule as the base of his *Oscillatoria ochracea*. Oscillatoriæ are sometimes found parasitically within it; they belong however to many different genera, and Agardh has therefore rightly regarded them as not distinct species.

All the other figures are magnified 290 or nearly 300 times.

Article XXI.

On the Chemical Effects of Electric Currents of low tension, in producing the Crystallization of Metallic Oxides, Sulphurets, Sulphates, &c.; in forms frequently closely resembling the native combinations; by M. Becquerel.*

From Becquerel's *Traité de l'Electricité et du Magnetisme,* vol. iii. p. 287.

1. By means of long-continued electrical action proceeding from a single pair of plates, chemical effects more or less considerable are produced, whether the affinity of the solution for one of the electrodes adds its action to these forces or opposes them. We every day observe that nature, having unlimited time at her disposal, produces with slender means immense effects. But these means frequently escape our senses, because they have not been studied with sufficient care, and are not included in the ordinary circle of our inquiries. It is only by working on a small scale, and closely observing every step of our processes, that we have a chance afforded us of discovering any of the means employed by nature to produce the phænomena of molecular attraction. With this view let us observe some of the decompositions obtained by means of apparently feeble electrical forces.

At present it is not doubted that voltaic action may produce chemical effects; but we do not know how far this action, when it is very feeble, influences affinities, and whether, at the very moment when these become sensible, particular phænomena may not be produced, which disappear in the general effect, when we employ a pile possessing a certain energy. We know, for instance, that if two wires of any metal are plunged into a metallic solution, each of them communicating with one of the poles of a voltaic pile of sufficient energy, we always obtain at the negative wire either hydrogen, reduced metal, or oxide. But when the tension is extremely slight, does the phænomenon take place in the same manner? Do all metals possess this property in the same degree? In order to answer these questions, successive reductions must be made in the intensity of the electricity, and at the same time what passes in the decompositions must be observed.

Let us put a metallic solution (for instance a solution of copper) into a cylindrical glass, and then with the greatest care pour over it distilled or acidulated water, so that the two liquids may remain separate, the one above the other; and then immerse a plate of copper into it, we find after a few hours this plate covered with a precipitate of copper in a metallic state. Different metallic solutions gave similar results. Hence

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* For the selection of this Paper the Editor is indebted to H. J. Brooke, Esq., F.R.S. A notice of a Memoir on this subject, read by M. Becquerel to the Academy, is given in the Philos. Magazine and Annals for March 1830.

it is obvious that metals can form, with their own solutions and pure or acidulated water, currents whose electrical action precipitates the metal. In this case there are two electrical effects: the one caused by the re-action of the two liquids one on the other; the second by the action of the acidulated water on the metallic plate; it is therefore a compound phænomenon, for the actions are added or subtracted according as they have the same or contrary directions. In the case under consideration the two actions are combined. Certain thermo-electrical phænomena and simple chemical actions ordinarily disengage electricity enough to produce decompositions resembling those just mentioned.

Let us first direct our attention to the decompositions produced by thermo-electrical currents.

2. Several experimentalists have tried to decompose water with thermo-electrical currents, but in vain; for in order to succeed, they should have experimented with salts decomposable by a weak current, such as nitrate of silver and iodide of potassium, and disposed the apparatus so as to be able to determine the production of a new compound.

Let us take two wires, the one of platina the other of copper, of a certain length and about $\frac{1}{3}$ of a millimetre* in diameter; forming at one end of each wire a ring, and hook one ring on the other; the ring of the platina wire being very small, and that of the copper wire about three millimetres in diameter. If we solder the two rings, the current goes always in the same direction, from the platina to the copper, whether we heat the wire to the right or to the left of the points of junction. Let us now solder a copper wire to the free end of the platina wire, after which burn a small quantity of sulphur upon the copper ring, and then place under the platina ring an alcohol lamp, so as to raise its temperature to red heat, keeping the copper ring as cool as possible, which may be done by placing the platina wire at the extremity of the white flame, so that this may be at very little distance from the copper ring. Now if we communicate the free ends of the copper wires with the ends of the wire which forms the circuit of a galvanometer, we obtain a current of electricity of considerable energy passing from the platina to the copper.

The copper ring is made larger than the other, so that it may be less heated when the temperature of the platina ring is raised to red heat. On the contrary, if we place the focal heat on the side of the copper wire, the electrical effects are reversed; and if we substitute in place of the platina wire another copper wire, the electrical effects will still be the same. But exposing the two rings to the same temperature, there will be no effect produced. The layer of sulphur with which the copper ring is covered sensibly augments the intensity of the current.

Here then are two distinct electrical effects in a closed circuit, consisting of wires of two different metals, according as those wires are

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* $\frac{1}{3}$ of a millimetre is about $\frac{1}{75}$ of an inch; 3 millimetres is about $\frac{1}{8}$ of an inch.

soldered or only touch one another at given points. In the first case the current has always the same direction, whether we heat the wire to the right or the left of the points of junction; in the second it is not so. The only difference arises from there being in the one only simple contact, while in the other there is contact accompanied by a chemical action which determines the formation of an oxide or a sulphuret.

3. The following experiment shows the influence of chemical action in phænomena of this kind. If a piece of sulphur be burnt at one of the extremities of a copper wire which forms the circuit of a galvanometer, and the other end be placed over it at the moment that the combustion is in full power, the current of electricity which then takes place is one of the most energetic, and more intense than the one which proceeds from a simple difference of temperature.

Suppose now a tube curved in the form of a U, containing a solution of nitrate or of sulphate of copper; plunge into each branch a copper wire, communicating with the end of a wire forming the apparatus we have just described; after one hour's experiment that end which corresponds to the negative side is covered with copper precipitated in a metallic state, while the other is sensibly oxidized. Two tin wires, prepared in the same manner as the copper wires, and plunged into a solution of hydro-chlorate of tin, give the same results; that is, that wire which communicates with the negative side will be covered with crystals of tin; wires of zinc, silver and lead, plunged in their respective solutions exhibit the same phænomena.

Platina wires are without action in a solution of platina. We here perceive the influence of the chemical action which takes place between the wires and the solutions upon electro-chemical decomposition.

Platina, gold, and silver wires, plunged in solutions of lead, tin, or copper, and prepared as those above, are equally without action on them, although the current has always the same intensity.

When two silver wires are plunged into solutions of sulphate or nitrate of copper, the positive wire is always attacked by the acid, and the precipitate is not sensibly formed on the negative wire. The oxygen and the acid appear therefore in this case to be more easily transported to the positive pole than the copper to the negative pole.

Platina wires produce a precipitate in nitrate of silver as well as silver wires, with this difference that it is more abundant on those of silver than on those of platina. This difference appears plainly by immersing at the same time a silver wire rolled round a platina wire in a solution.

Thus, we see that with weak currents of equal intensity, the easily reducible metals are disposed to be precipitated more readily from their solutions upon plates of the same than upon plates of any other metal than that which enters into the solution, and which does not of itself produce a precipitate, as iron when plunged into a solution of copper does.

This remarkable fact can only be observed with an electrical apparatus of very feeble tension, inasmuch as, when this tension possesses a certain energy, the dissolved metal goes always to the negative pole, be the wire or plate of metal which is plunged into the solution what it may.

To what cause are we, in this instance, to ascribe the predilection of a metal, combined with an acid, for a plate of the same metal? The force of cohesion, whatever it may be, is the only influence to which it can be attributed; for that principle must be supposed to act with greater force on similar than on dissimilar molecules. In this case, the force of cohesion added to that of the electric current should determine the precipitation. It must not be forgotten however that the chemical action of the solution on the positive wire is also powerfully conducive to the production of the general effect.

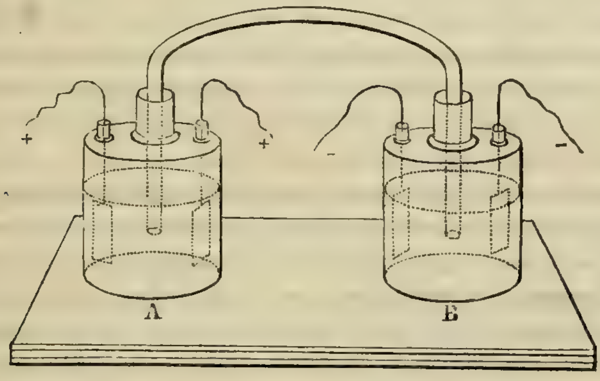
If it be desired to obtain continuous effects with the thermo-electric apparatus, the copper ring into which the platina ring is passed must be renewed from time to time, because, at a certain stage of the experiments, the copper being entirely oxidized, the continuity is broken and the electro-chemical effects consequently cease.

An apparatus with a platina and an iron wire has not sufficient action to produce decompositions. This negative effect is undoubtedly to be attributed to the singular electric properties of iron.

The apparatus we are about to describe is intended to produce slow and continuous electro-chemical actions.

Take two small glass jars (fig. 1); having poured into the one some nitric acid, and into the other some potash dissolved in water, we establish a communication between them by means of a bent glass tube filled with potters' clay moistened with a solution of nitrate of potash or chloride of sodium, and then plunge into each liquid a plate of platina fixed to the extremity of a wire of the same metal. At the free end of

Fig. 1.



each of the platina wires, the end of a wire of that metal is attached, on which the experiment is to be made. The plate in contact with the alkali receives the negative electricity which disengages itself in its reaction on the water, or the solution of the nitrate or the chloride, while the plate in the acid receives the positive electricity which escapes during the same reaction. We thus obtain a permanent pile if we only take care to close the vessels so as to prevent evaporation and the action of the air on the alkali. By plunging several plates of platina into the jars we may act on several sets of apparatus at the same time.

Water may also be substituted for potash, and a copper wire plunged into each vessel. We then obtain a chemical action, and an electric current (from the copper to the acid) sufficiently strong to produce decompositions similar to those already mentioned.

It cannot be doubted that, in the electro-chemical decompositions produced by means of currents proceeding from an electricity of low tension, the oxygen and the acid take the direction of the positive pole, as they do in the decompositions effected by means of a pile formed of several elements. The wire communicating with the negative pole is visibly covered with metal, but it cannot always be seen that the oxygen and the acid are transferred to the positive pole. In such cases they have formed an insoluble compound.

We have likewise already shown that when two silver wires, both in communication with the decomposing apparatus, were plunged into a solution of nitrate of copper, the positive end became perceptibly dimmed, while the negative end retained its metallic brilliancy, though no trace of metallic copper could be perceived on its surface. This is to be explained either by supposing that the copper, as will sometimes happen when an insoluble compound can be formed, has remained in the solution, (and in this case there has been no transfer of the elements of the nitrate although it has been decomposed,) or that the deposit on the negative end is so slight as to be imperceptible.

Let us now take two small glass vessels of a cylindrical form, the one containing a solution of nitrate of barytes, and the other a solution of sulphate of copper. We establish the communication between these solutions by means of a bent tube, small in diameter, and containing potters' clay moistened with a weak solution of sea salt, in order that the transfer of the electricity may be effected with ease. Into the sulphate we plunge the copper wire which corresponds with the negative side of the apparatus, and the other wire, into the nitrate of barytes. It is evident that if the sulphuric acid goes to the positive pole, it will, in passing through the solution of nitrate, combine with the barytes and form a precipitate.

Now the following is what actually takes place: after the experiment has been continued for four or five hours, the negative end is covered with copper, the solution of the nitrate of barytes is not perceptibly disturbed, and the positive end is oxidized. Are we to conclude therefore that there has been nothing transferred but the oxygen, and that the sulphuric acid has remained in the sulphate? The answer to this question is to be obtained only by analysing the secondary productions formed in the tubes; but there is every reason to believe that sulphate of barytes would be found. In general, when there is one of the products of the decomposition not to be found at one of the poles, we may be certain that it has been arrested, on its way, by superior affinities.

The acetates and subacetates of lead are also decomposed by means of leaden wires; but the acetate of copper and the saturated solution of the same salt in ammonia resist the action of an electricity of low tension when copper wires are plunged into their solutions. These remarks are of some importance, inasmuch as these salts are easily decomposable by the ordinary chemical processes.

Of Electro-chemical Compounds or Secondary Productions.

In stating the phænomena connected with the decompositions produced by voltaic electricity, we have directed attention to the fact that the results of these decompositions were simple or compound, according to the nature of the bodies submitted to experiment, and that of the bodies employed as conductors. We have observed, for instance, that in decomposing a solution of sulphuric acid with a piece of charcoal, serving as a positive conductor, we obtained, instead of oxygen, gaseous oxide of carbon and carbonic acid gas, in consequence of the action of the oxygen (which is in its nascent state) upon the charcoal. Other instances of the same kind have been cited in treating of the characters of the bodies developed on the metallic plates, and the definite nature and extent of electro-chemical decomposition.

We are now about to resume the consideration of this question and carry its solution as far as the present state of science will allow us, in order to show in what manner the chemical action of electricity may be applied in explaining a great number of natural phænomena which have been hitherto considered independent of this universal agent.

For a long time no one could conceive how, by means of electric forces, feeble in appearance, powerful affinities were to be overcome, for the purpose of decomposing bodies and producing new combinations. It was thought that it would be always necessary to employ currents possessing some degree of energy: but as soon as the electrical effects which take place in chemical actions were analysed, all doubt was removed as to the possibility of attaining the same end by making a proper application of those effects. It was conceived, in short, that when any voltaic pair is plunged into a solution which acts on one of the elements of that pair, the particles of the solution, at the instant when they are set in motion by the chemical action, being then in their nascent state, are most disposed to obey the action of the current produced by the pair. We have already cited examples in confirmation of this fact, but we shall have occasion hereafter to notice a still greater number of them.

Of the Formation of Metallic Oxides by Voltaic Action.

Gold.—It was long supposed that gold was converted into a purple oxide by means of electric discharges; but it appears that this state is only the effect of the extreme division of its parts, as it cannot be admitted that it is oxidized at the temperature at which its oxide is commonly reduced. It tends to confirm this conjecture, that when gold is precipitated from a very weak solution, we likewise obtain a purple powder by means of bodies which reduce its oxide.

Iridium.—By exposing iridium to the discharge of a very powerful electric battery, Children succeeded in reducing this metal to a white globule, which was very brilliant and yet porous; but he never could oxidize it.

Silver.—This metal when in contact with the air cannot be oxidized at any temperature; but it is found capable of oxidation when exposed in very thin leaves to the action of a very powerful battery.

It may be obtained also in the state of hyper-oxide, a compound for the discovery of which we are indebted to Ritter. It deposits itself on the positive conductor of a pile, when that is discharged through a weak solution of silver, in long crystalline needles crossed by three or four lines possessing a metallic brilliancy. With a slow action it is obtained in very regular and well-defined tetrahedrons. When subjected to the action of hydrochloric acid, this compound gives out oxygen, and is transformed into chloride of silver; under the action of ammonia it is decomposed and gives out azote, and when mixed with phosphorus it detonates under the hammer. It decrepitates by heat, is decomposed, and produces pure silver.

Mercury.—When a very powerful electric pile is discharged through a very small globule of mercury, the globule is driven in all directions, becomes oxidized, and produces red sparks.

Palladium is not oxided at the positive pole of the pile.

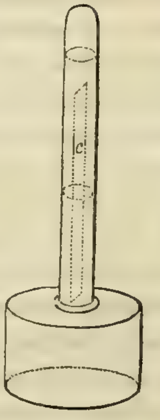
Antimony.—When a piece of antimony is employed as a positive conductor in order to decompose water, gray flakes are detached from its surface; these, under the action of hydrochloric acid, are transformed into antimony which remains, and into oxide of antimony which is dissolved. The flakes appear to be a suboxide.

Zinc and the oxidable metals are easily oxidized under voltaic influence when they are employed as positive electrodes: but as this effect presents nothing particularly worthy of remark, we shall forbear to dwell on it, that we may now proceed to explain some other processes by means of which we obtain the oxides crystallized.

Of Metallic Oxides Crystallized by Voltaic Action.

Copper.—In order to obtain crystals of protoxide of copper, we take a glass tube closed at one end and having at the bottom some deutoxide of copper (See fig. 2.). This tube is filled with a solution of saturated nitrate of copper, into which there is plunged a plate of copper, that touches the deutoxide also, and the tube is then hermetically sealed. After an interval of ten days we begin to perceive on the plate of copper small bright crystals of the form of an octahedron and of a deep red colour. In order to discover the electric phænomena by which they are produced, we must take two capsules of porcelain filled with a solution of nitrate of copper and connected with each other by means of a cotton wick. One extremity of a plate of copper is then plunged into each of them, while the other is attached to one of the extremities of the wire of a delicate multiplier. All things being now alike on one side and the other, there appears no current. But if we pour some deutoxide of copper on that part of one of the plates which is plunged in the solution, a current is soon produced, the direction of which shows that the plate in contact with the deutoxide has received the negative electricity. Hence it follows that the plate in the other capsule is the negative pole of the small pile which produces the decomposition of the nitrate of copper. Now, an effect perfectly similar to this takes place in the tube: the part of the plate in contact with the deutoxide is the positive and the other part the negative pole. We shall presently revert to the cause which produces this pile. The existence of the latter being established, the portion of the plate of copper which is not in contact with the deutoxide, should attract the copper in a metallic state or its oxides, according to the force of the current. It is therefore but natural that the protoxide of copper should take that direction if the current possesses sufficient energy. It crystallizes, because the electric and (consequently) the chemical action being very slow, the molecules have time to arrange themselves according to the laws of crystallization, although the body is insoluble,—an advantage which is never obtained when the chemical forces are of greater intensity.

Fig. 2.



According to the greater or less quantity of deutoxide of copper inclosed in the tube the phænomena which take place will vary. Let us suppose that the tube contains it in excess: at first protoxide is produced and crystallized; the solution gradually loses its colour; then becomes colourless, and crystals of nitrate of ammonia are seen on the interior surface of the tube. The liquor now contains nothing but the saturated solution of this salt and some traces of copper. Sometimes it takes a year or more to obtain this last result, which depends on the quantity of deutoxide employed. All this is effected without any contact with the air, since the tube is hermetically sealed, and the formation of the ammonia must have been owing to the hydrogen of the water and the azote of the nitric acid.

When the quantity of the deutoxide is very small, the effect is as follows: the protoxide crystals are formed equally on the plate of copper; but by little and little, they lose some of their brilliancy, and experience at last a discoloration which stops at a certain point. The solution remains always coloured. The experiment is then terminated, and time produces no change in the solution.

In order to explain the facts just mentioned, and to reascend to the cause of the electric phænomena by which they have been produced, we have found it necessary to analyse the octahedral crystals and the substance that replaces the deutoxide of the same metal. The change which the deutoxide undergoes is the only thing that can throw a light on the origin of the electric effects.

Those crystals possess the following properties: their powder is red: it is soluble in ammonia or in hydrochloric acid without colouring either. The latter solution is made turbid by the water, and receives a blue tinge from ammonia. These characters indicate that the crystals really are protoxide of copper.

Analysis of the Substance which replaces the Deutoxide of Copper in the Crystallization of the Protoxide.

We took two grammes of this substance. After having well washed and dried it, we proceeded to operate on it immediately by means of carbonate of potash. The filtrated liquor was gradually saturated with sulphuric acid, until there was no longer any alkaline reaction. Having now condensed the solution by evaporation and produced crystallization, we obtained $1^{\text{gr}}\cdot 0$ of nitrate of potash besides the mother-water which we neglected.

The insoluble salt which remained on the strainer was carbonate of copper, which being dried and weighed amounted to $1^{\text{gr}}\cdot 6$. Now one gramme of nitrate of potash, if we admit that an atom of this salt includes two atoms of acid and one atom of base, will contain $0^{\text{gr}}\cdot 5$ of acid and 0·45 of potash.

In like manner the carbonate of copper, being formed of an atom of deutoxide of copper and an atom of carbonic acid, gives 1·2 of oxide and 0·4 of carbonic acid.

Hence it follows that the substance subjected to analysis is a subnitrate consisting of

	Calcul. result.		Result of exper.
2 atoms of nitric acid	0·5	...	0·62
3 atoms of deutoxide of copper ...	1·2	...	1·37

It appears, by this analysis, that the deutoxide of copper is transformed into subnitrate of copper. This result enables us to explain the electric effects which give birth to the protoxide of copper and its accompanying products.

The hermetically sealed glass tube contains some deutoxide of copper, a saturated solution of nitrate of copper, and a plate of copper in contact with both. As the deutoxide takes possession of part of the acid of the nitrate, it follows that the part of the plate which touches the deutoxide comes into contact with solution of nitrate of copper, which is less saturated than that into which the upper extremity is plunged. A current should result from that circumstance, since the plate of copper is plunged into two solutions saturated in different degrees.

The upper extremity is the negative, and the lower extremity the positive pole: the former should consequently attract the copper or its oxides, and the latter the acid; and this is precisely what does take place. It appears then that there is nothing perplexing in the fact that the protoxide of copper is deposited on the upper part of the plate. The action of this pile must be extremely feeble at first, inasmuch as the deutoxide, especially when it is anhydrous, not acting easily on the acid of the nitrate, the difference between those two liquids is very small. But as the nitrate gradually loses its acid, which is not suddenly replaced by that of the upper part, it follows that the difference between the degree of concentration of the two solutions increases. The chemical action of the pile should be in the same ratio. Thus at the close of the operation we perceive crystals of copper especially on the upper part. As this process is gradual, all the bases from the deutoxide to the metal, should be obtained in a crystallized state, those however being excepted which are capable of reacting on the nitrate of copper.

It is proved by experiment that during these different actions no gas escapes. For this purpose all that is required is, that instead of closing the tube we should cover it with another tube inverted, and likewise filled with a solution of nitrate of copper. We find that, whatever may be the duration of the experiment, no gas is disengaged in the upper part. It appears that the oxygen which results from reducing the deutoxide of the nitrate into protoxide, is carried to the lower part of the plate (which is the positive pole), in order to oxidize it, so that it may combine with the acid which is also drawn thither by the action of the current. But as there is a formation of ammonia, it is likewise necessary that a portion of the water and the acid should be decomposed, in order to obtain hydrogen and azote. As to the oxygen, it oxidizes the lower part of the plate. The decompositions take place in such proportions that all the elements arising from them are employed to form new compounds. Thus the copper decomposes water and acid only in such quantities that the hydrogen and the azote may be in that proportion which is requisite to form ammonia. This instance of electro-chemical decomposition in definite proportions we made known several years since.

The deutoxide, by its action on the solution of the nitrate, is so far the cause of the electric current established in the system, that the same effect may be produced by putting things in the state in which they are subsequently to this action. We take two porcelain capsules, one of which is filled with a saturated solution of nitrate of copper, and the other with the same solution and an addition of water. The communication between them is established by means of a cotton wick, and the end of a plate of copper is plunged into each of the vessels. This apparatus is the same as that of the tube when the deutoxide of copper has begun to combine with part of the acid of the nitrate; since, in the one case as well as in the other, the two ends of the plate are plunged into solutions of nitrate of copper possessing different degrees of concentration. Now, as in both cases the electrical effects are the same, the explanation we have given is necessarily exact. The foregoing facts enable us to modify, at pleasure, the intensity of the small piles employed to bring into action the mutual affinities in bodies. In fact, a plate of copper immersed in two solutions of nitrate of copper, of which one is and the other is not saturated, will constitute a pile. It follows from this, that, if the solution which is not saturated be more or less diluted by the addition of water, we shall have electro-chemical actions of greater or less energy; and, as the solution may be progressively diluted, those actions will be increased or diminished in the same proportion.

It is thus that we may obtain the different oxides of a metal in a crystallized state and distinguish the proximate principles in organic compounds.

Lead.—In order to obtain the crystallized protoxide of lead, we take a glass tube measuring some millimetres in diameter, and closing it at one end, place at the bottom some pulverized litharge about a centimetre[*] high. We then pour over it a slightly diluted solution of subacetate of lead, and plunge into it a plate of lead which is equally in contact with the litharge. The tube is then hermetically sealed. The surface of the plate becomes gradually covered with small prismatic-needles of hydrate of lead; occasionally there is to be seen reduced lead; and lastly, but rather rarely, a deposit of very clear dodecahedral crystals of protoxide with pentagonal faces, which lose their transparence

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[*] A centimetre is about $\frac{4}{10}$ of an inch.

in contact with the air. These products are undoubtedly the result of the decomposition of the subacetate of lead arising from actions analogous to those which take place during the production of the protoxide of copper.

Zinc.—The surface of this metal when in contact with the air is usually covered with a very thin layer of suboxide which is opposed to electro-chemical action. This compound is slowly dissolved by the acids, and it is to its presence that we are to ascribe the feeble intensity of the currents produced by a pile which is charged with a non-acid solution.

The following is a very simple mode of obtaining crystallized oxide of zinc: we take two small bottles, one containing a solution of zinc in potash, and the other a solution of nitrate of copper. The communication between them is established by means of a bent tube filled with potter's clay moistened with a solution of nitrate of potash. A plate of lead communicating with the positive pole of a pile composed of two or three elements, is immersed in the solution of zinc, and a plate of copper, in communication with the negative pole, is immersed in the solution of nitrate of copper.

A pile may even be dispensed with, if the plate of lead and the plate of copper be put in metallic communication with each other.

The nitrate of copper is decomposed in consequence of the action of the current proceeding from the action of the alkali on the lead: the oxygen and the nitric acid are transferred to the plate of lead, and there produce nitrate of potash and oxide of lead which is dissolved in the alkali. After the experiment has been continued for some days there are found deposited on the plate of lead small clear crystals having the shape of flat prisms and so disposed as to form rosettes. These crystals are formed during the slow precipitation of the oxide of zinc by the oxide of lead, which gradually saturates the solution of potash. In contact with the air, they become gradually translucid. Exposed to the action of heat they take a yellow tinge without being melted, and become white when cooled again; a property which is characteristic of the oxide of zinc. Subjected to the action of acetic acid they gave an acetate of zinc which the sulphuret of potash throws down a white precipitate,—a proof that these crystals contain no lead.

If we substitute a plate of zinc for a plate of lead, the only deposit is a white substance, which is a combination of zinc and potash.

If we continue to let the pile operate with the plate of lead, a yellow powder is precipitated, which is probably a combination of anhydrous protoxide of lead with potash.

By substituting for the plate of zinc a plate of copper or platina, or any metal not easily oxidable, there will be found deposited on the upper side a tritoxide of lead in very simple layers which are easily detached and have all the properties of the brown oxide. We shall have occasion elsewhere to return to this compound.

Lime.—It is known that the solution of hydrate of lime in water becomes, when in contact with the air, covered with a pellicle of carbonate of lime, and that, if this solution be reduced one half by evaporation in a basin and left to cool slowly, the earth becomes crystallized in the form of small needles. M. Gay-Lussac has found that when the evaporation takes place in vacuo, the hydrate of lime is crystallized in regular hexahedrons. It is perfectly easy to obtain the same crystals by means of the pile, without operating in vacuo. Nothing more is required for this purpose than to pour some Seine water, which contains a certain quantity of sulphate of lime, into the two branches of a bent tube (U) having its lower part filled with moist clay, and then to plunge into each branch a plate of platina in communication with a pile of fifteen pair of plates. Not only is the water decomposed, but the sulphate of lime also. The water in the negative branch becomes alkaline, thus showing that it contains lime in solution. As the operation is not interrupted, there arrives a certain moment when the crystallization of the hydrate of lime is effected. If the salt with a calcareous base was more abundant, the quantity of lime that would find its way into the negative branch of the tube could not fail to disturb the regular grouping of the molecules. There can be no doubt that, by this process, several hydrated oxides, both alkaline and earthy, may be obtained in a crystallized state.

Action of Hydrogen on different bodies, serving as Negative Conductors; Formation of Metallic Chlorides.

When the hydrogen arrives at the negative pole, it usually contributes to the reduction of the oxide by instantly forming with its oxygen a portion of water, which is afterwards decomposed by the action of the current. If there are any elements present with which it may combine, the combination will undoubtedly take place, since the gas is in its nascent state. We shall now proceed to notice some circumstances of this kind.

A combination of gold and hydrogen is a thing unknown to chemistry. It has nevertheless been advanced by Ritter that in decomposing water with gold wires there was formed at the negative pole a hydruret of this metal. We mention this result without vouching for its accuracy.

It has been asserted also that by the same means silver might be combined with hydrogen, but the fact has not been yet proved.

Bismuth has likewise been said to combine with hydrogen when that metal served as a negative conductor in the decomposition of water. In this case the metal becomes black and is covered with a black dendritic substance.

Ritter has likewise asserted, that if, for the purpose of decomposing water, we employ a fragment of tellurium as a negative conductor, the hydrogen which comes into contact with it combines with the metal, and produces a brown powder or hydruret of tellurium. But M. Magnus, who examined this product, ascertained that it was composed of tellurium in its state of greatest division.

Hydrogen and carbon, which combine in different proportions when they are in their nascent state, (since all animal and vegetable substances in decomposition give out carbonated hydrogen,) must also be capable of combining at the negative pole of the pile. This property is of great importance in electro-chemistry, especially when it is required to deprive a body of its carbon. The following experiments will serve to show the use that can be made of anthracite, that is to say, of carbon almost pure, and of ordinary carbon, in the researches in which we are engaged.

When we plunge into an acid, in contact with a metal, a piece of anthracite or charcoal, a current is produced, the direction and intensity of which depend on the chemical action of the liquid on the charcoal and the metal. Let us, for instance, take a piece of charcoal freed from all foreign matter, and attach it to one end of a platina wire in communication with the multiplier, and then plunge it into nitric acid, which also communicates with the galvanometer by means of another platina wire. We then have a current from the carbon to the acid: this result shows that the carbon has been attacked by the acid.

A pair of carbon and copper plates plunged into hydrochloric acid, determines a current, which proceeds from the copper to the carbon, in consequence of the slow action of the acid on the metal. A pair of carbon and silver plates acts in a similar manner: whence we derive a very simple process for forming chloride of silver and copper. Into a glass tube closed at one end, we pour concentrated hydrochloric acid, and plunge into it a plate of silver, attached by a wire of the same metal to a piece of anthracite or charcoal. The tube is then almost totally closed, only a very small aperture being left, in order to afford a vent to the gas which escapes in the reaction of the bodies upon each other. The following is the result: the silver being the positive pole of the small pile, attracts the chlorine and combines with it, while the hydrogen goes to the carbon, with which it forms a gaseous combination, which escapes. When the tube is hermetically sealed, the tension acquired by the gas soon causes it to burst. The chloride of silver that had been formed is dissolved, and, when the acid is saturated, this compound crystallizes in beautiful translucid octahedrons of one or two millimetres in length. If, for the plate of silver, a plate of copper be substituted, the chemical reaction produces electric effects which increase the energy of the affinities; the hydrochloric acid is decomposed, and there is a disengagement of carburetted hydrogen. In six months or a year after this, the plate is covered with fine tetrahedral crystals of protochloride of copper, exceedingly brilliant and possessing great refrangibility. If the experiment be continued so as to exclude all contact with the air, the liquor changes its colour to a deep brown and the crystals are no longer visible. The carbon is then powerfully attacked, and the result is, a combination which has not yet been examined.

Of the Action of the Pile on the Alkaline Sulphurets.

When a solution of protosulphuret of potassium or sodium is exposed to the air, the metal and the sulphur are simultaneously oxidized; there is produced a hyposulphite, in which the acid and the base contain an equal quantity of oxygen: the solutions of the other sulphurets undergo other changes; so long as the solution retains a yellow tint, the hyposulphite is all that is formed, but as soon as the sulphur is precipitated, this salt is changed, first into a sulphite, and then into a sulphate.

The alcoholic solution of sulphuret of potash produces likewise, in contact with the air, hyposulphite of potash, which crystallizes at the surface of the liquid, at the same time that the sulphur, dissolved by the alcohol, is abandoned. The other alkaline sulphurets act in the same manner.

As to the sulphurets of barium and strontium, their solutions, in contact with the air, undergo changes somewhat different: though there is a formation of hyposulphate, there is none of hyposulphite.

When, in one of the branches of a tube bent like the letter U, (the other branch containing water,) a solution of one of the preceding sulphurets is submitted to the action of the positive pole of a pile composed of a small number of elements, by means of plates of platina, while the water is in communication with the negative pole; the solution undergoes changes similar to those which take place in the air. In fact, the solution of protosulphuret of potassium or sodium in water is affected, at first, in the usual manner by the positive pole; that is to say, a portion of it is decomposed, and there is a transfer of potash or soda into the negative side of the tube, while the sulphur left alone on the other side combines with one portion of the disengaged oxygen, which transforms it successively into hyposulphurous acid, sulphurous acid, and sulphuric acid, which combines with the base; for, after some days, we find only a sulphate. If the operation be continued, this last salt will itself also experience the decomposing action of the pile. The same phænomenon is exhibited by the solution of the persulphurets, except that there is a precipitation of sulphur.

The alcoholic solutions of the protosulphurets do not produce any particular phænomenon. As to the solution of a persulphuret, in addition to the formation of the sulphate, it presents on the plate of platina a fine deposit of crystals of sulphur, possessing great regularity of form, and admitting an increase of their dimensions by changing the solution when it is no longer saturated. The perfect crystallization of the sulphur is partly owing to the power which the alcohol possesses of dissolving a certain quantity.

The solutions of the sulphurets of barium and strontium being exposed, like the preceding solutions, to the action of the positive pole, produce, after some few moments, a deposit of sulphur and quadrilateral prisms (which are nearly regular and unalterable in the air), of hyposulphate of barytes or strontia. By continuing the operation these crystals are decomposed.

So great is the tendency of the protosulphurets of barium and strontium to undergo a change into hyposulphates, that when a plate of lead or copper is substituted for one of platina, we still have a hyposulphate, and only a small quantity of sulphuret of lead or copper deposited.

If we operate with a solution of persulphuret of barium, we shall have a precipitation of sulphur in the form of small tubercles, and a formation of hyposulphate. Hence it would seem that the sulphur and barium are contained in the protosulphuret in the proportions required for the oxygen arising from the decomposition of the water, and transferred to the positive pole, to be so divided between the two elements as to form the hyposulphate. All the superfluous sulphur contained in the persulphuret, being unable to enter into the combination, is necessarily abandoned, and, as it acts the part of an electro-negative element, is naturally carried to the positive pole.

It is perfectly obvious that all the chemical actions which give birth to these compounds can take place only under certain electric influences of small energy; for if we operate with an apparatus the action of which is too powerful, we isolate all the elements, and no combination is possible. The whole art consists then in disposing the apparatus in such a manner as to prevent the transfer of certain elements, and thus force them into combination with other elements which are conveyed to them by means of the electric currents. This new mode of producing combinations is fruitful in the variety of its applications, and promises results of great importance to chemistry.

In the foregoing experiments the sulphuret was submitted to the action of the positive pole: let us now see what will be the result when it is in communication with the negative pole. We will take the sulphuret of barium: in this case the hydrogen reacts upon this compound; whence there arises a sulphohydrate of barytes, as will be satisfactorily proved if we test the solution.

If it be desired to obtain this substance in a crystallized state, we must add to the solution one half its bulk of alcohol: as it does not dissolve in this liquid, it becomes crystallized in considerable quantity on the plate of platina as fast as it is formed. If we dissolve these crystals, we find again all the characteristics of the sulphohydrate of barytes.

Of Double Chlorides, Double Iodides, Double Bromides, Double Sulphurets, Double Cyanurets, &c.

It was conceived that by the application of the principles just explained, it would be possible to obtain in a crystallized state some double insoluble combinations, which by the ordinary processes of chemistry it would be difficult to produce, for want of sufficient slowness in their operation, and because they do not enable us to abstract, at pleasure, any given element of a body, or to add others. Let us first direct our attention to the chlorides.

We take a tube bent into the form of the letter U, having its transverse part filled with clay moistened with water: in one of the branches we put nitrate of copper, in the other a solution of the chloride which it is proposed to subject to experiment, for instance chloride of sodium. The end of a slip of metal (copper, for example,) is plunged into each of them, and kept there by corks. Immediately afterwards, in consequence of the reaction of the two solutions on each other, and that of the solution of the chloride on the copper, the end immersed in the solution of the nitrate becomes the negative pole of a small voltaic apparatus, and is covered with copper in a metallic state: the nitric acid and the oxygen are transferred into the positive branch, where both concur in the production of those chemical reactions which we are now about to describe. The plate of copper immersed in this branch tends immediately to decompose the chloride; but, in consequence of the voltaic action, it is oxidated at the expense of the oxygen transferred. The oxide of copper thus formed combines immediately with the chloride of copper and chloride of sodium; whence we obtain an oxychloride of copper and sodium. By little and little this combination is formed, on the positive plate, into distinct tetrahedral crystals. If it is desired to have crystals of two or three millimetres in size, the apparatus must be left in operation for at least a year. The success of the experiment depends on our preventing the mixture of the liquids contained in the branches of the tube, without impeding the transfer of the oxygen towards the positive pole. The nitric acid contributes not only to oxidize the copper but to decompose the sea salt; for there is found in the solution some nitrate of soda. We have said that this combination is effected only in proportion as the end immersed in the solution of sea salt is slowly oxidized; for it does not take place when we employ an intense electric current. The most effectual mode of oxidizing a metal, in electro-chemical researches, is to dispose the apparatus in such a manner that it will seize the oxygen which proceeds from the reduction of an oxide.

This double oxychloride, withdrawn from the contact of the air, remains unchanged; but as soon as it is in contact with water it is decomposed, the chloride of sodium being dissolved, and the oxychloride precipitated. Being anxious to analyse the latter product, in order to know its nature, we proceeded in the following manner. We took two grammes of the precipitate, after having well washed it, and digested it in a warm solution of carbonate of soda. The precipitate, when washed and dried, gave 2 grammes of carbonate of copper, consisting of 1·70 of oxide of copper, and 0·94 of carbonic acid. The oxychloride contained therefore 1·70 of oxide of copper, and 0·30 of hydrochloric acid,—a proportion which represents 2 atoms of oxide of copper and 1 atom of hydrochloric acid. We afterwards saturated the solution with sulphuric acid, and then caused it to crystallize. The crystals of chloride of sodium furnished the quantity of chlorine contained in the substance subjected to experiment.

The chlorides of ammonium, calcium, potassium, barium, strontium, and magnesium will give, with copper, analogous products, which also crystallize in regular tetrahedrons. They are all isomorphous.

Silver, as well as lead, will also give, with the same chlorides, isomorphous combinations similar to the preceding. The double chloride of potassium and tin crystallizes in prismatic needles, and this we should expect, since the atomic composition of the chloride of tin is not the same as that of the earthy or alkaline hydrochlorates.

We must here notice an observation made respecting the changes sometimes produced in the crystallization of double chlorides. The crystal is complete at first, but when the apparatus has been a long time in operation, the angles of the crystal become gradually truncated. It would seem from that circumstance that when the particles of the substance which crystallizes are not sufficiently abundant, the force which determines the regular grouping of these particles has no longer the energy requisite to complete the crystal. We have had frequent occasion to make the same remark in several crystallizations of products formed by the aid of electric forces emanating from a single pair.

By making use of the same apparatus we may form double sulphurets, double iodides, double bromides, &c. The observations which we are about to make in respect to the metallic iodides and sulphurets alone will throw some light on the rest of those substances.

On the Crystallization of Metallic Sulphurets.

Chemistry has been hitherto unable to obtain, in the moist way, the metallic sulphurets in a regular form; but this may be accomplished by uniting the action of the affinities to that of the electric forces, and so disposing the apparatus that it may be able to operate slowly and for a long time. We shall now notice in succession the several sulphurets which we have prepared.

Sulphuret of Silver.—A saturated solution of nitrate of silver is poured into one branch of the bent tube, and into the other a solution of hyposulphite of potash, obtained by decomposing in the air some protosulphuret of potassium. One end of a wire or plate of pure silver is then plunged into each of them. The reaction of the two solutions on each other, and that of the hyposulphite on the plate of silver, produce electrical effects, in consequence of which, the plate immersed in the nitrate becomes the negative pole of a voltaic apparatus. The nitrate of silver is slowly decomposed, the plate immersed in it is covered with silver in a metallic state, while the oxygen and the nitric acid pass into the other branch, where they concur in the formation of a double hyposulphite of silver and potash, which crystallizes in beautiful prisms; but as the oxygen and the acid continue to arrive, they react on this combination and the hyposulphite of potash: there are then formed sulphate and nitrate of potash, and sulphuret of silver, which remains unchanged so long as there is not a quantity of nitric acid sufficient to act on it. The formation of the sulphate and the nitrate of potash is easily explained; but the case is far otherwise with the formation of the sulphuret. Let us consider the circumstances by which it is accompanied. In proportion as the liquid evaporates in the positive branch, we see at the bottom of the tube and above the clay some pretty octahedral crystals of sulphuret of silver formed on the plate of silver. These crystals resemble, in appearance, those of the same substance that are found in silver mines. Like them, they extend themselves lightly under the hammer; their colour is a leaden gray, and their exterior surface is dim. The resemblance, indeed, is so close that the artificial cannot be distinguished in any respect from the natural crystals.

Why is it that, in consequence of the reaction of the oxygen and the nitric acid on the hyposulphite, we obtain a sulphuret of silver instead of a hyposulphite, a sulphite, or even a sulphate? This question cannot be answered but by supposing that the positive pole acts on the oxide of silver and the hyposulphurous acid so as to disoxidize them; when the silver and the sulphur, being in their nascent states, obey their mutual affinities. As these effects are produced slowly, there is nothing to oppose the regular grouping of the particles of sulphuret of silver. Nothing of a similar kind is obtained with a solution of sulphuret of potassium. In this case the results of the experiment are, sulphate of potash and sulphate of silver. This is very probably to be ascribed to the influence of the proportions and the energy of the action.

Sulphuret of Copper.—In order to apply the foregoing principles to the formation of other sulphurets, and first to that of sulphuret of copper, let us substitute for the solution of nitrate of silver a solution of nitrate of copper, and for the plate of silver a plate of copper: there is quickly formed in that side of the tube which contains the hyposulphite of potash, a double hyposulphite of copper and potassium, which crystallizes in very fine silky needles. This double hyposulphite is gradually decomposed, and we obtain at last, on the plate of copper, flat opake crystals with triangular faces two millimetres* in length.

These crystals are of a metallic gray colour, and some of them exhibit tints of a bluish cast: their powder is blackish. They are soluble in ammonia, to which they give a blue colour, and it is easy to perceive that they are composed only of sulphur and copper. Hitherto there has not been a sufficient quantity of this sulphuret collected to determine the relative proportions of the metal and the sulphur.

Oxysulphuret of Antimony.—In order to prepare the kermes, the same liquids are employed as in the preceding experiment, and the communication is established between the two tubes by means of an arc, composed of two plates, the one copper and the other antimony; the plate of copper being immersed in the nitrate, and the plate of antimony in the hyposulphite. The first becomes covered with copper, proceeding from the decomposition of the nitrate, while the other, as well as the sides of the tube, becomes covered with a reddish brown precipitate. Some time afterwards there are formed, on the antimony, small red octahedral crystals, and crystallized plates of the same nature as the precipitate. These crystals may be dissolved in neutral hydrosulphate of potash, and give out sulphuretted hydrogen by the action of the hydrochloric acid in which they are dissolved. They are made yellow by the alkalis. All these are characteristic of the oxysulphuret of antimony: the theory of its formation being the same as that of the sulphuret of silver, it is unnecessary to dwell longer on it.

Sulphuret of Tin.—Sulphuret of tin may be obtained in very small crystals, possessing a metallic brightness; but the success of the experiment depends on the electro-chemical action being very feeble. The operation is therefore difficult to be conducted.

Sulphuret of Lead or Galena.—This compound also is obtained in regular tetrahedral crystals, but the method to be pursued is different from that adopted in a former case. A tube, about one decimetre† in length and five or six millimetres‡ in diameter, is closed at one end. The lower part is filled, to the height of two or three centimetres§, with black sulphuret of mercury. On this we pour a solution of chloride of magnesium: a plate of lead is then plunged into the liquid to the very bottom of the tube. The apparatus being hermetically sealed, is then left to the electro-chemical action. In a month or six weeks we begin to perceive on the interior surface of the tube, above the sulphuret, a very thin layer of a brilliant precipitate (of a metallic gray colour), which is easily detached, and becomes gradually covered with small crystals, which appear, when seen through a microscope, to be regular octahedrons, presenting the same aspect as those of the galena. When the tube is opened, a gas escapes, which diffuses the

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* $\frac{1}{37}$ of an inch. † About $3\frac{1}{2}$ inches. ‡ About $\frac{1}{10}$ of an inch. § About 1 inch.

odour peculiar to the combinations of sulphur with chlorine and hydrogen. If the liquor is tested with an acid, there is an escape of sulphurous acid. The lower part of the plate of lead has become brittle, in consequence of the combination of the lead with mercury. In order to explain these several results, it must be recollected that when the lead is in contact with an alkaline or earthy chloride, such as that of magnesium, a double chloride is formed and magnesia is precipitated. In consequence of this reaction, the lead becomes negative and the solution positive; the circulation of the electric fluid is then carried on through the medium of the infinitely thin layer of liquid which adheres to the surface of the glass. Under the same circumstance, the sulphuret of mercury being dissolved in the chloride, is subjected to the action of the small pile: the lead, which is the negative pole, attracts the mercury, and the sulphur is attracted by the double chloride of lead and magnesium. One portion of the sulphur combines with the lead, whence there arises a sulphuret of lead, which crystallizes without any trace of mercury, while the other portion combines with the chloride of magnesium, and the chlorine which was combined with the lead; and thus there is produced a sulpho-chloride of magnesium and a chloride of sulphur.

The operation being continued for several months, the liquor in the part adjacent to the sulphuret of mercury assumes a reddish tint, which is probably owing to the presence of chloride of sulphur. It is necessary to observe, that no trace of lead is found in the liquor; a fact which proves that it is precipitated as soon as dissolved.

The action which determines the formation of the sulphuret of lead being very complex, it would be difficult to say, without having analysed the liquor, how the several decompositions and combinations first mentioned are effected in definite proportions.

Sulphuret of Iron and Zinc.—It is very difficult to form these compounds by the processes which we have been describing, because of their great liability to be affected by contact with air and with water. We have succeeded nevertheless in obtaining the first, by means of the alkaline hyposulphite, in small yellow crystals of great brilliancy, which became rapidly decomposed in contact with the air. As to the sulphuret of zinc, we have not been yet able to obtain it crystallized.

Sulphuret of Cadmium.—The sulphuret is obtained, of an orange yellow, in a crystalline form, by employing the second process; namely, that which gives the sulphuret of lead.

From the facts set forth in this chapter, we may conclude that, in order to obtain the crystallization of an insoluble substance by means of electro-chemical reactions, it will be sufficient that we bring it into combination with another substance which is soluble, and then produce a very slow decomposition. The same object may be attained, but with far greater difficulty, by the ordinary resources of chemistry, as the following observation will show:

Some clay, very much divided and moistened with a solution of arseniate of potash, having been put into a glass tube, there was poured over it a solution of nitrate of copper: the reaction of the two solutions took place rapidly in the first few instants at the surface of contact of the clay and the solution of the nitrate. But the solution having gradually penetrated the mass of the clay, the consequence was, that the reaction between the arseniate and the nitrate was very slow, and therefore favourable to the crystallization of the arseniate of copper. After the lapse of some time, there were seen in the vacant interstices of the clay some crystals resembling those of native arseniate of copper.

The formation of double sulphurets and simple sulphurets being subject to certain laws, we must not use tubes of any dimensions we please, and must not employ liquids possessing too great an electric conductibility. If, for example, the quantity of double hyposulphate formed were too great to be completely decomposed by the acid which comes from the tube containing the nitrate of copper and the nitrate of silver, the operation would be incomplete, inasmuch as we should not have, in that case, the reactions necessary for the formation of the compounds we wish to obtain. Thus, in proportion as the circumstances have been more or less favourable, the result will be a perfect or a confused crystallization, or a total absence not only of crystallization but of the production of double sulphuret.

The tubes employed must always be of small dimension, that is to say, of two or three millimetres* ; for if the acid came in too great quantities into the tube containing the double combination, it would react on each of the components, and the desired result would not be attained. It must not be forgotten that the hyposulphite which we employed proceeded from a protosulphuret of potassium decomposed in the air.

Of Metallic Iodides.

It is known that the metallic iodides are subject to the same law of composition as the sulphurets. We expected therefore to be able to obtain the insoluble iodides by the same process which has served to obtain the sulphurets. In thinking thus we are but generalizing the principle.

Iodide of Lead.—In the electro-chemical apparatus already described we substitute the iodide of potassium or soda for the alkaline hyposulphate; we then plunge a plate of copper into that branch of the bent tube which contains the nitrate of copper, and a plate of lead into the other branch, which contains the solution of iodide of potassium. In the latter branch of the tube we obtain at first a double iodide of lead and potassium, which crystallizes in very fine white silky needles; this combination is gradually decomposed, the decomposition commencing at the

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* About $\frac{1}{25}$ of an inch.

lower part, which is contiguous to the clay. We then perceive a great number of crystals, derived from the regular octahedron, and exhibiting that golden hue and brilliancy of aspect which belong to the iodide of lead.

Copper, subjected to the same sort of action, gives at first a double iodide in white crystallized needles; and then, after the decomposition, we obtain some octahedral crystals. The iodide of silver also is obtained without difficulty.

It is probable that the other metals whose iodides are insoluble would, with due precaution, afford similar results. The insoluble bromides and selenurets can unquestionably be obtained by the same process. We shall, in this place, confine ourselves to the bare mention of the fact, because their formation flows from a general principle which has been sufficiently developed in this chapter to render new details unnecessary.

ACCOUNT OF A GENERAL METHOD OF OBTAINING IN A CRYSTALLIZED STATE, SULPHUR, THE SULPHATE, AND THE CARBONATE OF BARYTES.

The Principle employed in the Formation of those Compounds.

We have already shown that when a body transferred by a current meets another body with which it forms an insoluble compound, the combination takes place, and the precipitate is produced instantaneously. But we have not yet adverted to that which happens when an acid encounters a salt, the base of which has a greater affinity towards that acid than towards the acid with which it is actually combined. The following experiment will show what it is that happens in such a case.

A glass tube several millimetres in diameter, open at each end, and containing in its lower part some very fine potter's clay moistened with a solution of nitrate of potash, and in its upper part some common alcohol, is placed in another tube filled with a solution of sulphate of copper. The communication between the two liquids is then established externally by means of an arc, consisting of two plates of copper and lead soldered end to end, the copper side being immersed in the sulphate and the leaden side in the alcohol. The sulphate of copper is quickly decomposed, in consequence of the electric effects resulting from the reaction of the two liquids on each other, and that of the alcohol on the lead. The copper of the sulphate is reduced on the plate of copper, which is the negative pole; the oxygen and the sulphuric acid are transferred to the side where the plate of lead is; but, instead of obtaining sulphate of lead, we see, after an interval of a few days, that a great number of octahedral crystals of nitrate of lead have been formed. This fact proves, beyond all doubt, that the sulphuric acid, in traversing the clay impregnated with nitrate of potash, decomposed this salt and combined itself with the potash, because of its having a greater affinity towards this base than the nitric acid has. The latter acid being liberated proceeded to the positive pole, where it combined itself with the oxide of lead, formed, in a great measure, at the expense of the oxygen of copper. The result was a nitrate of lead, which crystallized in proportion as it had saturated the alcohol. With a more powerful voltaic action, the sulphate of potash would have been decomposed. This example shows how valuable the feeble electric currents are in the production of combinations. In the case under consideration, the nitrate of potash was decomposed by the concurrence of the electric forces and the chemical affinities.

Let us now apply the foregoing principle to the decomposition of the sulpho-carbonate of potash, and the formation of some compounds.

1st Experiment.—The sulpho-carbonate of potash, whose solution when not very much concentrated is gradually decomposed in the air, is peculiarly disposed to admit of changes being produced in the state of combination of its molecules by the action of feeble forces. The following is the mode in which we operate on this substance: we take two glass jars, and pour into the one a solution of sulphate of copper, and into the other an alcoholic solution of sulpho-carbonate of potash. The communication between the two liquids is then established, on the one hand by means of a bent glass tube, filled with potter's clay moistened with a solution of nitrate of potash; and on the other by means of a metallic arc formed of two plates, one copper and the other lead, the plate of copper being immersed in the sulphate, and the plate of lead in the sulpho-carbonate. According to the nature of the electric effects produced in the various chemical reactions, the lead is found to be the positive pole of a pile whose intensity is sufficient to decompose the sulphate; the copper is reduced; the oxygen and the sulphuric acid are carried towards the lead; the acid, in its passage, decomposes the nitrate of potash, as in the preceding experiment, so that the oxygen and the nitric acid enter alone into the sulpho-carbonate. As soon as they have penetrated it, they begin to react on its constituent parts, and this reaction continues so long as the force of the current is superior to the affinities of the several bodies present: the transfer of the molecules is continued as far as the positive plate, where the last reaction takes place. The following products are successively formed: neutral carbonate of potash which crystallizes on the sides of the vessel, sulphate of potash, a sulpho-carbonate of lead and of potash in acicular crystals, carbonate and sulphate of lead in needles; and, in fine, a portion of sulphur proceeding from the decomposition of the sulphuret of carbon and the sulphuret of potash, is deposited on the plate of lead, which is the positive pole, and there crystallized in octahedrons with a rhomboidal base. These octahedrons sometimes attain a millimetre in length when the experiment has been continued for a month.

Crystallized sulphur is also obtained by abandoning to the influence of the air a solution of that substance in carburet of sulphur, or by melting some sulphur and letting the liquid stand to cool until there is formed at the surface a solid crust, which is broken in order to draw off the liquid. But the process which we have described is different from the two preceding processes, and bears some analogy to that which nature employs in some circumstances; for instance, in the slow decomposition of sulphuretted hydrogen gas and feculent matters which, in the course of time, deposit well-defined crystals of sulphur. In both cases the crystallization is the result of an excessively feeble action, on which we shall have occasion to remark when we come to treat of spontaneous actions.

The products resulting from the decomposition of the sulpho-carbonate of potash vary according to the intensity of the electric current and the degree of concentration of the solution. With an alcoholic solution of sulpho-carbonate, diluted with water, we obtain but little sulphur, and a great quantity of sulpho-carbonate of lead and potash. These differences in the results are owing to the ratios which the affinities of the several bodies bear to the intensities of the current, which vary according to the conductibility of the liquids and the energy of the chemical action which produces the current. In the present state of the science, it is impossible to see *à priori* what should happen in any proposed case; that is to be learnt from experiment only.

The sulpho-carbonates of the other bases submitted to the same species of experiment gave analogous results. It is owing to the slowness of their decomposition, and to the appropriate choice of metals with respect to the positive pole, that we obtained the sulphate of barytes in crystals derived from the primitive form.

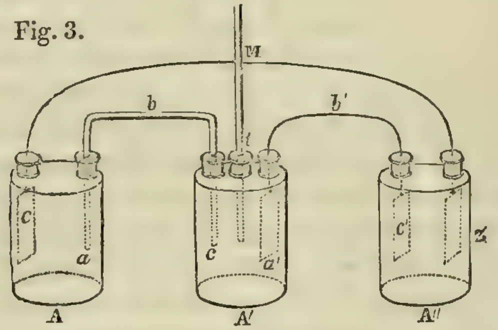
Another Application.—We fill with a solution of bicarbonate of soda a tube containing, in its lower part, clay moistened with the same solution, and place it in another which contains a solution of sulphate of copper. We then plunge into each liquid one of the extremities of a plate of copper, and observe the following effects: the end which is in the solution of sulphate being the negative pole decomposes this salt, and attracts the copper, while the oxygen and the sulphuric acid pass to the other side. But as the sulphuric acid on its passage meets with carbonic acid, it expels this from the combination and takes its place. The carbonic acid then forms with the oxide of copper a carbonate which, by combining with that of the soda, produces a double carbonate of copper and potash, which crystallizes in beautiful satin-like needles of a bluish green colour. This substance, which does not dissolve in water, is decomposed with the aid of heat; the carbonate of soda is dissolved, that of copper is precipitated and become brown, like the common carbonate when subjected to the action of boiling water.

In the preceding experiments, the power of sulphuric acid in expelling acids having a less affinity than itself towards the bases is owing solely to the small energy of action in the pile; for if the action were more powerful, all the acids would be indiscriminately transferred to the positive pole.

The electric current employed by us in order to produce decompositions, may arise from two causes; the chemical reaction which the two liquids in contact exercise on each other, and the chemical action of the liquid of the small tube on the metal immersed in it. In the first case, if the reaction be sufficiently energetic, the second cause may be dispensed with: in like manner, if the second cause be of sufficient intensity, the first becomes superfluous. But when both are feeble and the currents resulting from them have the same direction, their sum is then indispensable to the production of the electro-chemical effects. In general, whenever the two currents take the same course, their sum cannot but be favourable to the decompositions and the formation of the products. It often happens that these two currents are so weak that the reduction of the oxide in the great tube cannot take place. In that case there is no effect produced. If, therefore, after an interval of some days, we perceive no precipitation of copper on the plate of copper immersed in the solution of the nitrate or the sulphate, it is useless to continue the experiment longer, and the apparatus must be changed. In the experiment in which the great tube contains sulphate of copper, and the small one contains, in its lower part, potter's clay moistened with a solution of nitrate of potash, and alcohol, the chemical reaction of the nitrate on the sulphate has had considerable influence in producing the current which has decomposed the sulphate of copper; for the action of the alcohol on the lead must have been sufficiently feeble to give rise to a sensible electric current. It would be desirable to operate always on such solutions as would exercise on each other chemical actions sufficiently energetic to give out the requisite currents, when the plate immersed in the liquid is gold or platina, in order that we may be able to study the phænomena of decomposition and recomposition with facility, and independently of the reaction of the oxides. This would be the only course to be taken in order to discover what it is that takes place in the liquid organic compounds, when, by means of electricity, we introduce into them such bodies as are capable of carrying off some of their constituent parts. This want of sufficient reaction in the liquids may be supplied by operating with the following apparatus, which enables us, when we wish, to avoid the action of the metallic oxides formed at the positive pole. As this apparatus enables us to operate in a great variety of cases, we shall more minutely describe its construction and use.

We take three jars A, A′, A″, (fig. 3.) placed in a line at a short distance from each other. The first is filled with a solution of sulphate or nitrate of copper; the second, with a solution of that substance in the constituent parts of which we wish to produce changes; and the third, with water rendered a slight conductor of electricity by the addition of an acid or common salt capable of acting chemically on the metal which is to be immersed in it. A communicates with A' by means of a bent tube, *abc*, filled with potter's clay moistened with a saline solution, the nature of which depends on the effect intended to be produced in A; A' and A'' communicate with each other through the medium of a plate $(a'b'c')$ of platina or gold; and A and A'' communicate by means of a voltaic pair (CMZ), composed of two plates (MC and MZ) of copper and zinc; in fine, a safety-tube (*t t*) is placed in the vessel A', in order to indicate the internal pressures resulting from the disengagements of gas. According to this arrangement, the extremity a' of the plate of platina is the positive pole of a voltaic apparatus whose action is slow and continuous. When the liquid contained in A' is a good conductor, the intensity of the electric forces is sufficient to decompose the sulphate of copper in A. From that instant the oxygen proceeds towards a', as well as the sulphuric acid, which, in passing into the tube (*abc*) expels those acids which have a less affinity than it has itself towards the bases. These acids and the oxygen pass into the liquid A', where their slow reactions determine the relative changes in the bodies which they find there. This apparatus allows us to operate on a greater scale, and to avoid the reaction of the oxide in the same manner as it was avoided in the apparatus previously described.

Fig. 3.



We are often compelled to place a fourth jar between A and A', into which there is put so much of a saline solution, to be decomposed by the sulphuric acid, that the effects produced in the liquid A' may not be interrupted when all the liquid of the clay has been decomposed. Thus, when we wish to bring a gas or an acid in its nascent state into the liquid of the vessel A', we have only to introduce into the clay a solution which, by its reaction on the sulphuric acid proceeding from the decomposition of the sulphate of copper, may let the gas or the acid escape. If, on the contrary, it should be found necessary to conduct into the same liquid either hydrogen or an electro-positive gas, the media of communication must be reversed, $a' b' c'$ being put in the place of $a\ b\ c$, and *vice versâ*. In fine, if for the plate of platina there is substituted a plate of oxidable metal, we introduce into the intermediate solution the reaction of an oxide, which being in its nascent state conduces to the formation of the products. The bare inspection of the figure is sufficient to give an idea of the results that may be obtained by making the requisite variations in the solutions.

Mode of using the Apparatus when the Positive Plate is not oxidable.

1st Experiment. We pour into the vessel A′ an alcoholic solution of sulpho-carbonate of potash; into the vessel A a solution of sulphate of copper; and into the clay of the tube $a\ b\ c$ a solution of nitrate of potash: after 24 hours' trial the reaction of the oxygen and the nitric acid on the solution of the sulpho-carbonate is now perceptible; for we observe on the extremity (a') of the platina plate the products already indicated (when the operation was supposed to be performed with a plate of lead), that is to say, crystals of sulphur, of neutral carbonate of potash, &c., but no carbonate of lead, because there is no oxide of this metal.

2nd Experiment. *Crystals of Sulphur; Sulphate and Carbonate of Barytes.*—In the preceding apparatus we substitute for the sulpho-carbonate of potash a solution of sulpho-carbonate of barytes: we shall not have long to wait for the appearance of analogous reactions; a precipitation of sulphur in small crystals, and a formation of sulphate and carbonate of barytes in prismatic needles. We should perhaps by this process obtain crystals of some size, if the plate of platina was so bent as to form a spoon, and thus prevent the crystals formed on the surface of the plate from falling to the bottom of the vessel.

3rd Experiment. *Mode of ascertaining the presence of Nitric Acid and that of Hydrochloric Acid in any Solution, even when those two acids exist there in very small quantities.*—For the plate of platina $a' b' c'$ a plate of gold is substituted: there is then poured into the vessel A a solution of sulphate of copper; and into the vessel A′, and the clay of the tube $a\ b\ c$, a solution of the compound which is supposed to contain the two acids in combination with bases. As soon as the apparatus begins to operate, the sulphuric acid expels the two acids from their combinations, which, together with the oxygen arising from the reduction of the oxide of copper, are carried to the extremity (a') of the plate of gold: the yellow colour which immediately makes its appearance indicates the presence of nitric and hydrochloric acids. This reaction is also obtained when tubes of small dimensions are substituted for the jars. In this process no part of the acids is lost; for they are all transferred to the positive pole, and contribute to the production of hydrochlorate of gold.

4th Experiment. *Sulphite of Copper.*—The vessel A′ is filled with a solution of sulphite of potash, and for the plate of gold $a' b' c'$ a plate of copper is substituted. The extremity a', which is still the positive pole, attracts the oxygen and the nitric acid: the latter decomposes the sulphite and takes possession of the base; the sulphurous acid is carried to the oxide of copper, which is formed at the same time, and combines with it: the sulphite of copper itself combines with the sulphite of potash, whence there results a compound that crystallizes in beautiful octahedrons; but as the nitric acid still continues to arrive, it decomposes this double sulphite at last: sulphurous acid gas then escapes, and the sulphite of potash is transformed into bisulphite and nitrate of potash. As to the sulphite of copper, it is precipitated in transparent octahedral crystals, of a vivid red, with the brilliancy of garnets. M. Chevreul, a long time since, obtained this sulphite of copper by the ordinary processes of chemistry. We might extend the number of the results yet further, but the object we have had in view, which was to communicate the description of an apparatus of very general applicability, seems to us to be sufficiently attained.

Notes communicated by Golding Bird, Esq.

Potter's-clay, p. 417, last line but two.

M. Becquerel invariably uses clay as a medium for forming a connection between two fluids when their mutual reaction is necessary, but he gives a very requisite caution that the clay must be carefully *tempered* with water or a saline solution; for if too dry, it will not allow the transfer of electric currents with sufficient facility; and if too moist, it admits of the admixture of the two fluids, which (as when the tube curved like the letter U is used) it is intended to separate. Thence it has not unfrequently happened, that many of the experiments described by M. Becquerel have failed in the hands of some who have repeated them, merely from inattention to the clay being in a sufficiently moist or dry state. All these difficulties, which although apparently trifling are nevertheless occasionally fatal to the success of experiments, may be obviated by substituting *plaster of Paris* for the potter's-clay; for by merely mixing it with water or a saline solution, it may be readily poured into a tube of any required form, where it rapidly solidifies, and while in this state is sufficiently porous to admit of a very slow admixture of the two fluids employed, as well as to allow of the ready transfer of the feeblest voltaic current.

P. 422. *Crystallization of Protoxide of Copper.*

The crystals obtained by the process here described by M. Becquerel are exceedingly distinct, and very closely resemble some of the native forms of the oxide or ruby copper ore. This protoxide is generally found native, mixed with crystals of metallic copper; and by availing ourselves of a very weak electric current, kept up for some weeks, an exceedingly close imitation of this native combination may be obtained. For this purpose take a glass tube open at both ends, about half an inch in diameter and three inches in length: close one end of this tube by means of a plug of plaster of Paris, about one-third of an inch in thickness; fill this tube with a solution of the nitrate or chloride of copper moderately diluted, and place it inside a cylindrical glass vessel nearly filled with a weak solution of potassa or soda. These two fluids, very slowly mixing through the plug of plaster of Paris, would, if left to themselves, cause a gradual formation and deposition of hydrated deutoxide of copper. Make a compound arc of two pieces of metal, one of copper, the other of lead, taking care that their surfaces are quite clean, or even polished; plunge the leaden leg of the arc into the outer cylinder of the little apparatus, and the copper leg into the smaller one, containing the solution of nitrate or chloride of copper, and leave the apparatus to itself. Slow electric action ensues; the copper limb of the arc becoming the negative, and the lead (which slowly dissolves in the alkaline solution) the positive electrode; the electric currents thus set in motion readily traverse the plaster of Paris partition, and cause the reduction of the deutoxide of copper (precipitated by the slow admixture of the alkaline solution with the copper salt) partly to the metallic state and partly to the state of protoxide. That portion of the peroxide which is thus reduced to the metallic state yields very fine and tolerably large crystals, which are of course deposited on the surface of the copper negative electrode. *Mixed* with these crystals, the protoxide is deposited in very delicate, transparent, ruby-red crystals, not isolated and separate, but deposited in rosette-like patches, which present under a lens an exceedingly beautiful appearance. So closely do some of the specimens of protoxide and metallic copper thus obtained resemble the native forms, that it would be difficult, if not impossible, even for an experienced eye to distinguish the native from the factitious specimens, if the nature of the substance they are precipitated on did not betray their origin.

P. 425. *Crystallization of Oxide of Zinc.*

This oxide may be obtained much more conveniently by the use of the apparatus contrived for the crystallization of the ruby oxide of copper (vide last note) with the substitution of a solution of oxide of zinc in caustic potassa for the uncombined alkali, in the larger vessel. By this arrangement, the copper leg of the arc becomes, as before, the negative, and the leaden the positive plate of a miniature battery, sufficient however to cause a very elegant deposition of oxide of zinc on the plate of lead or positive electrode in the course of a week or ten days; whilst upon the copper plate or negative electrode a mixed deposit of metallic copper and ruby-coloured protoxide takes place, rivaling in beauty that obtained by the last-described process. By this modification of the apparatus, the two oxides (zinc and copper) may be obtained crystallized by one and the same operation.

ARTICLE XXII.

On a New Combination of the Anhydrous Sulphuric and Sulphurous Acids; by HENRY ROSE, *Professor of Chemistry at the Royal University of Berlin**.

From Poggendorff's *Annalen der Physik und Chemie*, vol. xxxix. No. 9. 1836. p. 173.

BY passing dry sulphurous acid gas into anhydrous sulphuric acid, I obtained a limpid fluid, which smelt very strong of sulphurous acid, and which by exposure to the air evaporated entirely in thick fumes.

This fluid is a combination of the anhydrous, sulphurous, and sulphuric acids, in definite proportions. In order to obtain it several precautions are requisite: the slightest trace of moisture must in particular be avoided; for should any be present, the compound even when formed is very easily decomposed; and should either of its components contain any trace of moisture, its composition would be entirely prevented.

In order to avoid this, I passed the sulphurous acid gas first through a cooled receiver, and then through a tube at least four feet long, filled with freshly heated chloride of calcium. From hence it passed very slowly into a glass vessel, which contained the anhydrous sulphuric acid, and which was closed by a cork, through which the tube conveying the sulphurous acid was passed. This glass vessel was cooled to about the freezing point of water, but not lower, lest the new compound might contain free condensed sulphurous acid. As soon as a certain quantity of the fluid was formed, it was poured off from the remaining solid sulphuric acid into a small glass, and immediately submitted to examination.

The tube containing the chloride of calcium could only be used for one preparation; it was obliged to be heated once more previously to being again employed. When a certain quantity of the compound (a few grammes for instance) is formed, the formation of a further portion ceases entirely, because the chloride of calcium no longer dries the sulphurous acid so perfectly as at the beginning of the operation. On exposure to the air the fluid thus obtained fumes very much, and smells strongly of sulphurous acid. I have always obtained this fluid of a brownish colour; but this is not essential to the compound, which is colourless, but arises from the cork which closes the vessel containing the sulphuric acid. The fluid is so volatile, that when brought into con-

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* [The Editor is indebted for the translation of this Paper to E. Solly, jun. Esq.

tact with the air it very soon evaporates, and then occasionally leaves behind an exceedingly small portion of hydrous sulphuric acid. This great volatility, as also the easy decomposability, of the compound, entirely prevented my bringing it into a small glass bulb with a long neck drawn to a point, as is done with other less volatile and decomposable fluids, by warming the ball and then dipping the point in the fluid. When the bulb is quite cooled, the compound does not rise in the stem, less on account of its own vapour preventing it, than because it is decomposed in the rarified space in the bulb, and sulphurous acid gas is evolved. This is also the reason why it is not possible to ascertain the specific gravity of the vapour of this compound. If ever so small a portion of water be brought in contact with this fluid, a strong effervescence and evolution of sulphurous acid gas immediately ensues. The compound is entirely decomposed by a small quantity of water. If it be brought into a glass vessel, so nearly dry that not the slightest moisture is perceptible on its sides, even then a slight effervescence and decomposition occur; this is the reason of the necessity for the great care to avoid the slightest trace of moisture in the formation of this compound, which would otherwise be entirely prevented. If a large quantity of water be added to it, it boils fiercely, through the sudden evolution of sulphurous acid.

If dry ammoniacal gas be passed into the fluid, anhydrous sulphate and sulphite of ammonia are formed. The product thus obtained is of a yellowish colour, and soluble in water; if the solution be saturated with muriatic acid, sulphurous acid is evolved, but no precipitate of sulphur falls until the fluid is boiled. A solution of nitrate of silver causes a precipitate in it, which is at first white, then yellow, brown, and at last (very quickly if boiled) black. These are the properties of a combination of dry sulphurous acid and ammonia, which I have before described*. A solution of chloride of strontium causes a precipitate of sulphate of strontia, occasioned by the sulphuric acid, which is formed by the action of the chloride of strontium upon the solution of the anhydrous sulphite of ammonia; if this precipitate be removed, and the supernatant liquid boiled, a fresh precipitate of sulphate of strontia falls, which is one of the properties of the solution of the anhydrous sulphite of ammonia†.

In analysing this substance, I have only succeeded in estimating exactly the quantity of sulphuric acid, not of sulphurous, though I attempted it in several ways. A weighed portion of the compound, in a very small bottle, which had been weighed previously with the glass stopper, was oxidized by fuming nitric acid, in such a way that no loss could be sustained by the violent action. The nitric acid was in a large

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* Poggendorff's Annalen, vol. xxxiii. p. 235.

† Ibid. vol. xxxii. p. 81.

bottle, which by a ground stopper could be rendered air-tight. The small bottle containing the weighed portion, and without the stopper, was fastened by a platina wire, and thus quickly introduced into the large bottle, which was then immediately closed, but in such a manner that the fluids themselves could not act upon each other, but only their vapours. After some time I agitated this carefully, but in such a manner that only a little of the compound in the small bottle was thrown out and could mix with the nitric acid, which always caused a very strong action, though never the evolution of light. If, on the contrary, by shaking the bottles, a little nitric acid fell into the small bottle, a crystalline deposit was formed, which I have not examined more closely, but which perhaps may be the same as that which is often formed during the preparation of the English oil of vitriol, and which consists of sulphuric and nitrous acids and water. After the mixture of the nitric acid and the substance was completed, the whole was diluted with water, and then saturated with a solution of chloride of barium.

From the quantity of sulphuric acid which was contained in the sulphate of baryta thus obtained, I could easily appreciate the relative proportions of the sulphuric and sulphurous acids in the compound; for what the first contained more than the latter in weight, could only consist in oxygen which the compound had absorbed.

But in two experiments, both conducted with equal care, I obtained from the sulphate of baryta less sulphuric acid than I had taken in weight of the compound; a proof that evidently only a part of the sulphurous acid had been oxidized by the nitric acid.

In the first experiment 2·237 grammes of the compound gave 5·633 grammes of sulphate of baryta, which contained 1·936 of sulphuric acid, equal to 82·08 per cent. of the compound. In the second experiment I obtained, from 1·250 grammes of the compound procured by another preparation, 3·443 grammes of sulphate of baryta, which contained 1·1834 grammes of sulphuric acid, indicating 94·67 per cent. of the compound.

This very slight difference shows plainly that it only arises from the mode of preparation, and that in the combination fuming nitric acid did not convert the free sulphurous into sulphuric acid. Perhaps it might have been effected had more dilute nitric acid been employed, because in the preparation of the English oil of vitriol the sulphurous can convert itself into sulphuric acid; but for a quantitative analysis it did not seem to me so fitting. Moreover the compound, oxidized by nitric acid and then diluted with water, did not smell of sulphurous acid. That the nitric acid did not fully oxidize the sulphurous acid in the compound is evident from the result of a third experiment, in which I took some of the compound which had been oxidized by fuming nitric acid, and having mixed it with a weighed quantity of freshly heated oxide of lead, I evaporated the whole to dryness, and heated the mass to redness. I obtained from 1·613 grammes of the compound, treated with fuming nitric acid and mixed with 10·739 grammes oxide of lead, a mass which after being heated weighed 12·238 grammes, and which contained therefore 1·499 grammes of sulphuric acid, or 92·93 per cent. This experiment proves that the loss cannot be due to any formation of hyposulphuric acid.

The result of experiments in which I endeavoured to oxidize the sulphurous acid by solutions of gold were much more uncertain. For this purpose I employed a carefully prepared solution of the triple salt, the chloride of gold, and sodium. The compound was brought into contact with this in the same manner as in the former experiments with nitric acid. The mixture was submitted, out of contact with the air, to a moderate heat for about twenty-four hours. Two experiments, conducted with equal care, gave however such widely differing results, that it was impossible for me to explain the cause of the great dissimilarity: for from 1·259 grammes of the compound I obtained in one experiment only 0·058 of a gramme of metallic gold; whilst in a second experiment I obtained, from a much smaller quantity of the compound, out of 0·667 of a gramme, more gold, namely 0·196 of a gramme.

The estimating of the sulphuric acid in the compound gave much more satisfactory results; I was obliged to be contented with these, and to estimate the quantity of sulphurous acid by the loss sustained. The determination was effected in the following manner: a quantity of the compound, weighed in a small glass bottle with a stopper, was put into a larger bottle, which could also be closed by a glass stopper: this contained a solution of chloride of barium, to which free muriatic acid was added. As soon as the little bottle was introduced, the larger one was closed; and by shaking the vessel, the stopper of the little bottle, which was merely loosely fixed, was thrown off, so that the compound might mix with the solution of chloride of barium;—an exceedingly violent but not dangerous action ensued.

After the sulphate of baryta was deposited, the liquid was quickly filtered out of contact with the air, and its weight determined. An addition of muriatic acid to the solution of chloride of barium is quite necessary, because otherwise the sulphate of baryta cannot easily be filtered, but passes milky through the filter.

I have analysed four different portions, prepared at different times, in this manner, and have obtained results which, though they agree much less than those of less easily decomposable substances, yet on account of the great decomposability and volatility of the compound, and therefore the difficulty of freeing it from uncombined sulphuric or sulphurous acid, agree more nearly than I expected.

The substance contains more sulphuric acid when it is not analysed immediately after preparation, and after it has given forth some sulphurous acid; less sulphuric acid, on the contrary, when this was the case, and it therefore contained some free sulphurous acid.

The result of these four experiments carefully conducted, according as the substance was of newer or older preparation, was as follows:

	Weight of Compound.	Weight of sulphate of baryta obtained.	Percentage of sulphuric acid.
I.	0·529 gr.	1·122 gr.	72·90
II.	0·955	1·945	70·00
III.	1·274	2·554	68·91
IV.	2·550	5·021	67·68.

The compound does not contain, therefore, as I suspected before the experiment, sulphuric and sulphurous acids in the same proportions as in the anhydrous hyposulphuric acid ($\dddot{S} + \ddot{S}$), but 2 eq. of sulphuric acid to 1 eq. of sulphurous acid ($2\dddot{S} + \ddot{S}$) which in 100 parts may be stated as follows:

Sulphuric acid	71·42
Sulphurous acid	28·58
	100·00

As the sulphurous is the least strong acid in the combination, and may therefore be considered of the two as the base, according to this view the compound may be considered as a neutral sulphate salt, in which the sulphuric acid contains three times as much oxygen as the base.

ARTICLE XXIII.

On the Forces which regulate the Internal Constitution of Bodies. By O. F. MOSSOTTI.

From a Memoir addressed to M. Plana, published separately, and communicated by M. FARADAY, Esq., D.C.L., F.R.S., &c.

PRELIMINARY REMARKS.

1. THE study of the phænomena of nature has led philosophers to consider bodies as being composed of molecules held in a state of fixed equilibrium at a certain distance from each other. Such a state requires that they should be endued with a certain action. Some peculiarities of this action we are already able to assign, but its complete characteristics are not yet well defined.

As the resistance opposed by bodies to compression increases indefinitely with the reduction of their volume, though their molecules have not come into contact with each other, it shows that the force which they exercise is repulsive at the least distances. At a distance greater than these, but still imperceptible, it must vary with great rapidity, and become attractive, in order that a steady equilibrium of the molecules may be possible; and finally, when it has become perceptible, it must decrease in the inverse ratio of the square of the distance, in order to represent the universal attraction. The limits of the distance at which the negative action becomes positive vary according to the temperature and nature of the molecules, and determine whether the body which they form be solid, liquid, or aëriform.

There is a class of phænomena, rather singular at first sight, in which however it appears that nature designed, by separating the forces which she employs, to present herself in all her simplicity. Such are the phænomena which constitute what we denominate *statical electricity*. It is well known with what admirable facility Franklin explained these phænomena, by supposing that the molecules of bodies are surrounded by a quantity of fluid or æther, the atoms of which, while they repel each other, are attracted by the molecules. It is known also how Coulomb subsequently proved that the force with which the repulsion of atoms and the attraction of the molecules are produced, is, like universal attraction, regulated by the law of the inverse ratio of the square of the distance. Indeed, the latter philosopher has substituted for the hypothesis of Franklin, which is that generally followed in England, Germany, and Italy, another hypothesis, in which a second fluid is supposed to perform the part assigned to matter in that of Franklin; and this mode of explaining the phænomena has been more generally adopted in France. It is even asserted that the latter hypothesis is the only one that should be received, inasmuch as it has been completely confirmed by the results of the beautiful analysis with which M. Poisson has begun to enrich the Memoirs of the Academy of Sciences. But they who put forward this assertion have not paid due attention to the fact that, although this illustrious geometer has, for the purpose of establishing his calculations, adopted the language of his school, the inferences drawn from them are not more applicable to the one hypothesis than to the other. He sets out in fact with the principle, that, " If several bodies, being electric conductors, are placed in presence of each other, and attain a permanent state, the result of the actions of the electric layers which cover them, on a point taken anywhere in the interior of a body must, in that state, be null; otherwise the combined electricity which exists in the point under consideration would be decomposed; but this is contrary to the supposed state of permanence." Now if for this principle the following be substituted: " If several bodies, being electric conductors, are placed in presence of each other, and thus attain a permanent state, the result of the actions of the layers of electric fluid which cover them, and of the exterior layers of matter which are not yet neutralized, on the electric fluid at a point taken anywhere in the interior of a body, must, in that state, be null; otherwise the electric fluid which exists in that point would be displaced, which is contrary to the supposed state of permanence;"—and if we interpret accordingly the literal denominations employed by M. Poisson in his equations,—all his results will be equally true on Franklin's hypothesis. In general, the action of the condensed electric fluid will stand for that of the vitreous fluid; and the action exhibited by the matter, in proportion as it is deprived of a quantity of its electric fluid, will stand for that of the resinous fluid. There is one circumstance, however, which makes a difference between the hypothesis of Dufay or Coulomb and that of Franklin: it is this, that, according to the one, the two fluids are moveable in the bodies, while according to the other the electric fluid is, but the matter is not, moveable. As the equilibrium, however, requires that we should only regard the relative position, the mobility of the electric fluid alone is sufficient for its establishment.

Æpinus, who has reduced Franklin's hypothesis to the form of a mathematical theory, was the first to remark, that if it be the requisite condition for the equilibrium of the electric fluids of two bodies, in their natural state, that " the attraction of the matter and the repulsive action of the fluid of the first body on the fluid of the second should be equal, and *vice versa*," there are but three forces in operation; two of which are attractive, and but one repulsive. In other words, each of the two bodies attracts the fluid of the other, while the mutual repulsion of the two fluids constitutes only a single force, equal to each of the two attractive forces. If then, with the equilibrium of the fluids, it is desired to find the equilibrium of the masses also, an equal repulsion must be allowed between the molecules; since the bodies would otherwise forcibly attract each other. But such an attraction is contrary to what we learn from experience. He felt at first a strong objection to the admission of such a repulsive force between the material molecules, as being opposed to the idea entertained of their mutual attraction, which was so clearly demonstrated on Newton's principles. But a little reflection satisfied him that this admission contained nothing that was opposed to facts, or, as he might rather have said, that was not confirmed by facts. Universal attraction itself may follow as a consequence from the principles which regulate the electric forces: for if we suppose that, the masses being equal, the repulsion of the molecules of matter is a little less than their attraction of the atoms of the æther, or than the mutual repulsion of the atoms themselves, this will be sufficient to leave an excess of attraction which, being directly as the product of the masses and inversely as the square of the distance, would exactly represent the universal attraction.

2. While reflecting on these principles, in a course of lectures on natural philosophy which I gave at the University of Buenos Ayres, I conceived the idea, that if the molecules of matter, surrounded by their atmospheres, attract each other when at a greater, and repel each other when at a less distance, there must be between those two distances an intermediate point at which a molecule would be neither attracted nor repelled, but would remain in steady equilibrium; and that it was very possible this might be the distance at which it would be placed in the the composition of bodies. I thought the idea of sufficient importance to fix it in my memory, but did not at the time pursue its development further.

On my return to Europe I learned, through the reading of some memoirs, and in the course of conversation with men of science, that the attention of geometers was particularly directed to the molecular forces, as being those which may lead us more directly to the knowledge of the intrinsic properties of bodies. I was thus led to recall my ideas on the subject, and set about subjecting them to analysis. The results of my first investigations I here submit to the judgement of philosophers.

I have supposed that a number of material molecules are plunged into a boundless æther, and that these molecules and the atoms of the æther are subject to the actions of the forces required by the theory of Æpinus, and then endeavoured to ascertain the conditions of equilibrium of the æther and the molecules. Considering the æther as a continuous mass, and the molecules as isolated bodies, I found that, if the latter be spherical, they are surrounded by an atmosphere the density of which decreases according to a function of the distance which contains an exponential factor. The differential equation which determines the density being linear, is satisfied by any sum of these functions answering to any number of molecules. Whence it follows that their atmospheres may overlay or penetrate each other without disturbing the equilibrium of the æther. Proceeding in the next place to the conditions of equilibrium of the molecules, I observed that, for a first approximation (which may be sufficient in almost all cases), the reciprocal action of two molecules and of their surrounding atmospheres is independent of the presence of the others, and possesses all the characteristics of molecular action. At first it is repulsive, and contains an exponential factor which is capable of making it decrease very rapidly: it vanishes soon after, and at this distance two molecules would be as much indisposed to approach more nearly as they would be to recede further from each other; so that they would remain in a state of steady equilibrium. At a greater distance the molecules would attract each other, and their attraction would increase with their distance up to a certain point, at which it would attain a maximum: beyond this point it would diminish, and at a sensible distance would decrease directly as the product of their mass, and inversely as the square of their distance.

This action, possessing all the properties with which we can presume that molecular action is endued, is the more remarkable as it has been deduced from those forces only whose existence was already admitted by philosophers, and whose law is characterized by such extraordinary simplicity. When tested in the explanation of the varied phænomena which are proper to it, it must lead, in case of failure, to the exclusion of those forces from amongst physical principles; or, in case of success, establish their reality; and thus mark in a striking manner the admirable œconomy of nature.

To apply the formulæ which we have found, for the purpose of representing molecular action, to the phænomena of the interior constitution of bodies, requires methods of calculation which are not yet developed, and which must become still more complicated when the arrangement of the molecules, their form and their density, are taken into consideration. I have thought it advisable, however, in consideration of the use to which it might be applied by able geometers, not to postpone the publication of this mode of viewing molecular action. It is a subject which appears to me entitled to the greatest attention, because the discovery of the laws of molecular action must lead mathematicians to establish *molecular mechanism* on a single principle, just as the discovery of the law of universal attraction led them to erect on a single basis the most splendid monument of human intellect, *the mechanism of the heavens.*

Analysis.

3. If several material molecules, which mutually repel each other, are plunged into an elastic fluid, the atoms of which also mutually repel each other, but are at the same time attracted by the material molecules, and if these attractive and repulsive forces are all directly as the masses, and inversely as the square of the distance, it is proposed to determine whether the actions resulting from these forces are sufficient to bring the molecules into equilibrium, and keep them fixed in that state. The object of this inquiry, as may be perceived, is to complete the deductions from the hypothesis of Franklin and Æpinus. It is already known that the conditions of equilibrium which it furnishes, in reference to questions of statical electricity, are in accordance with the phænomena: it remains to be ascertained, whether the molecular actions which result from it are also in accordance with those which determine the interior constitution of bodies. An agreement of this kind would add greatly to the probability that the hypothesis in question is well founded, and afford us a glimpse of the means by which we should be enabled to consider all physical phænomena in one and the same point of view.

Let f be the accelerative force of repulsion existing among the atoms of the æther at a distance taken as unity; q the density at a point $x\,y\,z$, and ε the measure of the elastic force or pressure at the same point, referred to the superficial unit. Let g be the accelerative force of attraction between the atoms of the æther and the matter of the molecules at a distance equal to unity, and ϖ the density at the point $\xi\,\eta\,\zeta$ of a molecule which we suppose to be possessed of a certain though very small extension.

By putting

$$F = \iiint \frac{f\,q'\,dx'\,dy'\,dz'}{\{(x'-x)^2+(y'-y)^2+(z'-z)^2\}^{\frac{1}{2}}},$$

$$G = \iiint \frac{g\,\varpi\,d\xi\,d\eta\,d\zeta}{\{(\xi-x)^2+(\eta-y)^2+(\zeta-z)^2\}^{\frac{1}{2}}},$$

the triple integral F being extended to the whole space from x', y', z', equal to $-\infty$, as far as x', y', z', equal to $=\infty$ (the small parts occupied by the molecules being excepted), and the triple integral G being extended to all the values of ξ, η, ζ, that answer to the points occupied by the molecule, we shall have for the equilibrium of the æther the equations

$$\frac{d\varepsilon}{dx} = -q\frac{dF}{dx} + q\frac{dG}{dx} + q\frac{dG_1}{dx} + q\frac{dG_2}{dx} \cdots\cdots + q\frac{dG_\nu}{dx} + \text{etc.}$$

$$(\mathrm{I})\quad \frac{d\varepsilon}{dy}=-q\frac{dF}{dy}+q\frac{dG}{dy}+q\frac{dG_1}{dy}+q\frac{dG_2}{dy}\cdots\cdots+q\frac{dG_\nu}{dy}+\text{etc.}$$

$$\frac{d\varepsilon}{dz}=-q\frac{dF}{dz}+q\frac{dG}{dz}+q\frac{dG_1}{dz}+q\frac{dG_2}{dz}\cdots\cdots+q\frac{dG_\nu}{dz}+\text{etc.};$$

in which G_1, G_2, ... G_ν, &c. denote the quantities analogous to G which correspond with the different molecules 1, 2 ... ν, &c.

Let us likewise put

$$\Phi=\iiint\frac{gq'\,dx'\,dy'\,dz'}{\{(x'-\xi)^2+(y'-\eta)^2+(z'-\zeta)^2\}^{\frac{1}{2}}}$$

$$\Gamma_\nu=\iiint\frac{\gamma\varpi_\nu d\xi_\nu d\eta_\nu d\zeta_\nu}{\{(\xi_\nu-\xi)^2+(\eta_\nu-\eta)^2+(\zeta_\nu-\zeta)^2\}^{\frac{1}{2}}}$$

where γ denotes the force of repulsion existing among the molecules of matter at the distance assumed as unity.

The equations for the equilibrium of a molecule, if we take into consideration the motion of its centre of gravity only, will be

$$\iint\varepsilon\,d\eta\,d\zeta=\iiint\varpi\frac{d\Phi}{d\xi}\,d\xi\,d\eta\,d\zeta-\Sigma\iiint\varpi\frac{d\Gamma_\nu}{d\xi}\,d\xi\,d\eta\,d\zeta$$

$$(\mathrm{II})\iint\varepsilon\,d\xi\,d\zeta=\iiint\varpi\frac{d\Phi}{d\eta}\,d\xi\,d\eta\,d\zeta-\Sigma\iiint\varpi\frac{d\Gamma_\nu}{d\eta}\,d\xi\,d\eta\,d\zeta$$

$$\iint\varepsilon\,d\xi\,d\eta=\iiint\varpi\frac{d\Phi}{d\zeta}\,d\xi\,d\eta\,d\zeta-\Sigma\iiint\varpi\frac{d\Gamma_\nu}{d\zeta}\,d\xi\,d\eta\,d\zeta.$$

The sum Σ is to be extended to all the numbers ν, that is to say, to all the molecules except that one the equilibrium of which we are considering; the double integral is to be extended to the whole surface of this molecule, and the triple integrals to its whole volume.

4. Let us begin by considering the equilibrium of the æther. The elasticity possessed by the æther at any point of space can be only the result of the reciprocal action of the contiguous parts: hence we are led, by considerations analogous to those employed by Laplace in reference to the repulsion of caloric, in the 12th book of the *Mécanique Céleste*, to conclude that, in a fluid considered as a continuous mass, the elasticity is proportional to the square of the density. If then k represents a constant coefficient, we shall have $\varepsilon=\frac{1}{2}kq^2$, and by substituting this value in the equations (I) we shall derive the following:

$$k\frac{dq}{dx}=-\frac{dF}{dx}+\frac{dG}{dx}+\frac{dG_1}{dx}+\frac{dG_2}{dx}\cdots\cdots\cdots+\frac{dG_\nu}{dx}+\text{etc.}$$

$$\text{(I)}' \quad k\frac{dq}{dy} = -\frac{dF}{dy} + \frac{dG}{dy} + \frac{dG_1}{dy} + \frac{dG_2}{dy} \ldots\ldots + \frac{dG_\nu}{dy} + \text{etc.}$$

$$k\frac{dq}{dz} = -\frac{dF}{dz} + \frac{dG}{dz} + \frac{dG_1}{dz} + \frac{dG_2}{dz} \ldots\ldots + \frac{dG_\nu}{dz} + \text{etc.},$$

which lead directly to the complete integral

$$\text{(III)} \qquad k\,q = C - F + G + G_1 + G_2 \ldots\ldots G_\nu + \text{etc.};$$

C being an arbitrary constant.

In order to determine, by means of this equation, the density q, we must substitute for F, G, G_1, G_2, ... G_ν, &c. the integrals which they represent. If the rectangular co-ordinates are changed into polar co-ordinates by means of the known formulæ

$$x = r \sin\theta \cos\psi \qquad y = r \sin\theta \sin\psi \qquad z = r\cos\theta$$

$$x' = r' \sin\theta' \cos\psi' \qquad y' = r' \sin\theta' \sin\psi' \qquad z' = r' \cos\theta'$$

the expression for F takes the form (see the additions to the *Connaissance des Temps* for the year 1829, p. 356)

$$\text{(IV)} \quad F = \sum_0^\infty \left[\frac{1}{r^{n+1}} \iint \left(\int_0^r f q' r'^{n+2} dr'\right) P_n \sin\theta' d\theta' d\psi'\right]$$

$$+ \sum_0^\infty \left[r^n \iint \left(\int_r^\infty \frac{fq'}{r'^{n+1}}\right) P_n \sin\theta' d\theta' d\psi'\right]$$

The coefficient P_n being given by the formula

$$P_n = \frac{1.3.5..2n-1)}{1.2.3.\ldots\ldots n}$$

$$\left\{p^n - \frac{n(n-1)}{2(2n-1)}p^{n-2} + \frac{n(n-1)(n-2)(n-3)}{2.3(2n-1)(2n-3)}p^{n-4} + \text{etc.}\right\}$$

in which

$$p = \cos\theta\cos\theta' + \sin\theta\sin\theta'\cos(\psi - \psi'),$$

and the limits of the integrals relative to θ' and ψ' should be such that the value of F may take in the whole space, except the small portions occupied by the material molecules.

In order to have the expression for G, let us in like manner put

$$\xi = \rho \sin\omega \cos\phi, \qquad \eta = \rho \sin\omega \sin\phi, \qquad \zeta = \rho \cos\omega$$

and represent by Π_n the function P_n, when r', θ', ψ', are therein changed into ρ, ω, ϕ. Then, if we suppose the origin of the co-ordinates to be taken in the interior of the molecule, we shall have (see *Connaissance des Temps* for the year 1829, p. 357)

$$\text{(V)} \qquad G = \sum_0^\infty \left[\frac{1}{r^{n+1}} \iint \left(\int_0^r g\varpi\rho^{n+2} d\rho\right) \Pi_n \sin\omega\, d\omega\, d\phi\right]$$

$$+\sum_0^\infty\left[\frac{1}{r^{n+1}}\int\int\left(\int_0^r g\varpi\rho^{n+2}d\rho\right)\Pi_n\sin\omega\,d\omega\,d\varphi\right]$$

$$+\sum_0^\infty\left[r^n\int_{,}\int_{,}\left(\int_r^u\frac{g\varpi}{\rho^{n-1}}\right)\Pi_n\sin\omega\,d\omega\,d\varphi\right].$$

The double integral $\int'\int'$ is to be extended only to the points in respect to which the radius u from the surface of the molecule is $\angle\, r$, and the integral $\int_{,}\int_{,}$ is to be extended to the points in respect to which $u > r$.

By means of a beautiful theorem which M. Poisson has demonstrated in the Memoir already quoted, and in the additions to the *Connaissance des Temps* for the year 1831, the functions given by the integrals

$$\int_0^r g\varpi\rho^{n+2}d\rho, \qquad \int_r^u g\varpi\rho^{n+2}d\rho, \qquad \int_r^u\frac{g\varpi}{\rho^{n-1}}d\rho$$

may be represented by series of integer and rational functions of the spherical co-ordinates. Let $\Sigma\, H_n$, $\Sigma\, H'_n$, $\Sigma\, H''_n$, be these series; if the functions H'_n, H''_n shall be found, so that they may be discontinued, and such that they are reduced to zero, the first for all the values of $u \angle r$, and the second for the values of $u \angle r$, we shall be able by means of the known theorems to reduce the expression for G to the form

$$G=\sum_0^\infty\frac{4\pi}{2n+1}\left(\frac{1}{r^{n+1}}H_n+\frac{1}{r^{n+1}}H'_n+r^nH''_r\right).$$

Such are the expressions for F and G which should be introduced into the equation (III). We might directly employ those which give the values of G, because they are always determinable when the position, figure, and density of the molecules are known; but the same thing cannot be done with the expression for F. This integral includes the quantity q, which is still unknown; and we should not be able to determine it by the condition that it would render the equation (III) identical without previously performing the integrations, an operation which would require the same function to be known. In order to avoid this difficulty, we are about to employ for the purpose of determining q a differential equation corresponding with that marked (III), but in which the density q is no longer included under the signs of integration.

The sum of these equations (I)′, when they are differentiated in reference to x, y, z, respectively, gives

(VI.) $$k\left(\frac{d^2q}{dx^2}+\frac{d^2q}{dy^2}+\frac{d^2q}{dz^2}\right)=4\pi fq.$$

and it being observed that

$$\frac{d^2F}{dx^2}+\frac{d^2F}{dy^2}+\frac{d^2F}{dz^2}=-4\pi f q, \qquad \frac{d^2G}{dx^2}+\frac{d^2G}{dy^2}+\frac{d^2G}{dz^2}=0$$

with respect to which see the third volume of the *Bulletin de la Société Philomatique*, p. 388.

If in this equation we change the differentials taken relatively to the rectangular co-ordinates into differentials taken relatively to the polar co-ordinates, we have

$$(1)\ k\left\{\frac{d^2rq}{dr^2}+\frac{1}{r^2\sin\theta}\,\frac{d\left(\sin\theta\,\frac{drq}{d\theta}\right)}{d\theta}+\frac{1}{r^2\sin^2\theta}\,\frac{d^2rq}{d\psi^2}\right\}=4\pi f\,r\,q.$$

Let us suppose that $r\,q$ is developed in a series of integer and rational functions of the spherical co-ordinates, so that we may have

$$(2) \qquad rq=Q_0+Q_1+Q_2\,\ldots\ldots\,+Q_i+\text{etc.};$$

in which any one of the quantities Q_i renders identical the equation

$$(3) \qquad \frac{d\left(\sin\theta\,\frac{dQ_i}{d\theta}\right)}{\sin\theta\,d\theta}+\frac{1}{\sin^2\theta}\,\frac{d^2Q_i}{d\Psi^2}+i\,(i+1)\,Q_i=0.$$

On this supposition the equation (1) will be satisfied by taking in general

$$(4) \qquad k\left\{\frac{d^2Q_i}{dr^2}-\frac{i\,(i+1)}{r^2}\,Q_i\right\}=4\pi f\,Q_i.$$

In order to integrate this differential equation of the second order* let us take

$$Q_i=\frac{\overset{(1)}{Q}_i}{r}-\frac{1}{i}\,\frac{d\overset{(1)}{Q}_i}{dr},$$

and consequently

$$\frac{dQ_i}{dr}=-\frac{\overset{(1)}{Q}_i}{r^2}+\frac{1}{r}\,\frac{d\overset{(1)}{Q}_i}{dr}-\frac{1}{i}\,\frac{d^2\overset{(1)}{Q}_i}{dr^2}$$

$$\frac{d^2Q_i}{dr^2}=2\,\frac{\overset{(1)}{Q}_i}{r^3}-\frac{2}{r^2}\,\frac{d\overset{(1)}{Q}_i}{dr}+\frac{1}{r}\,\frac{d^2\overset{(1)}{Q}_i}{dr^2}-\frac{1}{i}\,\frac{d^3\overset{(1)}{Q}_i}{dr^3}.$$

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* The integration of this equation with the second member negative has also exercised the ingenuity of the two illustrious geometers Plana and Paoli. See the Memoirs of the Academy of Turin, vol. xxvi., and those of the Italian Society, vol. xx.

By making the substitutions in the equation (4) we shall be able to exhibit the result in the following form:

$$k\left[\frac{1}{r}\left\{\frac{d^2Q_i^{(1)}}{dr^2}-\frac{(i-1)i}{r^2}Q_i^{(1)}\right\}-\frac{1}{i}\left\{\frac{d^3Q_i^{(1)}}{dr^3}-\frac{(i-1)i^{(1)}}{r^2}\frac{dQ^{i\,(1)}}{dr}\right.\right.$$
$$\left.\left.+\frac{2i(i-1)}{r^3}Q_i^{(1)}\right\}\right]=\frac{4\pi f}{r}Q_i^{(1)}-\frac{4\pi f}{i}\frac{dQ_i^{(1)}}{dr}.$$

The foregoing equation is satisfied by taking

$$k\left(\frac{d^2Q_i^{(1)}}{dr^2}-\frac{(i-1)i}{r^2}Q_i^{(1)}\right)=4\pi fQ_i^{(1)}.$$

This equation is of the same form with that proposed (4), except only that i is replaced by $i-1$. If therefore again, in the latter, we put

$$Q_i^{(1)}=\frac{1}{r}Q_i^{(2)}-\frac{1}{i-1}\frac{dQ_i^{(2)}}{dr}$$

we shall deduce from it another in terms of $Q_i^{(2)}$, in which $i-1$ will be replaced by $i-2$; and by continuing these substitutions we shall finally obtain the equation

$$k\frac{d^2Q_i^{(i)}}{dr^2}=4\pi fQ_i^{(i)};$$

which is integrable by the known methods, and gives

$$Q_i^{(i)}=T_ie^{r\sqrt{\frac{4\pi f}{k}}}+V_ie^{-r\sqrt{\frac{4\pi f}{k}}}$$

where T_i and V_i may be considered as two arbitrary functions of θ and ψ of the order i which satisfy the equation (3).

By adopting this value $Q_i^{(i)}$, and by afterwards taking

$$Q_i^{(i-1)}=\frac{1}{r}Q_i^{(i)}-\frac{dQ_i^{(i)}}{dr}$$

$$Q_i^{(i-2)}=\frac{1}{r}Q_i^{(i-1)}-\frac{1}{2}\frac{dQ_i^{(i-1)}}{dr}$$

$$Q_i^{(i-3)}=\frac{1}{r}Q_i^{(i-2)}-\frac{1}{3}\frac{dQ_i^{(i-2)}}{dr}$$

....

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... .

$$Q_i = \frac{1}{r}\; \overset{(1)}{Q_i} - \frac{1}{i} \frac{d\, \overset{(1)}{Q_i}}{d\, r}$$

the last of these quantities will satisfy the equation (4), and will be its complete integral.

If the successive substitutions are performed, and, for brevity's sake, we make

$$\overset{(i)}{a_i} = \frac{(-1)^i}{1.2.3..i}; \quad \overset{(i-\iota)}{a_i} = \left[-\frac{1}{i}\right]^i \Sigma \frac{\left(1 + \frac{\iota - 1}{i+1}\right) \overset{(i-\iota)}{a_{(i-1)}}}{\left[-\frac{1}{i+1}\right]^{i+1}} :*$$

which gives, in the particular case of $\iota = i$, $\overset{(0)}{a_i} = \left[1 + \frac{i-1}{i}\right]^i$, we shall have

$$Q_i = \frac{\overset{(0)}{a_i}}{r^i} \overset{(i)}{Q_i} + \frac{\overset{(1)}{a_i}}{r^{i-1}} \frac{d\, \overset{(i)}{Q_i}}{d\, r} + \frac{\overset{(2)}{a_i}}{r^{i-2}} \frac{d^2\, \overset{(i)}{Q_i}}{d\, r^2} \ldots\ldots\ldots + \overset{(i)}{a_i} \frac{d^i\, \overset{(i)}{Q_i}}{d\, r^i}.$$

Now if we make

$$\Omega_i(r') = \left\{ \frac{\overset{(0)}{a_i}}{r'^i} + \frac{\overset{(1)}{a_i}}{r'^{i-1}}\,\alpha + \frac{\overset{(2)}{a}}{r'^{i-2}}\,\alpha^2 \ldots\ldots + \frac{\overset{(i)}{a_i}}{r'^0}\,\alpha^i \right\} e^{\alpha r'},$$

$$\Omega'_i(r') = \left\{ \frac{\overset{(0)}{a_i}}{r'^i} - \frac{\overset{(1)}{a_i}}{r'^{i-1}}\,\alpha + \frac{\overset{(2)}{a_i}}{r'^{i-2}}\,\alpha^2 \ldots\ldots + \frac{\overset{(i)}{a_i}}{r'^0}\,\alpha^i \right\} e^{-\alpha r'},$$

where α is put instead of $\sqrt{\frac{4\pi f}{k}}$, we shall have

$$Q'_i = T'_i\, \Omega_i(r') + V'_i\, \Omega'(r'),$$

and the expression for F may take the form

$$(5)\ F = \Sigma_0^\infty \iiint f \left\{ \frac{1}{r'^{n+1}} \int_0^r \Sigma_0^\infty \Omega_i(r')\, T'_i\, r'^{n+1}\, d\, r' + r^n \int_r^\infty \Sigma_0^\infty \frac{\Omega_i(r')\, T'_i\, d\, r'}{r'^n} \right\} P_n \sin\theta'\, d\,\theta'\, d\,\psi'$$

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* The brackets are here employed in the same way as in Vandermonde's notation.

$$= \sum_0^\infty \iiint \left\{ \frac{1}{r^{n+1}} \int_0^r \sum_0^\infty \Omega'_i(r')\, V'_i\, r'^{\,n+1}\, dr' \right.$$

$$\left. + \, r^n \int_r^\infty \sum_0^\infty \frac{\Omega'_i(r')\, V'_i\, d r'}{r'^{\,n}} \right\} P_n \sin\theta'\, d\theta'\, d\psi'.$$

The functions T', V'_i of this expression remain arbitrary; and, as the sum of an infinite number of these functions may be employed to represent any function whatsoever, they will serve as two arbitrary functions which are to complete the integral of the equation (1).

When in some particular cases the integrations of the preceding formula shall have been performed by substituting its expression in the equation (II), the functions T_i and V_i will be determined by comparing them with those of the same order introduced by means of the different expressions for G; so that this equation may become identical. All being thus determined, the density q given by the formula (2) will be known.

We have hitherto left our formulæ in all their generality, so that one may be the better able to judge of the restrictions to which we shall subject them while making the first applications of them. In the present state of our physical knowledge, the figure of the material molecules is totally unknown. We will therefore begin by considering the most simple case,—that in which their form is spherical, and their density uniform. We will, besides, assign to these molecules a very small volume, and suppose them in their state of equilibrium at a mutual distance, which is very considerable as compared with their dimensions. This manner of considering the constitution of bodies has been adopted by several philosophers as that which is most conformable to truth, and presents at the same time a considerable advantage in an analytical point of view. In adopting it we shall be able, by approximation, to consider the æther as if it were continuously diffused in all directions; and to disregard, in the integration of the formula (5), the small spaces occupied by the material molecules. But as, by proceeding in this manner, we should include in the repulsion of the æther a surplus which is due to the actions answering to the points of space which are really occupied by the molecules, we shall compensate for this surplus by adding to the action of each molecule an action equal and contrary to that of a quantity of æther of the same volume as the molecule, and of the same density as that which answers to the point of space which the molecule occupies. This is done by substituting $g\varpi + f\mathrm{q}$ for $g\varpi$ in the expression for G (q representing the density which the æther would have at the point occupied by the molecule, and within so small a space we will suppose that density constant), and by extending the integrals of the formula (5) from $\theta' = 0$ to $\theta' = \pi$, from $\psi' = 0$ to $\psi' = 2\pi$, and from $r = 0$ to $r = \infty$.

Let us begin with performing the integrations of the formula (V). In consequence of the quantity $g\varpi + f\mathrm{q}$ being considered as constant, and as the spherical form of the molecules renders ρ independent of ω and φ, all the terms of the second and third line of this formula will vanish, and it being observed that we always have

$$\int_0^{\pi}\int_0^{2\pi} \Pi_n \sin\omega \, d\omega \, d\varphi = 0,$$

unless in the case of $\Pi^\circ = 1$, which gives

$$\int_0^{\pi}\int_0^{2\pi} \Pi_0 \sin\omega \, d\omega \, d\varphi = 4\pi;$$

the expression for G will become $G = \frac{4\pi}{3} \; \frac{(g\varpi + f\mathrm{q})\,\delta^3}{r}$, δ representing the semidiameter of the molecule.

This integral has been obtained under the supposition that the origin of the coordinates is in the centre of the molecule; but the origin may be transferred to any point whatever, by restoring, instead of r, its general expression, and writing

$$(\mathrm{V})' \qquad G = \frac{4\pi(g\varpi + f\mathrm{q})\,\delta^3}{3\{(x-\mathrm{x})^2 + (y-\mathrm{y})^2 + (z-\mathrm{z})^2\}^{\frac{1}{2}}};$$

where x, y, z represent the coordinates of the centre of the molecule.

Before we proceed to the expression for F, we had better clearly define the signification of the term q which it contains. We must consider this quantity (q) such as it is given by the equation (III), not as the entire value of the density of the æther, but as the value only of its excess or deficiency above or below the sensibly uniform density which the æther diffused in equilibrium is supposed to have in that part of space. If we represent the latter density by q_0, the equations (III) and (VI), while we suppress the terms due to the quantities G, G_1, G_2, &c., must be satisfied by the substitution of $q = q_0$: and that, in order that the æther may remain in equilibrium spontaneously, or in consequence of the action of the forces not expressed, whose centres must be supposed to be at a very great distance. If, therefore, we take the difference between the equations resulting from this substitution and the original equations themselves, we shall have

$$(\mathrm{III})' \quad k(q - q_0) = -F + G + G_1 + G_2 \ldots\ldots + G_\nu + \&c.$$

$$\frac{d^2(q-q_0)}{dx^2} + \frac{d^2(q-q_0)}{dy^2} + \frac{d^2(q-q_0)}{dz^2} = 4kf(q-q_0)$$

provided that, in F, we substitute for q the value of $q - q_0$ resulting from this last equation, that is to say, that which is given by the formula

$$(2)' \qquad r(q - q_0) = Q_0 + Q_1 + Q_2 \ldots\ldots + Q_i + \&c.$$

This being premised let us return to the formula (5). As the integrations indicated in the second member of this equation may, according to what we have stated at the commencement of this paragraph, be extended from $r' = o$ to $r' = \infty$, $\theta' = o$ to $\theta' = \pi$, and $\psi' = o$ to $\psi' = 2\pi$, and as all these limits are independent of each other, observing that we have in general

$$\int_o^\pi \int_o^{2\pi} P_n T'_i \sin\theta' \, d\theta' \, d\psi' = o$$

$$\int_o^\pi \int^{2\pi} P_n V'_i \sin\theta' \, d\theta' \, d\psi' = o$$

and in particular when $i = n$;

$$\int_o^\pi \int_o^{2\pi} P_n T'_n \sin\theta' \, d\theta' \, d\psi' = \frac{4\pi}{2n+1} T_n;$$

$$\int_o^\pi \int_o^{2\pi} P_n V'_n \sin\theta' \, d\theta' \, d\psi' = \frac{4\pi}{2n+1} V_n.$$

we shall find

$$F = \Sigma_o^\infty \frac{4\pi}{2n+1} \frac{f}{r^{n+1}} \left\{ T_n \int_o^r \Omega_n(r') \, r'^{\,n+1} \, dr' \right.$$

$$\left. + V_n \int_o^r \Omega'_n(r') \, r'^{\,n+1} \, dr' \right\}$$

$$+ \Sigma_o^\infty \frac{4\pi}{2n+1} f r^n \left\{ T_n \int_r^\infty \frac{\Omega_n(r')}{r'^n} \, dr' \right.$$

$$\left. + V_n \int_r^\infty \frac{\Omega'_n(r')}{r'^n} \, dr' \right\}$$

Without actually making the substitutions of the expressions previously given for G, and latterly for F, in the equation (III)$'$ for the purpose of comparing the functions of the spherical coordinates of the same degree which are to render it identical, we see that, as G, G_1, G_2, &c., contain none of these functions, all the T_n and V_n must be null, with the exception of T_0 and V_0, which answer to the value $n = o$, and represent two arbitrary constants.

The expression for F will then be reduced to

$$(5)' \qquad F = 4\pi f \frac{1}{r}\int_0^r \left(T_0 e^{\alpha r'} + V_0 e^{-\alpha r'}\right) r'\, dr'$$

$$+ 4\pi f \int_r^\infty \left(T_0 e^{\alpha r'} + V_0 e^{-\alpha r'}\right) dr'$$

All the quantities T_n and V_n being null, except T_0 and V_0, the values of Q_n will also be null, except that of Q_0 : the formula (2)′ will then give $q - q_0 = \dfrac{T_0 e^{\alpha r} + V_0 e^{-\alpha r}}{r}$

When $r = \infty$ we must have $q = q_0$; we must then also have $T_0 = 0$, and there will remain only $q = q_0 + \dfrac{V_0}{r} e^{-ar}$.

By performing the integrations of the formula (5)′ within the limits indicated, and observing that $T_0 = 0$, we shall obtain

$$F = -k \frac{V_0}{r}\left(e^{-\alpha r} - 1\right);$$

As, in the differential expression for F, we may change x' into $x' - \mathrm{x}$, and x into $x - \mathrm{x}$, without any change taking place in its value, and as a similar change may be made in respect to the other coordinates, it follows that, by taking the point x, y, z, as the origin of the coordinates, we shall be able, in the two preceding formulæ, to put

$$r = \sqrt{(x - \mathrm{x})^2 + (y - \mathrm{y})^2 + (z - \mathrm{z})^2}$$

or, generally, $r_\nu = \sqrt{(x - \mathrm{x}_\nu)^2 + (y - \mathrm{y}_\nu)^2 + (z - \mathrm{z}_\nu)^2}$.

Now if, by placing the origin of the coordinates in the centre of each molecule respectively, we substitute these expressions of F and q, and that previously found for G in the equation (III)′, and take successively for V_0 as many constants as there are molecules, we shall find that the equation

$$\Sigma\, k\, \overset{(\nu)}{V_0} \frac{e^{-\alpha r_\nu}}{r_\nu} = \Sigma\, k\, \overset{(\nu)}{V_0} \frac{e^{-\alpha r_\nu} - 1}{r_\nu} + \Sigma \frac{4\pi (g\,\varpi_\nu + f\,\mathrm{q}_\nu)\,\delta_\nu^3}{3\, r_\nu}$$

will be satisfied by taking for each molecule

$$\overset{(\nu)}{V_0} = \frac{4\pi}{3k}\left(g\,\varpi_\nu + f\,\mathrm{q}_\nu\right)\delta_\nu^3.$$

By substituting for $\overset{(\nu)}{V_0}$ the value just found, we shall finally have

$$(\mathrm{IV})' \qquad F = -\frac{4\pi}{3}\,\Sigma\left(g\,\varpi_\nu + f\,\mathrm{q}_\nu\right)\delta_\nu^3\,\frac{e^{-\alpha r_\nu} - 1}{r_\nu}$$

$$\text{(III)}'' \qquad q = q_0 + \frac{4\pi}{3k}\Sigma(g\,\varpi_\nu + f\,\mathrm{q}_\nu)\,\delta_\nu^3\,\frac{e^{-\alpha r_\nu}}{r_\nu}.$$

where the sums Σ are to be extended to all the molecules, including the first.

This last equation determines what the density of the æther must be at each point $x\,y\,z$, in order that it may be in equilibrium when it is submitted to the action of the spherical molecules of matter. The value of this density consists of different terms, each of which is due to a particular molecule, and represents its proper atmosphere. As the quantity of æther diffused through the immensity of space may be considered as infinite, the atmosphere formed by each molecule for itself is always the same, and its density is only superadded to that which the æther in the same places owes to other causes. According to the nature of the molecular actions, the value of the coefficient $\alpha = \sqrt{\frac{4\pi f}{k}}$ should be considered as very great: hence it follows that the density of each atmosphere will be incomparably greater when quite near or in contact with the molecule, and will decrease very rapidly as its distance from the molecule increases. This circumstance enables us to determine with ease, by approximation, the value of q_ν, or the density of the æther at the surface of any molecule whatsoever, on the supposition that the molecules are not too near each other. If, for instance, we make $r = \delta$ in the term answering to the first molecule, and $r_1 = \mathrm{r}_1$, $r_2 = \mathrm{r}_2 \ldots r_\nu = \mathrm{r}_\nu$ in the other terms, all these will be very small in comparison with the first, and by neglecting them we shall have very nearly

$$\mathrm{q} = q_0 + \frac{4\pi}{3k}(g\,\varpi + f\mathrm{q})\,\delta^2$$

whence we derive

$$(6) \qquad \mathrm{q} = \frac{q_0 + \frac{4\pi}{3k} g\,\varpi\,\delta^2}{1 + \frac{4\pi}{3k} f\delta^2}.$$

6. We are now in a condition to consider the equilibrium of any molecule whatever, such as it is given by the equations (II).

The quantity ϵ under the double integral in these equations must be replaced by $\frac{1}{2}\,k\,q^2$. Let us represent the coordinates x, y, z, so far as they belong to the points in contact with the surface of the molecule, by $\mathrm{x} + \xi$, $\mathrm{y} + \eta$, $\mathrm{z} + \zeta$; x, y, z being the coordinates of its centre: by developing the expression for q, and stopping, because of the smallness of the molecule, at the first terms, we shall be able to take

$$q^2 = \mathrm{q}^2 + 2\,\mathrm{q}\frac{d\mathrm{q}}{d\mathrm{x}}\xi + 2\,\mathrm{q}\frac{d\mathrm{q}}{d\mathrm{y}}\eta + 2\,\mathrm{q}\frac{d\mathrm{q}}{d\mathrm{z}}\zeta.$$

If this expression for q^2 be put in the integral $\frac{1}{2} k \int q^2\, d\eta\, d\zeta$, and the limits extended to the whole surface of the molecule, it is easy to see that it is reduced to $k\, \mathrm{q} \frac{d\mathrm{q}}{d\mathrm{x}} \iiint \xi\, d\eta\, d\zeta$. But $\iiint \xi\, d\eta\, d\zeta$ expresses the volume v of the molecule, which is equal to $\frac{4\pi}{3} \delta^3$; the term on the right in the first of the equations (II) will therefore be simply represented by $k\, \mathrm{v}\, \mathrm{q} \frac{d\mathrm{q}}{d\mathrm{x}}$. It is proper to remark, that in the value of $\mathrm{q} \frac{d\mathrm{q}}{d\mathrm{x}}$, we are not to include the term which, in the expression for q marked (III)$''$ is due to the molecule whose equilibrium we are considering, because this term undergoes a change of sign at the two opposite sides of the surface of the molecule, and vanishes within the limits between which the integral is extended.

The inspection of the triple integral which gives the value Φ is sufficient to show that this integral must be given by the same function that represents F, in which f, x, y, z may be replaced by g, ξ, η, ζ. If, because of the smallness of the dimensions of the molecule, we consider in the differential $\frac{d\Phi}{d\xi}$, the coordinates ξ, η, ζ, which answer to any point of the surface as being constant, and substitute for them x, y, z which answer to the centre, then, it being observed that $\iiint d\xi\, d\eta\, d\zeta$ represents the volume v of the molecule, the first integral of the second member of the first of the equations (II) may be represented by $\varpi\, \mathrm{v} \frac{d\Phi}{d\mathrm{x}}$.

The value of Φ being deduced from the expression for F, such as it is given by the equation (IV)$'$, will contain, as we have already observed, a surplus of action, due to the æther which is supposed to occupy the place of the molecules also. It will therefore be necessary to make a compensation here also, by adding to the contrary action of the molecules an equal quantity; that is to say, by changing in the triple integral represented by $\Gamma_{,}$, the mass $\gamma \varpi_{,}$ into the mass $\gamma \varpi_{,} + g\, \mathrm{q}_{,}$. If we conceive this change made, the expression for $\Gamma_{,}$ will be of the same form as that for G marked (V)$'$, except that x, y, z and $g\varpi + f\mathrm{q}$ will be replaced by ξ, η, ζ, and $\gamma \varpi_{,} + g\, \mathrm{q}_{,}$, and x, y, z by $\mathrm{x}_{,}$, $\mathrm{y}_{,}$, $\mathrm{z}_{,}$. Let us then, by approximation, introduce into the differential $\frac{d\Gamma_{,}}{d\xi}$, instead of the coordinates (ξ, η, ζ) of the surface, the coordinates $(\mathrm{x}, \mathrm{y}, \mathrm{z})$ of the centre considered as constant; if we perform the integration, which is done by substituting the volume v for $\iiint d\xi\, d\eta\, d\zeta$, the term which stands under the sign Σ in the first of the equations (II) will be represented by $\varpi\, \mathrm{v}\, \frac{d\,\Gamma_\nu}{d\,\mathrm{x}}$.

If we now write in their places all the expressions just found for the integrals which constitute the first of the equations (II), we shall have

$$k\, q\, \mathrm{v} \frac{d\,\mathrm{q}}{d\,\mathrm{x}} = \varpi \mathrm{v} \frac{d\,\Phi}{d\,\mathrm{x}} - \varpi\, \mathrm{v}\, \Sigma \frac{d\,\Gamma_\nu}{d\,\mathrm{x}}.$$

By similar substitutions the second and the third equation will give respectively

$$k\, \mathrm{q}\, \mathrm{v} \frac{d\,\mathrm{q}}{d\,\mathrm{y}} = \varpi\, \mathrm{v} \frac{d\,\Phi}{d\,\mathrm{y}} - \varpi\, \mathrm{v}\, \Sigma \frac{d\Gamma_\nu}{d\,\mathrm{y}}$$

$$k\, \mathrm{q}\, \mathrm{v} \frac{d\,\mathrm{q}}{d\,\mathrm{z}} = \varpi\, \mathrm{v} \frac{d\,\Phi}{d\,\mathrm{z}} - \varpi\, \mathrm{v}\, \Sigma \frac{d\Gamma_\nu}{d\,\mathrm{z}}.$$

These three equations must hold good for the particular values x, y, z; x_1, y_1, z_1;.........x_ν, y_ν, z_ν, &c., which answer to the centre of the molecules in their state of equilibrium; and as each molecule furnishes three similar equations, the whole collectively will be sufficient to enable us to determine the unknown quantities.

If from the formulæ marked (III)″, (IV)′, (V)′ we derive, by means of the changes already indicated, the expressions for $\frac{d\,\mathrm{q}}{d\,\mathrm{x}}$, $\frac{d\,\Phi}{d\,\mathrm{x}}$, $\frac{d\,\Gamma_\nu}{d\,\mathrm{x}}$, we find

$$\frac{d\,\mathrm{q}}{d\,\mathrm{x}} = -\frac{1}{k} \Sigma \mathrm{v}_\nu (g\,\varpi_\nu + f\,\mathrm{q}_\nu) \frac{(1 + \alpha\,\mathrm{r}_\nu)\, e^{-\alpha\,\mathrm{r}_\nu}}{\mathrm{r}_\nu^2} \cdot \frac{\mathrm{x}_\nu - \mathrm{x}}{\mathrm{r}_\nu}$$

$$\frac{d\,\Phi}{d\,\mathrm{x}} = \frac{g}{f} \Sigma\, \mathrm{v}_\nu\, (g\,\varpi_\nu + f\,\mathrm{q}_\nu) \left\{ \frac{(1 + \alpha\,\mathrm{r}_\nu)\, e^{-\alpha\,\mathrm{r}_\nu}}{\mathrm{r}_\nu^2} - \frac{1}{\mathrm{r}_\nu^2} \right\} \frac{\mathrm{x}_\nu - \mathrm{x}}{\mathrm{r}_\nu}$$

$$\frac{d\,\Gamma_\nu}{d\,\mathrm{x}} = -\mathrm{v}^\nu\, (\gamma\,\varpi_\nu + g\,\mathrm{q}_\nu)\, \frac{1}{\mathrm{r}_\nu^2} \cdot \frac{\mathrm{x}_\nu - \mathrm{x}}{\mathrm{r}_\nu}$$

and shall obtain $\frac{d\,\mathrm{q}}{d\,\mathrm{y}}$, $\frac{d\,\Phi}{d\,\mathrm{y}}$, $\frac{d\,\Gamma_\nu}{d\,\mathrm{y}}$; $\frac{d\,\mathrm{q}}{d\,\mathrm{z}}$, &c., by changing in these formulæ x into y and into z.

If we introduce these expressions into the foregoing equations, recollecting that, according to the hypothesis of Franklin and Æpinus, we must make $f = g$, and take γ a little less than g, the result will be

$$g\,v\,(q+\varpi)\,\Sigma\,v_\nu\,(\varpi_\nu+q_\nu)\,(1+\alpha\,r_\nu)\,e^{-\alpha r_\nu}\,\frac{x_\nu-x}{r_\nu^3}$$

$$-\,(g-\gamma)\,\varpi\,v\,\Sigma\,\varpi_\nu\,v_\nu\,\frac{x_\nu-x}{r_\nu^3}=0$$

$$\text{(A)}\qquad g\,v\,(q+\varpi)\,\Sigma\,v_\nu\,(\varpi_\nu+q_\nu)\,(1+\alpha\,r_\nu)\,e^{-\alpha r_\nu}\,\frac{y_\nu-y}{r_\nu^3}$$

$$-\,(g-\gamma)\,\varpi\,v\,\Sigma\,\varpi_\nu v_\nu\,\frac{y_\nu-y}{r^3}=0$$

$$g\,v\,(q+\varpi)\,\Sigma\,v_\nu\,(\varpi_\nu+q_\nu)\,(1+\alpha\,r_\nu)\,e^{-\alpha r_\nu}\,\frac{z_\nu-z}{r_\nu^3}$$

$$-\,(g-\gamma)\,\varpi\,v\,\Sigma\,\varpi_\nu\,v_\nu\,\frac{z_\nu-z}{r_\nu^3}=0,$$

where the sums Σ are to be extended to all the members ν, that is to say, to all the molecules except that whose equilibrium we are considering.

7. The equations which we have just found are those which must take place in case of equilibrium, or in the natural state of a body composed of spherical molecules, if Franklin's hypothesis respecting statical electricity may be applied to the constitution of bodies also. The form in which the equations present themselves shows that this equilibrium takes place exactly as if there existed between each pair of molecules a reciprocal action, in the direction of the straight line which would join their centres of gravity, and would be represented by

$$(a)\ g\,v\,(\varpi+q)\,v_1\,(\varpi_1+q_1)\,\frac{(1+\alpha r_1)\,e^{-\alpha r^1}}{r_1^2}-(g-\gamma)\,v\,\varpi.\,v_1\,\varpi_1\frac{1}{r_1^2}.$$

Let us examine the nature of this action. We are able to distinguish in its expression the products $g\,v\,(\varpi+q).\ v_1\,(\varpi_1+q_1)$, $(g-\gamma)\,\varpi\,v.\ \varpi_1\,v_1$, the constant α and the variable r_1.

As the difference $(g-\gamma)$ between these two accelerative forces is to be supposed very small relatively to g, the product of this force by the masses $v\,(\varpi+q)\ v_1\ (\varpi_1+q_1)$ will, for a twofold reason, be greater than the product of the difference $g-\gamma$ by the masses $\varpi\,v.\ \varpi_1\,v_1$.

The value α depends on that of f and k, that is to say on the repulsive force of the atoms of the æther, their mutual distances, their masses, and their volumes, which are all unknown to us. The agreement of the results of calculation with those of experiment requires that α should be a very high number.

On the condition that α is very great, the first term of the expression (a) will decrease rapidly with r_1, because of the multiplier $e^{-\alpha r_1}$; if then r_1 has a greater value than that which renders this expression null, the force represented by the last term will preponderate over that represented by the first; and if r_1 be so great that this term may be neglected as of no value, then the only remaining force will be that given by the last term. This term being negative, the force which corresponds with it tends to bring the molecules nearer to each other; and as it is in the direct ratio of the product of the masses, and the inverse ratio of the square of the distance, it will exactly represent the *universal gravitation* which takes place at finite distances.

By diminishing r_1 we shall obtain a value that will satisfy the equation

$$(b)\, g\,v(\varpi + q)\,v_1(\varpi_1 + q_1)\,\frac{(1 + \alpha r_1)\,e^{-\alpha r_1}}{r_1^2} - (g - \gamma)\,v\varpi . v_1\varpi_1\,\frac{1}{r_1^2} = 0.$$

At this distance two molecules would remain in equilibrium, and as the differentiation of this equation gives the result

$$-\,g\,v\,(\varpi + q)\,v_1\,(\varpi_1 + q_1)\,\frac{\alpha^2\,e^{-\alpha r_1}}{r_1}$$

which is always negative, the equilibrium will be permanently fixed. Should it be attempted, by the application of an external force, to bring the molecules nearer to each other, the repulsive force represented by the first term of the expression (a), which would now increase in a greater ratio than the attractive force represented by the last term, would produce a resistance to such an approximation: on the other hand, if it should be sought to remove the molecules to a greater distance from each other, the repulsive force would decrease in a greater ratio, and the attractive would preponderate and prevent the separation. These two molecules would therefore be so placed relatively to each other as by mutual adhesion to form a whole, and we should not be able to remove the one without at the same time removing the other. Thus these molecules present a picture in which the *hooked atoms* of Epicurus are as it were generated by the love and hatred of the two different matters of Empedocles.

As the attractive force is null at the distance which we have been just now considering, and at a greater distance decreases as the square of the distance of the molecules, there must be an intermediate point at which it reaches its maximum. By the ordinary rules of the differential calculus we find that the function (a) is a maximum when

$$(c)\, -g\,v\,(\varpi + q)\,v_1\,(\varpi_1 + q_1)\,\left(1 + \alpha r_1 + \frac{1}{2}\alpha^2 r_1^2\right)e^{-\alpha r_1} + (g - \gamma)$$
$$v\,\varpi . v_1\,\varpi_1 = 0;$$

that is to say, that it is at the distance r_1 we should find, by the resolution of this equation, that the molecules attract each other most forcibly.

Recapitulating these results, we shall say then, that the action of two spherical molecules on each other is repulsive, from their point of contact to a distance given by the equation (*b*). At this distance the two molecules are in a state of fixed equilibrium, and as it were linked together; at a greater distance their action is attractive, and the attraction continues to increase until they are at the distance r_1 furnished by the equation (*c*), which distance is still very inconsiderable because of the magnitude of α in the exponential term e^{-ar_1}. From this point the force remains always attractive, and, when the distance has acquired a sensible value, follows the inverse ratio of the square of the distance. All these properties of molecular action flow as necessary consequences from Franklin's hypothesis respecting statical electricity, and appear perfectly conformable to those indicated by the phænomena.

Let us suppose four homogeneous and equal molecules placed at the points of a regular tetrahedron. If we assume as the origin of the coordinates the place occupied by the molecule whose equilibrium it is proposed to consider, and as the plane of the $x\,y$, a plane parallel to that in which the three others are found, the coordinates of these molecules will be given by the formulæ

$$\begin{array}{lll}
x = 0 & y = 0 & z = 0 \\
x_1 = \frac{r}{\sqrt{3}} \cos\beta & y_1 = \frac{r}{\sqrt{3}} \sin\beta & z_1 = r\sqrt{\frac{2}{3}} \\
x_2 = \frac{r}{\sqrt{3}} \cos\left(\beta + \frac{2\pi}{3}\right) & y_2 = \frac{r}{\sqrt{3}} \sin\left(\beta + \frac{2\pi}{3}\right) & z_2 = r\sqrt{\frac{2}{3}} \\
x_3 = \frac{r}{\sqrt{3}} \cos\left(\beta + \frac{4\pi}{3}\right) & y_3 = \frac{r}{\sqrt{3}} \sin\left(\beta + \frac{4\pi}{3}\right) & z_3 = r\sqrt{\frac{2}{3}}
\end{array}$$

where r denotes the mutual distance of the molecules, which is the same for all; β the angle which is formed in the plane of $x\,y$ with the axis of the x, by the projection of the straight line drawn from the molecule placed at the origin of the coordinates to the first of the three others; and π the semicircumference.

If these values be substituted in the three equations (A), and it is observed that we always have, whatever may be the value of β,

$$\cos\beta + \cos\left(\beta + \frac{2\pi}{3}\right) + \cos\left(\beta + \frac{4\pi}{3}\right) = 0,\ \sin\beta + \sin\left(\beta + \frac{2\pi}{3}\right) + \sin\left(\beta + \frac{4\pi}{3}\right) = 0,$$

it will be seen that the two first are verified by themselves, and that the third gives, for the determination of r,

$$g\, \mathrm{v}^2 (\varpi + \mathrm{q})^2 \frac{(1 + \alpha\, \mathrm{r})\, e^{-\alpha \mathrm{r}}}{\mathrm{r}^2} - (g - \gamma)\, \mathrm{v}^2 \varpi^2 \frac{1}{\mathrm{r}^2} = 0.$$

If the density of the æther into which the molecules are plunged, or the quantity q_0, becomes greater, the density q given by the equation (6) will increase also; that value of r which will satisfy the foregoing equation will consequently become greater, and the molecules will fix themselves in equilibrium at a greater distance. We see in this result that the æther performs the functions of caloric, and that it is to its greater or less density we are to ascribe the temperature and volume of the body. For what else, in fact, is an increase or diminution of temperature in respect to a body, than a new state in which its molecules, placed in equilibrium, form, in consequence of their being more or less widely separated, a greater or less volume. It has been known to philosophers since the time of Galileo, who was the first that clearly pointed out this difference, that we are not to confound the sensation which we experience while this new arrangement of the molecules of our body is taking place, with the motion by which it is produced.

NOTE.

[The readers of this Memoir will doubtless be interested in referring to Dr. Roget's "Treatise on Electricity" in the Library of Useful Knowledge, published March 15th, 1828; the following passage from which was noticed with reference to M. Mossotti's views, by Prof. Faraday in his lecture at the Royal Institution, Jan. 20th of the present year.—Edit.]

"(239.) It is a great though a common error to imagine, that the condition assumed by Æpinus, namely that the particles of matter when devoid of electricity repel one another, is in opposition to the law of universal gravitation established by the researches of Newton; for this law applies, in every instance to which inquiry has extended, to matter in its ordinary state; that is, combined with a certain proportion of electric fluid. By supposing, indeed, that the mutual repulsive action between the particles of matter is, by a very small quantity, less than that between the particles of the electric fluid, a small balance would be left in favour of the attraction of neutral bodies for one another, which might constitute the very force which operates under the name of gravitation; and thus both classes of phænomena may be included in the same law."

ARTICLE XXIV.

On certain Combinations of a New Acid, formed of Azote, Sulphur, and Oxygen; by J. PELOUZE.

From the *Annales de Chimie et de Physique,* vol. xvi. p. 151.*

DAVY made the important observation that nitrous gas (deutoxide of azote) was susceptible of absorption by a mixture of potash or soda and an alkaline sulphite, and that from this action a peculiar matter resulted, whose principal characteristic was to disengage abundantly protoxide of azote when brought into contact with acids. His experiments are recorded in vol. xx. of the *Revue Britannique* (for 1802): an extract is also given in the *Traité de Chimie* of Berzelius, vol. ii. p. 50.

Davy, believing the presence of the potash and soda, in a free state, necessary to the absorption of the deutoxide of azote by the sulphites, and remarking moreover that the acids no longer disengaged nitrous gas, but protoxide of azote, from these new combinations, considered them as formed of this latter gas and an alkali, and proposed to give them the name of *nitroxides* of potash and soda. He attempted unsuccessfully to obtain analogous compounds with ammonia, baryta, strontia, and lime.

To explain the disappearance of the deutoxide of azote, Davy supposed that the potash and soda, although incapable of absorbing this gas when wholly formed, might yet take it up during its formation, and combine with it by the action of the affinities which decompose the nitrous gas and convert it into protoxide of azote.

The celebrated English chemist extended his observations no further. The salts which he had obtained not having been disengaged from the extrinsic matter with which they were combined, he was unable either to submit them to analysis, or to examine their principal characteristics; and thus he left their description very incomplete.

Upon passing an aqueous solution of sulphite of ammonia, cooled to the point at which it begins to congeal, into a small tube filled with deutoxide of azote, which has been exposed for several minutes to a temperature of −15° to −20° (Reaum.), the volume of the gas gradually diminishes; and upon withdrawing the tube by degrees from the cooling mixture, and agitating it until the congealed matter is liquified, repeating

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* The Editor is indebted for the translation to Mr. J. E. Taylor.

this operation several times, the deutoxide of azote is completely absorbed by the sulphite; and these two bodies, upon disappearing, give birth to a very remarkable new salt, to which I shall revert presently.

If, instead of causing the deutoxide of azote to act upon the sulphite at — 15°, we bring them in contact at zero, or rather at the ordinary temperature, the result is very different: the deutoxide of azote is completely destroyed, it is true, but it is replaced by half its volume of protoxide of azote, and instead of a new salt we obtain the neutral sulphate of ammonia.

I am not aware that chemistry presents a single similar instance, namely, an example of actions so different at temperatures so nearly equal, above all within the lower limits of the thermometrical scale. It is probable that facts of this nature will eventually multiply, and that by the aid of freezing mixtures we shall succeed in obtaining combinations which, though little stable, may nevertheless offer a composition and definite properties.

To return to the experiment above described: If, when all the deutoxide of azote has been absorbed by the sulphite we leave the solution to itself, at the ordinary temperature, the new salt is gradually destroyed, pure protoxide of azote is disengaged, and the liquid retains only sulphate of ammonia. The volume of the new gas collected is found to be precisely equal to half the volume of deutoxide of azote employed.

The instability of the new substance scarcely allowed me to examine it completely: at zero, it is converted into protoxide of azote and into sulphate of ammonia: at the ordinary temperature, its decomposition is rapid; at 40° and above, its action is violent, and appears like a brisk effervescence; further than this, it is not easy, especially in summer, to operate on the gases in refrigerating mixtures. It was necessary then to contrive another mode of preparation, and the following was the reasoning which led me to discover it. "The salt cannot be what Davy has said, namely a combination of protoxide of azote and an alkali; for, since it is possible to obtain it with a neutral sulphite, the sulphurous acid eliminated from its base by the deutoxide of azote, and converted into sulphuric acid by the absorption of half the oxygen of the latter gas, would infallibly decompose it, and a disengagement of protoxide of azote would be the result; but, on the other hand, Davy obtained these combinations, although impure, at the ordinary temperature; he obtained them with sulphites mixed with free alkalies; the alkalies must therefore increase the stability of the salts in question, and it is probable that by modifying more or less the process of Davy I shall obtain them pure."

This is in fact what takes place. The presence of a free alkali retards in a remarkable manner the decomposition of the precipitated combinations, and furnishes at the same time a means of preparing them with facility. After several attempts, which it is useless to recount here, I arrived at the following process, in which I have best succeeded.

A concentrated solution is made of sulphite of ammonia, which is mixed with five or six times its volume of liquid ammonia; and into this is passed, during several hours, deutoxide of azote: the experiment may be conveniently made in Woolff's apparatus. The gas which is not absorbed by the liquid contained in the first flask, is taken up by that in the second or third. A number of beautiful crystals are seen gradually to deposit themselves, in the same manner as those obtained at a low temperature with the neutral sulphite of ammonia; these are to be washed with ammonia previously cooled, which, beside the advantage of retarding their decomposition, offers that of dissolving less of them than pure water. When the crystals are desiccated, they should be introduced into a well-closed bottle; in this state they undergo no alteration. The same process is applicable to the corresponding salts of potash and soda.

Before passing to the examination of this new class of bodies, I shall detail an experiment which throws the clearest light upon their composition. If a strong solution of caustic potash be passed into a graduated tube, containing a mixture of two volumes of deutoxide of azote and one volume of sulphurous acid, all the gas after some hours disappears. If the deutoxide of azote be in a proportion greater than 2 : 1 to the sulphurous acid, the excess remains free and unabsorbed above the liquor; and if, again, we employ less nitrous gas than the quantity indicated, the new salt will be found always mixed with sulphite of potash: in a word, the two gases, deutoxide of azote and sulphurous acid, never react except in the proportion in volumes of 2 : 1.

It is easy to convince ourselves that the sulphite disappears, and that the salt which replaces it is formed by a new acid. In fact, the red sulphate of manganese, introduced into the tube in which the action takes place, is not discoloured; whilst the sulphuric solution of indigo shows plainly, by the permanence of colour, the absence of nitrates and nitrites; and if, after pouring a salt of baryta into the liquor, we gather the precipitate which is there formed, wash it several times with a diluted solution of potash, and treat it afterwards with nitric acid, it dissolves entirely, and we may thus be assured that there has been no production of sulphate.

These experiments, added to the complete absorption of the deutoxide of azote by a neutral sulphite, at a temperature of —15°, left no doubt in my mind as to the composition of these new salts. Two volumes of sulphurous acid, in acting upon four volumes of deutoxide of azote and one atom of alkali (potash, soda, or ammonia), should produce one atom of a peculiar acid, composed of two atoms of azote, one atom of sulphur, and four atoms of oxygen. This assumption has been verified by the direct analysis of the salts. I call this acid *nitrosulphuric*, and the salts which it forms *nitrosulphates*.

Nitrosulphate of Ammonia.

The nitrosulphate of ammonia is a white salt, of a sharp and slightly bitter taste, with nothing of that of the sulphites; it does not act upon either litmus or turmeric paper; it crystallizes in prisms, with bases of oblique-angular parallelograms, more or less flattened, and terminated in different manners. It is insoluble in alcohol, either warm or cold; it is easily dissolved in water, in which it finally decomposes, with a rapidity proportioned to the elevation of the temperature: the water retains sulphate of ammonia, whilst it disengages a gas which has the properties of the protoxide of azote. Alcohol precipitates this salt from its aqueous solution. Exposed to a temperature of 110°, it resists, but at a few degrees above that point it decomposes with an explosion, caused by the rapid disengagement of protoxide of azote. If thrown upon red-hot coals, it burns with scintillation.

All the acids disengage from it protoxide of azote, and cause it to pass into the state of sulphate of ammonia; this decomposition is slow with carbonic acid gas, but it proceeds with rapidity when it is dissolved in water. In the open air the nitrosulphate of ammonia decomposes gradually, disengages protoxide of azote, effloresces, and yields a residue of pure sulphate of ammonia.

I have above stated that the alkalies increase the stability of the nitrosulphates; this however takes place with the nitrosulphate of ammonia only to a certain degree. The salt, mixed with concentrated caustic ammonia, still decomposes very visibly, though much more slowly than in pure water, and yields moreover the same products. This decomposition agrees well with what we observe in passing a mixture of two volumes of deutoxide of azote and one volume of sulphurous acid into a bell-glass containing liquid ammonia: the absorption is never in this case complete, as it is with the potash; there is invariably a gaseous residue of protoxide of azote; and if the nitrosulphate of ammonia is obtained at the ordinary temperature, by the process which I have indicated, this depends upon the much greater rapidity of its production than of its decomposition. We see by this, that it is possible for a body to be formed and to exist for a certain time at the same temperature at which it is decomposed.

The excessive mobility of the elements of the nitrosulphate of ammonia, and the stability which the alkalies give them, made me think it not impossible that this salt might present phænomena of decomposition of the same class as the singular ones which M. Thénard observed with oxigenated water. In fact this is the case: many bodies which decompose the deutoxide of hydrogen, without either losing or gaining anything, equally decompose the nitrosulphates. Amongst these are the fine spongy platina, oxide of silver, metallic silver, powdered charcoal, oxide of manganese: the two first bodies act moreover with extreme rapidity upon the nitrosulphate of ammonia.

I convinced myself that this remarkable phænomenon was due, as in the case of the oxygenated water, to an action of presence, and that nothing is ever produced but a simple transformation of the nitrosulphate of ammonia into protoxide of azote and sulphate of ammonia. Oxide of silver is not reduced; for if we wash it, after having caused it to decompose a great quantity of salt, it dissolves finally in nitric acid, without the disengagement of red fumes.

It was interesting to attempt to obtain the metallic nitrosulphates by pouring a solution of nitrosulphate of ammonia into salts whose bases were oxides insoluble in water. The experiment was made with liquids previously cooled to several degrees below zero; it gave the following results. Chloride of mercury, the sulphates of zinc and copper, the persulphate of iron, the protonitrate of mercury, the chloruret of chrome, the nitrate of silver, produced a brisk effervescence, which is attributable to a disengagement of the protoxide of azote: there was formed at the same time sulphate of ammonia, which mixed with these saline solutions without lessening their transparence. With acetate of lead there was also an effervescence and production of sulphate of lead.

It would be very difficult to discover the probable cause of these singular phænomena; but from this very cause, of their present inexplicability, they appeared to me the more to merit the attention of chemists; and what indeed is more calculated to excite curiosity than to see a salt, by simple contact with a body which absolutely gives nothing to it and takes nothing from it, decompose with an extreme rapidity, and form new substances, in the midst of which the agent which produces these violent actions remains chemically passive?

We are already acquainted with two bodies, oxygenated water and hydruret of sulphur, possessing the property of decomposing under the influence of a simple action of presence. M. Thénard, to whom we owe the first observation, had foreseen that facts of this kind would multiply, and that they would open to the chemist a new field for inquiry which would enlarge every day.

I should not omit to mention another fact, which brings into still closer connection the nitrosulphates of oxigenated water; namely, that these salts, mixed with alkaline solutions, cease to decompose under the influence of the same bodies which destroy them so rapidly when they are dissolved in pure water. The nitrosulphate of ammonia contains one atom of water; its formula is: $H^6\,Az^2\,S\,Az^2,\,O^4,+HO$.

Nitrosulphate of Potash.

This salt is white, very soluble in water, insoluble in alcohol, without smell, of a slightly bitter taste, without action upon the test papers; it crystallizes in irregular hexagonal prisms, similar to those of nitrate of potash. Exposed to a heat of from 110° to 115°, it is not decomposed, nor does it lose any weight; at a little higher temperature, nearly 130°, it is destroyed; but instead of being converted into sulphate and protoxide of azote, like the nitrosulphate of ammonia, it gives rise to a disengagement of deutoxide of azote, and to a residue of sulphite of potash. The weakest acids disengage from it a gas, which has been found to possess the properties and the composition of protoxide of azote.

Platina sponge, oxide of silver, the sulphates of copper and manganese, chloruret of barium, and acetate of lead, decompose it and produce the neutral sulphate of potash and protoxide of azote: these actions are always manifested with greater slowness than with the nitrosulphate of ammonia. I have already said in general terms that the stability of the nitrosulphate of potash was greater than that of the corresponding ammoniacal compound; it is even sufficient to enable us to employ boiling water as a means of purifying this salt: in this treatment only a small quantity is decomposed; and by washing with very cold water the crystals which are deposited in the solution, we easily separate them from the sulphate of potash with which they are impregnated.

This salt is anhydrous, and is formed of one atom of potash and one atom of nitrosulphuric acid. Its formula is: $Ka^2\,Az^2\,SO^4$. By analysis it has been found to contain 20 parts of protoxide of azote, and 80 of sulphate of potash.

The nitrosulphate of soda is much more soluble; in other respects it appears to me to possess the general properties assigned to the last salt; and as its preparation is difficult, I have not made it the object of particular study.

It only remains for me, in concluding this Memoir, to consider two principal points of view in which it seems to me possible to regard the constitution of the nitrosulphates: whether they are formed by a peculiar acid, composed of two atoms of azote, one atom of sulphur, and four atoms of oxygen; or sulphates combined with protoxide of azote, acting in an analogous manner to the water of crystallization. The first hypothesis seems to me preferable, and the following are the reasons on which it is based:

1. The nitrosulphates are not precipitated by *baryta-water;* and, if the protoxide of azote entered into these salts in the way the water

of crystallization does into the ordinary salts, it is not likely that its presence could modify the sulphates so far as to cause them to lose their most characteristic property, that of forming an insoluble substance with baryta.

2. The nitrosulphate of potash yields, by heat alone, a disengagement of deutoxide of azote and a residue of sulphur. It is little probable that the protoxide of azote could become deutoxide at a temperature of 140°, especially when it must take the oxygen which it wants from so stable a salt as the sulphate of potash. And, moreover, experience has proved to me that the protoxide has no action upon it at that temperature and above. I would add that, if the action of heat upon the nitrosulphate of ammonia induces the belief of the pre-existence of the protoxide of azote in that salt, the entirely different products of the decomposition of the nitrosulphate of potash by the same agent would lead us, adopting the same reasoning, to consider the latter salt as formed of sulphite of potash united with deutoxide of azote.

I am more inclined to see in the action of heat a disorganizing power, whose effects vary with the nature of the substances upon which it is exerted. The question seems to me to be precisely the same as that of the nitrates and hyposulphites, from which it has not been possible to abstract the hyposulphurous and nitrous acids; only that, instead of two elements, the nitrosulphuric acid contains three, of which there are examples enough in chemistry.

I have endeavoured to isolate this acid, and to prepare it directly, without the influences of the bases: in this I have not succeeded; but in the course of my attempts I have had occasion to remark a curious fact, which is at variance with all that has been said and written upon the theory of the formation of sulphuric acid; namely, that the deutoxide of azote and sulphurous acid are able to produce sulphuric acid without the necessary presence of the air or of oxygen. The experiment is easily performed, and I have repeated it many times. Two hundred volumes of deutoxide of azote and one hundred of sulphurous acid, left alone for some hours at the ordinary temperature, in a graduated tube containing a small quantity of boiled water, are converted into pure sulphuric acid and a residue of protoxide of azote equal to one hundred volumes: such is the result; as to the theory, I am induced to believe that nitrosulphuric acid is at first formed, and is afterwards decomposed in the same manner and with still greater facility than the nitrosulphates.

Hence the theory, or rather theories, on the formation of sulphuric acid, in the forms in which they have been propounded, must undergo a notable modification; for a certain quantity of protoxide of azote must necessarily be produced in the leaden chambers. I have for a long time past been occupied with experiments relative to this subject, and I hope shortly to publish the results.

ARTICLE XXV.

Attempt to explain the Absorption of Light according to the Undulatory Theory; by BARON FABIAN VON WREDE.

From Poggendorff's *Annalen der Physik and Chemie*, vol. xxxiii. No. 23. p. 353. Nos. 24 and 25.*

THE mathematical accuracy with which most optical phænomena are explained according to the theory of undulations, and the simplicity of the first principles of this theory, as well as the analogy which it presupposes between both the means with which Nature has endowed the living being for the purpose of enabling it to perceive and distinguish those external objects by which it is surrounded, render this theory in itself highly probable, although we have not yet been fortunate enough to bring under its general laws all the simple phænomena. Though we do not consider ourselves authorized to judge of the relative value of this theory, yet everything appears to pronounce in its favour, if we compare it with the theory of emission; as this, with all its complex and not very probable auxiliary means, accounts for a limited number only of optical phænomena, whilst it leaves without explanation many others which the theory of undulations not only entirely explains but had even anticipated before they were observed.

The chief objection made to the theory of undulations is that it leaves unexplained *dispersion* and *absorption*. With respect to dispersion, Fresnel has it is true not explained it, but he has shown at least that it does not stand in opposition to the undulatory theory. For he has proved that the velocity with which light is transmitted through an elastic medium, can only be independent of the length of the waves when the opposite spheres of action of the molecules of the elastic medium are so small that we may consider them as null in comparison with the length of the waves. If this is not the case, the velocity of transmission for the shorter waves of light must be smaller than for the longer waves, and this therefore the theory of undulations must take for granted, according to the observations made on the dispersion of light.

I shall now proceed to the special object of this paper, The Absorption of Light. Brewster, who more than any other person has followed up with attention the phænomena connected with the absorption of light,

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* Translated by Mr. W. Francis.

and has enlarged our knowledge in this as well as in the other branches of optics, gives (in the paper in which he describes the curious discovery that certain coloured gases possess the property of absorbing a countless multitude of species of light, while they freely transmit others lying between these) a collection of those phænomena of absorption which he regards as contrary to the theory of undulations. Among other remarks he makes the following: "That the undulatory theory is defective as a *physical* representation of the phænomena of light, has been admitted by the more candid of its supporters; and this defect, insofar as it relates to the dispersive power of bodies, has been stated by Sir John Herschel as a '*most formidable objection*.' That there are other objections to it, as a physical theory, I shall now proceed to show; and I shall leave it to the candour of the reader to determine, whether they are more or less formidable than that which has been stated*." All these new objections of Brewster against the theory of undulations are derived from the phænomena of absorption.

Airy†, in his remarks upon this paper of Brewstr, has certainly fully acknowledged that the undulatory theory had hitherto given no explanation of the phænomena of absorption; but he has on the other hand compared the two rival theories in other respects, with so much knowledge and clearness that I think it impossible after perusal of this collection of facts to hesitate for a moment which to prefer. I am however obliged to controvert what Airy has intimated in relation to the absorption of light, if indeed my own view of this subject be correct. He says that he did not think that absorption could be considered as an essential part of the theory of light. "It is," says he, "a sort of extraneous interruption, which either leaves the ordinary laws in full vigour, or wholly destroys, not the laws, but that which is the subject of the laws." Granting this, I do not see how the conclusion may be drawn from it, that the theory of light need not include the absorption. If we presuppose in bodies certain properties by means of which they act in a disturbing manner on the phænomena of light, we must also on the other hand presuppose in light a property through which its phænomena *would allow themselves to be disturbed* by the bodies, and we must then necessarily explain this last property by the theory of light. I have pursued with attention the interesting phænomenon described by Brewster; but far from drawing the same consequence as he has done, I think I find in it only a complete confirmation of the theory of light.

When I saw for the first time the spectrum of a light which had traversed iodic or bromic gas, whose regularity leaves no doubt that all the absorptions (nearly one hundred) do not proceed from one and the same cause, I was convinced that the whole was a phænomenon of inter-

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* Phil. Mag. and Annals, vol. ii. p. 360. † Ibid. p. 419.

ference, although I could not at that time explain satisfactorily to myself the manner in which it originated.

I will now attempt to show that as soon as we assign to matter a very simple property which is no way in opposition to the idea we otherwise conceive of matter, all the phænomena which we include in the class of *phænomena of absorption* become mere corollaries of the general principle of interference.

Sir John Herschel has lately shown, in a memoir in many respects interesting and instructive, *On the absorption of light by coloured media viewed in connection with the undulatory theory* *, the possibility of considering the phænomena of absorption as originating in interference. But he has thus traced each individual absorption back to a different cause, by which he is obliged to suppose in the absorbing bodies as many different causes as there are absorptions in the spectrum. If we could conceive of about two thousand separately acting causes in one and the same body, as would be the case for example in nitric acid gas, it would still be difficult to form to ourselves a correct idea of the reason of the great regularity which we must presuppose for the hundred causes in the iodic or bromic gas. Moreover he is of opinion that we must relinquish every notion of a regular functional gradation of this phænomenon, upon observation of its quantity and apparent irregularity. He states further, that "if the phænomena were at all reducible to analytical expression, this must be of a singular and complex nature, and must at all events involve a great number of arbitrary constants dependent on the relation of the medium to light, as well as trascendents of a high and intricate order."

I will endeavour to prove, on the contrary, that they may be all reduced to one, or at least to a very limited number of causes, and that they may be all comprehended in one very simple analytical expression, which contains very few constants, and those dependent on the nature of the absorbing medium. The little knowledge we possess of the internal constitution of matter does not permit us to predict what effect it exercises upon a traversing wave of light. If however we may imagine it to be composed of particles which are kept by certain forces at a determinate distance from one another, we may also imagine that these particles are capable of offering a resistance to the traversing wave of light, and consequently of partially reflecting it.

The light thus reflected, which proceeds in a direction contrary to the one it originally had, must be now in like manner reflected in the original direction, in order to experience again a partial reflection in the contrary one, and so on *ad infinitum*. Thus arises an endless series of systems of waves of light, each of which possesses a feebler intensity than the one which had immediately preceded it, and which has

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* Phil. Mag. and Annals, vol. iii. p. 401.

been, in comparison with this, diminished by one portion equal to the double distance between the reflecting surfaces.

If now we confine ourselves to the consideration of the two first systems, it is clear that their results must depend on the relation between the length of the wave of light and the amount of retardation which has taken place in the one system in relation to the other; so that when this amount equals $\frac{1}{2}$, $\frac{3}{2}$, $\frac{5}{2}$, $\frac{7}{2}$... of the length of the waves of light, the intensity of the results must be equal to the difference between the intensity of both the systems; and when it is equal to an entire multiple of the length of the wave of light, the intensity of the results must be equal to the sum of the intensities of the systems. If we now admit that waves of light, of all lengths, from the limit α (the longest) to that of β (the shortest), traverse a medium which causes a delay φ in one part of this light, it is evident that the intensity of all kinds of light the half length of whose wave φ amounts to $\frac{\phi}{3}$, $\frac{\phi}{5}$, $\frac{\phi}{7}$, $\frac{\phi}{9}$, $\frac{\phi}{11}$,... $\frac{\phi}{2m-1}$, $\frac{\phi}{2m+1}$ etc. must be a minimum; that, on the contrary, those kinds of light the half length of whose wave is $\frac{\phi}{2}$, $\frac{\phi}{4}$, $\frac{\phi}{6}$, $\frac{\phi}{8}$, ... $\frac{\phi}{2m}$, $\frac{\phi}{2m+2}$ etc., must attain their maximum of intensity. When these species of light are separated by means of a prism, each one whose intensity is a minimum must appear as absorbed in relation to the others situated between them, and the whole spectrum must be analogous to that which a light which has traversed iodic or bromic gas presents.

Before I enter further into the comparison between the spectra which, according to the theory, must originate in consequence of such a simple retardation, and those which, as experience shows, are produced by absorbing media, I will try to determine what the consequences are of the hypothesis just laid down, namely that of an indefinite continued reflection. If a represents the original intensity of the light, and r the part which is lost at every reflection, the intensities of each of the originating systems of waves of light attain the value shown in Plate VI. fig. 9.* As for the rest, the figure requires no other explication than that I determine the reflecting surfaces with the lines $A\,B$ and $C\,D$, and call b the distance between these surfaces.

Fresnel has shown, in his excellent paper on the diffraction of light†, that the velocity of undulation u which a particle of æther receives

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* Plate VI. will be given, with the rest of this Memoir, in Part IV.

† Poggendorff's *Annalen der Physik und Chemie*, vol. xxx. p. 100.

through a system of waves of light after a lapse of the time t, whose intensity is a and whose length of undulation is λ, is expressed by the equation

$$u = a \sin 2\pi \left(t - \frac{x}{\lambda}\right),$$

in which x stands for the distance of the particles from the centre of vibration.

Let us use this formula to determine the velocities of undulation u, $u_1, u_2, u_3, \ldots$, which the particle of æther acquires through the system of waves of light, whose intensities are: $(1-r)^2 a$, $r^2(1-r)^2 a$, $r^4(1-r)^2 a$, $r^6(1-r)^2 a \ldots.$, thus we have:

$$u = (1-r)^2 a \,.\, \sin 2\pi \left(t - \frac{x}{\lambda}\right)$$

$$= (1-r)^2 a \,.\, \sin 2\pi \left(t - \frac{x}{\lambda}\right)$$

$$u_1 = r^2 (1-r)^2 a \,.\, \sin 2\pi \left(t - \frac{x + 2b}{\lambda}\right)$$

$$= r^2 (1-r)^2 a \left[\sin 2x \left(t - \frac{x}{\lambda}\right) \cos \frac{2\pi 2b}{\lambda}\right.$$

$$\left. - \cos 2\pi \left(t - \frac{x}{\lambda}\right) \sin \frac{2\pi 2b}{\lambda}\right]$$

$$u_2 = r^4 (1-r)^2 a \,.\, \sin 2\pi \left(t - \frac{x + 4b}{\lambda}\right)$$

$$= r^4 (1-r)^2 a \left[\sin 2\pi \left(t - \frac{x}{\lambda}\right) \cos 2 \,.\, \frac{2\pi 2b}{\lambda}\right.$$

$$\left. - \cos 2\pi \left(t - \frac{x}{\lambda}\right) \sin 2 \,.\, \frac{2\pi 2b}{\lambda}\right]$$

$$u_3 = r^6 (1-r)^2 a \,.\, \sin 2\pi \left(t - \frac{x + 6b}{\lambda}\right)$$

$$= r^6 (1-r)^2 a \left[\sin 2\pi \left(t - \frac{x}{\lambda}\right) \cos 3 \,.\, \frac{2\pi 2b}{\lambda}\right.$$

$$\left. - \cos 2\pi \left(t - \frac{x}{\lambda}\right) \sin 3 \,.\, \frac{2\pi 2b}{\lambda}\right]$$

. . . .

$$u_n = r^{2n} (1-r)^2 a \,.\, \sin 2\pi \left(t - \frac{x + 2nb}{\lambda}\right)$$

$$= r^{2n}(1-r)^2 a \left[\sin 2\pi \left(t \frac{x}{\lambda} \right) \cos n \,.\, \frac{2\pi 2b}{\lambda} \right.$$

$$\left. - \cos 2\pi \left(t - \frac{x}{\lambda} \right) \sin n \,.\, \frac{2\pi 2b}{\lambda} \right].$$

The velocity of undulation U, which the particle of æther acquires through the action of the collective systems of waves of light, must now be:

$$= u + u_1 + u_2 + u_3 + u_4 + \ldots\ldots u_n$$

and therefore

$$U = a(1-r)^2 \left\{ \sin 2\pi \left(t - \frac{x}{\lambda} \right) \left[1 + r^2 \cos 2\pi \frac{2b}{\lambda} \right. \right.$$

$$\left. + r^4 \cos 2 \,.\, 2\pi \frac{2b}{\lambda} + \ldots r^{2n} \cos n \,.\, 2\pi \frac{2b}{\lambda} \right]$$

$$- \cos 2\pi \left(t - \frac{x}{\lambda} \right) \left[r^2 \sin 2\pi \frac{2b}{\lambda} \right.$$

$$\left. \left. + r^4 \sin 2 \,.\, 2\pi \frac{2b}{\lambda} + \ldots r^{2n} \sin n \,.\, 2\pi \frac{2b}{\lambda} \right] \right..$$

If we now assume

$$\left. \begin{aligned} a(1-r)^2 \Big[1 + r^2 \cos 2\pi \frac{2b}{\lambda} + r^4 \cos 2 \,.\, 2\pi \frac{2b}{\lambda} \\ + \ldots r^{2n} \cos n \,.\, 2\pi \frac{2b}{\lambda} \Big] = A \,.\, \cos i \end{aligned} \right\} \ldots\ldots (1)$$

$$\left. \begin{aligned} a(1-r)^2 \Big[r^2 \sin 2\pi \frac{2b}{\lambda} + r^4 \sin 2 \,.\, 2\pi \frac{2b}{\lambda} \\ + \ldots r^{2n} \sin n \,.\, 2\pi \frac{2b}{\lambda} \Big] = A \,.\, \sin i \end{aligned} \right\} \ldots\ldots (2)$$

we shall have

$$U = A \,.\, \sin \left[2\pi \left(t - \frac{x}{\lambda} \right) - i \right] \ldots\ldots (3)$$

The system of waves of light thence resulting will be quite of the same nature as the original, only that it has a different intensity and a different position.

(To be continued.)

SCIENTIFIC MEMOIRS.

VOL. I.—PART IV.

ARTICLE XXV. continued.

Attempt to explain the Absorption of Light according to the Undulatory Theory; by BARON FABIAN VON WREDE.

From Poggendorff's *Annalen*, vol. xxxiii. Nos. 23, 24 and 25.

In order to find its intensity A, I multiply the equation (2) by $\sqrt{-1}$ and add it to the equation (1); I then obtain

$$A\,(\cos i + \sqrt{-1}\,.\sin i)$$

$$= (1-r)^2\,a\left\{1 + r^2\left(\cos 2\pi\frac{2b}{\lambda} + \sqrt{-1}\,.\sin 2\pi\frac{2b}{\lambda}\right)\right.$$

$$+ r^4\left(\cos 2\,.\,2\pi\frac{2b}{\lambda} + \sqrt{-1}\,.\sin 2\,.\,2\pi\frac{2b}{\lambda}\right)$$

$$\left.\ldots + r^{2n}\left(\cos n\,.\,2\pi\frac{2b}{\lambda} + \sqrt{-1}\,.\sin n.2\,\pi\frac{2b}{\lambda}\right)\right\};$$

or, since

$$\cos mz + \sqrt{-1}\,.\sin mz = (\cos z + \sqrt{-1}\,.\,\sin z)^m$$

$$A\,(\cos i + \sqrt{-1}\,.\sin i)$$

$$= (1-r)^2 a\left\{1 + r^2\left(\cos 2\pi\frac{2b}{\lambda} + \sqrt{-1}\,.\sin 2\pi\frac{2b}{\lambda}\right)\right.$$

$$+ r^4\left(\cos 2\pi\frac{2b}{\lambda} + \sqrt{-1}\,.\sin 2\pi\frac{2b}{\lambda}\right)^2 +$$

$$\ldots + r^{2n}\left(\cos 2\pi\frac{2b}{\lambda} + \sqrt{1}\,.\sin 2\pi\frac{2b}{\lambda}\right)^n.$$

This is a geometrical series, by the summation of which we obtain

$$A\,(\cos i + \sqrt{-1}\,.\sin i)$$
$$= (1-r)^2 a \left\{ \frac{1 - r^{2(n+1)} \left(\cos 2\pi \frac{2b}{\lambda} + \sqrt{-1}\,.\sin 2\pi \frac{2b}{\lambda}\right)}{1 - r^2 \left(\cos 2\pi \frac{2b}{\lambda} + \sqrt{-1}\,.\sin 2\pi \frac{2b}{\lambda}\right)} \right\} \quad (4)$$

If in this expression, in which r is naturally less than 1, we suppose n to be infinitely great, we shall have

$$A\,(\cos i + \sqrt{-1}\,.\sin i)$$
$$= (1-r)^2 a \left\{ \frac{1}{1 - r^2 \left(\cos 2\pi \frac{2b}{\lambda} + \sqrt{-1}\,.\sin 2\pi \frac{2b}{\lambda}\right)} \right\}.$$

By separating in this expression the real from the imaginary magnitudes we obtain

$$A = \frac{(1-r)^2 a}{\cos i \left(1 - r^2 \cos 2\pi \frac{2b}{\lambda}\right) + r^2 \sin i \sin 2\pi \frac{2b}{\lambda}} \quad \ldots\ldots \; (5)$$

and

$$\sin i \left(1 - r^2 .\cos 2\pi \frac{2b}{\lambda}\right) + r^2 \cos i\,.\sin 2\pi \frac{2b}{\lambda} = 0.$$

From the last expression we obtain

$$\sin i = \frac{r^2 \sin 2\pi \frac{2b}{\lambda}}{\sqrt{1 - 2r^2 \cos 2\pi \frac{2b}{\lambda} + r^4}}$$

and

$$\cos i = \frac{1 - r^2 \cos 2\pi \frac{2b}{\lambda}}{\sqrt{1 - 2r^2 \cos 2\pi \frac{2b}{\lambda} + r^4}}.$$

If we substitute this value of $\sin i$ and $\cos i$ in the formula (5) we have after reduction

$$A = \frac{(1-r)^2 a}{\sqrt{1 - 2r^2 \cos 2\pi \frac{2b}{\lambda} + r^4}} \quad \ldots\ldots\ldots (6) \ldots *$$

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* Considering the *partial* reflection of a surface as a *total* reflection of all the light in contact with the particles of the body, it is evident that, the form of the particles being neglected, and the reflected part being called as before $r\,a$, i. e. $(1-r)\,a$, that part continuing in the original direction, the whole quantity $r\,a$ cannot return in a contrary direction, but that a part of the same must be reflected in different directions. To be convinced that such a change in the presupposed hypothesis does not materially alter the results deduced from it, we have only to suppose that the part of $r\,a$ which is reflected in a contrary direction is called $r'\,a$, as it is then evident that the intensities of the system of waves of

When this expression, which represents the intensity of the resulting wave of light, is differentiated in relation to $\frac{2b}{\lambda}$, it is clear that A becomes a maximum or minimum, if $\sin 2\pi \frac{2b}{\lambda} = 0$; that is, A becomes a maximum when $\frac{2b}{\lambda}$ is $= 0, 1, 2, 3, 4$, etc., and it becomes a minimum when $\frac{2b}{\lambda} = \frac{1}{2}, \frac{3}{2}, \frac{5}{2}, \frac{7}{2}, \frac{9}{2}$, &c.

The result of an indefinite quantity of wave-systems of light becomes also a maximum or minimum, under precisely the same circumstances as the result of only two such systems. To make apparent the hypothesis which I have advanced, I have constructed in fig. 1, Plate VI., the equation (6) in such manner that the values of the intensity A, which represent the different values of $\frac{\lambda}{2b}$, are taken as ordinates, and the logarithms $\frac{\lambda}{2b}$ as abscissæ. As the difference of the logarithms of two numbers depends on the relation of those numbers, and not on their absolute magnitude, the difference between two points of the axis of the abscissæ, which represent two lengths of undulation, standing in a given relation to one another, must be independent of the representative substituted value of $\frac{\lambda}{2b}$, and consequently must be of equal magnitude along the whole curve.

In order to examine the phænomena of absorption which are exhibited in a spectrum whose extreme lengths of undulation (red and violet) are to one another as 1·58 : 1, I described a spectrum (fig. 2) whose length, log. 1·58, and whose divisions, red, yellow, green, &c., take in the lengths

$$\log. \frac{\text{outermost red}}{\text{limit between red and yellow}} \quad \log. \frac{\text{limit between red and yellow}}{\text{limit between yellow and green}}, \text{ \&c.}$$

If I now at first suppose the distance b between the reflecting surfaces to be very small, for example equal to $\frac{1}{40}$ of the length of the wave of the red light, the value $\frac{\lambda}{2b}$ which represents that of the red light

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light originating in this case become $a\,(1-r)^2$, $a\,(1-r)^2\,r'^2$, $a\,(1-r)^2\,r'^4$; $a\,(1-r)^2\,r'^6$, &c., and the final results are

$$A = \frac{a\,(1-r)^2}{\sqrt{1 - 2\,r'^2 \cos 2\,\pi \frac{2b}{\lambda} + r'^4}},$$

which according to this differs from the one before obtained only in this particular, that the magnitude r of the denominator is changed into r'. It thence follows that all the conclusions which may be drawn from one of the formulæ, may also be drawn from the other.

will equal 20. I now place fig. 2 on fig. 1, in such a manner that the red end may lie on 20, and see that the whole part of the curve which represents the spectrum lies near the maximum; and from this I conclude that the body which makes $2b = \frac{1}{20}$ of the length of the waves of red light absorbs little light, or is *translucid*, and absorbs all the colours with an almost equal power, that is, appears *colourless*.

Let us now suppose $2b$ somewhat greater, for example $= \frac{1}{4}$ of the length of the wave of red light; fig. 2 must then be so placed that the red end may lie on 4; we then see that the entire spectrum lies nearer to the minimum of the intensity, but that the violet end lies nearest to this minimum; from this we conclude that the body is but *slightly translucid*, and that its colour must fall into the red.

Let us now move fig. 2 a little further; suppose, for example, that $2b$ is equal to half the length of the wave of green light; we find that the whole spectrum lies in the minimum of the intensity; the body must according to that be *nontransparent*, when r is so great that the intensity of the minimum lies within the limit of our range of vision; in the contrary case it must appear black.

If we continue to move fig. 2 still further, that is, to suppose constantly increasing retardations, we obtain spectra in which the maximum of the intensity falls successively on the violet, blue, green, yellow and red, and in which the complementary colours are more or less absorbed. Let us now suppose the magnitude of r, on which the real magnitude of the intensity, but not the condition of the maxima and minima, depends, to be also unequal on the different bodies; we then easily perceive that we can *imagine all degrees of the natural colours of the bodies, as well as their greater or less transparency, as originated in this manner* *.

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* If we reckon the resultants of the reflected rays, the intensities of which (fig. 9) are $r\,a$, $r\,a\,.\,(1-r)^2$, $r\,a\,(1-r)^2\,.\,r^2\ldots$ etc., in the same manner as we reckoned before the resultants of the transmitted rays, we obtain for their intensity

$$A' = r\,a\,.\,\frac{\sqrt{1 + 2\,(1-2\,r)\cos 2\,\pi\,\frac{2\,b}{\lambda} + (1-2\,r)^2}}{\sqrt{1 - 2\,r^2\cos 2\,\pi\,\frac{2\,b}{\lambda} + r^4}} \quad \ldots\ldots \quad (7)$$

If this expression be differentiated in relation to $\frac{2\,b}{\lambda}$, it is evident that A' becomes a maximum when $\frac{2\,b}{\lambda}$ is 0, 1, 2, 3, 4 . . . ., and a minimum when $\frac{2\,b}{\lambda}$ is $\frac{1}{2}$, $\frac{3}{2}$, $\frac{5}{2}$, $\frac{7}{2}$ . . ., *i. e.* under the same circumstances as A. Hence it follows, that what has been said in respect to the transmitted light holds good also for the reflected light; so that the natural colour of bodies is explained in the same manner for reflected light as for transmitted light.

So long as we take $2b$ less than from three to four times the length of the wave of red light, we obtain only one place of the spectrum, or both its ends absorbed; but if we increase b still more, that is if we advance fig. 2 still more forwards, we perceive that more maxima and minima appear in the spectrum, the more indeed the greater $2b$ is taken. If we suppose $2b = 0{\cdot}004$ of an English inch, we obtain about the same number of absorptions as by iodic gas.

I have endeavoured to produce artificially those kinds of retardations which the phænomena of absorption presuppose, and have been so fortunate as to produce in a very simple manner any kind of the phænomena of absorption I chose. The simplest, and, as I have found, the easiest, manner of performing this experiment successfully is the following: Bend a piece of a thin plate of mica so that it forms the surface of a perpendicular cylinder; then place at some distance a lighted candle at the same elevation. The flame which is reflected towards the eye from the cylindrical surface must now appear as a slender vertical line. This light is reflected partly from the front surface of the mica, partly, once or more than once, from its hind surface; the retardation of the last part is to that of the first in proportion to a distance whose magnitude depends on the thickness of the mica. If the thickness of the mica is at all considerable in proportion to the length of the wave of light, that is about 0·001 inch and more, the reflected light appears quite uncoloured. But if we divide this light into colours by means of a prism, and observe the spectrum through a telescope, it appears, from the most external red to the most external violet, filled with stripes, which are quite black, and more numerous the thicker the plate of mica is. After having shown how we can explain a great number of phænomena of absorption by the supposition of a single retarding cause, I will endeavour to show how we may explain all the rest by a further supposition of many other similar retarding causes. If we suppose light of the intensity a, which has been subjected to the action of a retarding medium, and through that brought down to the intensity

$$\frac{a\,.\,(1-r)^2}{\sqrt{1-2\,r'^2\cos 2\,\pi\,\dfrac{2b}{\lambda}+r^4}},$$

and subjected anew to a fresh retardation, which of itself would have caused the intensity

$$\frac{a\,(1-r')^2}{\sqrt{1-2\,r'^2\cos 2\,\pi\,\dfrac{2b'}{\lambda}+r'^4}},$$

it is evident that the result A' of both retardations must be

$$\frac{A\,(1-r')^2}{\sqrt{1-2\,r'^2\cos 2\,\pi\,\frac{2b'}{\lambda}+r'^4}}$$

or

$$A' = \frac{a\,(1-r)^2\,(1-r')^2}{L\,.\,M.},$$

where

$$L = \sqrt{1-2\,r^2\cos 2\,\pi\,\frac{2\,b}{\lambda}+r^4}$$

and

$$M = \sqrt{1-2\,r'^2\cos 2\,\pi\,\frac{2b'}{\lambda}+r'^4}.$$

In the same manner the results of the three retarding causes will be

$$A'' = \frac{a\,.\,(1-r)^2\,(1-r')^2\,(1-r'')^2}{L\,.\,M\,.\,N.},$$

where

$$L = \sqrt{\left(1-2\,r^2\cos 2\,\pi\,\frac{2\,b}{\lambda}+r^4\right)}$$

$$M = \sqrt{\left(1-2\,r'^2\cos 2\,\pi\,\frac{2\,b'}{\lambda}+r'^4\right)}$$

$$N = \sqrt{\left(1-2\,r''^2\cos 2\pi\,\frac{2b''}{\lambda}+r''^4\right)}.$$

From these equations it will be evident that in general A′ or A″ must attain a partial maximum or a partial minimum as often as one of the angles $2\,\pi\,\frac{2\,b}{\lambda}$, $2\,\pi\,\frac{b'}{\lambda}$, &c. has completed an entire periphery; that is to say, there must originate in the resulting spectrum as many absorptions as would have originated in the sum of each single spectrum. It is therefore easy to account for that which must arise from two or more retardations, in the manner which I will now show by an example. If we convey light into a vessel into which iodine has been brought, and which is then gradually heated so that the iodic gas may increase slowly in intensity, the phænomena of absorption take place in the manner and order following: as soon as so much iodic gas is disengaged that the vessel receives a slightly reddish tint, we perceive in the blue light, or rather at the limit between the blue and the violet, some slender pale black stripes. The dark stripes become blacker the more the intensity of the colour increases, and at the same time we perceive more dark stripes. With increasing intensity of the colour of the iodic gas the light stripes in the blue commence gradually to decrease in strength, till at last a complete absorption of the blue end of the spectrum takes place. In proportion as the entire absorption proceeds toward the red end of the spectrum, new black stripes arise before them, till at last, at a certain intensity of the colour of the iodic gas, the whole spectrum is absorbed, with the exception of a small piece of the red, which is now entirely filled with black stripes. This beautiful phænomenon of absorption is explained with the greatest ease and exactness by supposing two different causes of retardation. We need only presuppose that the one retardation is about equal to the length of the wave of red light, and the other about 150 times greater. The part of the curve of the intensity which represents the first retardation has the form A B (fig. 3); but that part of it which represents the second has the form C D (in the same figure). The results of both may therefore be expressed by a curve of the form of E F. With increasing intensity of the colour of the iodic gas, we must suppose that r and r' are increased, and that this increase can of course have an influence, not on the station of the maxima and minima, but merely on the absolute magnitude of the intensity. The greater r and r' become, the less must also the intensity become. And as we must imagine that the light, in order to be visible, must have a certain intensity, and as we can also express these limits of perception of the light by a line, it is evident that the increase of r and r' must force down the curves of the intensity toward this line.

If we now place fig. 2 on fig. 3, and conceive the line A B in fig. 2 to represent the limit of perceptibility for the eye, we shall render the effect of the increase in the value of r and r' evident by sliding fig. 2 gradually higher on fig. 3. If fig. 2 lies on the line $a\ b$, we see some stripes appear in the blue; if we move it higher towards $a'\ b'$, we see that the blue end is absorbed, and the stripes now make their appearance in the green; if we move it still higher to $a''\ b''$, we find the whole spectrum absorbed, with the exception of a piece of red, which is now filled with black stripes. This is precisely the process in the spectrum of the iodic gas. The phænomena of absorption in bromic gas are explained just in the same manner. In order to explain the spectra arising from the light which has traversed nitric acid gas, or euchlorine, we must suppose several causes of retardation. We must not be astonished at this difference, as the two last gases belong to the compound, while the two first belong to those which we consider as simple. It appears to me quite natural to assume that the elementary constituent parts of a compound body may each of itself cause different retardations; and if we consider nitric acid gas as a compound of nitric acid and nitrogen, instead of considering it as a binary compound of azote and oxygen, we then easily conceive how very possible it is that a great number of retarding causes may be contained therein, each of which arises in the same manner as in the single gases.

Without endeavouring to explain the presupposed causes of retardation, or rather the form which we must suppose matter to possess in order to produce them, I cannot but remark *en passant* a circumstance which has excited my attention, and which perhaps deserves consideration.

Most of the gases retain, when they are in any manner brought into another aggregate condition, very nearly the same colour. The retarding cause to which we ascribe the colour of the body must therefore be almost independent of the aggregate condition. The other retarding cause, on the contrary, suffers a certain change when that condition is changed, because the spectrum of the light which has traversed a solid or liquid body does not possess the black stripes which it would have if the body had been gaseous. In this fact we have a certain reason for referring the first to the particles of the body, and the latter to their distance from one another, because we properly think these to be changeable. A reflection in the *interior* of a particle, or a kind of propagation of light through it, we are not able to imagine, inasmuch as we consider it as an elementary particle. Here then we have an increased probability of the truth of the supposition that bodies consist of such groups of elementary particles as Ampère * has supposed in order to explain the propagation of caloric; an hypothesis which Herschel also in other respects thinks probable †. Be this as it may, we must not be thought too bold when we suggest that by observations on the absorption of light we may find a new way opened to us of viewing the constitution of matter which may perhaps lead to results that could be attained in no other way.

The other facts stated by Brewster, which, as he thinks, remain incapable of explanation according to the undulatory theory, are, on the above supposition, all exceedingly easy to be explained.

In a spectrum of light which has traversed oxalate of chromium and potash, all the colours are absorbed with the exception of the red, which contains black stripes. Brewster mentions as a consequence of this, *that this body permits æther to undulate freely to a red ray whose index of refraction, in flint-glass, is* 1·6272, *and also to another red ray whose index is* 1·6274; *while it is absolutely opake, or its æther will not undulate at all, to a red ray of intermediate refrangibility whose index is* 1·6273.

Set forth under this form, the fact must surely appear as a paradox. It is, however, easily explained if we suppose two interruptions, one of nearly the same magnitude as the length of the wave of the red light, and the other greater, for instance ten times as great. In consequence of the first retardation, the curve of the intensity obtains the form of A B (fig. 4), and, through the second, the form C D (in the same figure): the resultant of both must possess the form of E F. If now we consi-

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* Poggendorff's *Annalen*, vol. xxvi. p. 161.

† *Ibid.*, vol. xxxi. p. 255.

der G H as the limit of perceptibility for the eye, we obtain a spectrum exactly like that above described.

The phænomena which appear in the spectrum of coloured flames stand undoubtedly in connection with the present object, and may be explained in the same manner as the phænomena of absorption. But besides the presupposition of retardations, we must here still make another, namely, that certain flames can only produce light of a certain length of wave, or at least that the produced light is inclosed in certain limits, which lie closer to one another than the red and violet. Various phænomena in the spectra of coloured flames arise only from this cause. This, for example, is the case with the bright orange-coloured stripe which is formed in the spectrum of a common candle light. If we consider the light of a candle, we find it to consist of several divisions, differing from one another. The inner and lighting flame contains, as is well known, heated particles, which undergo a real combustion then only when they arrive at the outermost border, or where they come into contact with the air. The outer flame is therefore of quite a different consistence from the inner; it has also quite a different appearance; it lights feebly, and possesses a faint orange colour. The broadest part of the flame has, on the contrary, a blue colour, and resembles in every respect that produced by a slow combustion of coals. As this flame originates at the point where even the wick comes into contact with the air, I consider it quite probable that it arises from a slower combustion of it.

If we place a convex lens between the flame and the opening through which we allow the light to fall on the prism, so as to produce a magnified image of flame on the prism, we are then able to bring to the opening any part of the image of the flame by sliding the lens, and in that manner to examine any particular part of it. Suppose we then move the image so that only its outermost border lies on the aperture, and consequently only the light of the most external flame can pass through it, we find that the spectrum contains nothing else than a part of the orange-coloured stripe. If we change the form or breadth of the aperture, we find that the orange-coloured stripe undergoes just the same change, so that it always remains a complete copy of the aperture. If we slide the image of the flame so that the inner lighting part arrives at the aperture, we obtain a complete spectrum; and the nearer the middle part of the flame comes to the aperture the greater is the brightness the spectrum acquires, while the orange-coloured stripe decreases more and more. Hence I conclude that the inner flame produces light of all possible lengths of wave; the outer flame, on the contrary, only light of a single length of wave, that is, of a completely homogeneous light. If we view a flame of light through a prism, without letting the light pass through a minute aperture, we naturally obtain an irregular spectrum, which contains all colours. But in this spectrum we find a perfectly distinct orange-coloured image of the whole flame of light produced from the homogeneous light in the outer flame. If we move the image of the light so that only the inferior blue part may fall on the aperture, we find that the spectrum contains only violet, blue, and green light; but at the same time we find three quite apparent and regularly placed maxima, to explain which we must presuppose a retardation of about 10 or 12 lengths of the waves of light.

One of the most peculiar of this kind of spectra is, without doubt, that from the light of burning spirits of wine, in which chloride of copper has been dissolved. This spectrum is filled with bright stripes, which are so arranged that they always appear in pairs with a black stripe between them, while those belonging to the different pairs are separated by a broader stripe, as it is represented by K L, fig. 5. In order to explain this phænomenon we only need suppose two retardations, one twice as great as the other, and having such a position that the maxima of the smaller one fall on the minima of the greater one. In the first the curve of intensity obtains the form A B, in the latter the form C D; the resultants of both must consequently have the form E F. If G H expresses the limit for the power of perception of the eye, it is evident that the spectrum must receive the appearance of K L. On the contrary, let us imagine that the maxima of both components fall together, so that the one has the position A B (fig. 6), and the other that of C D, the resultant then takes the form E F. If now G H represents again the limit of the power of perception of the eye, it is clear that the spectrum produced must contain *black* stripes, appearing in pairs, separated by bright ones, or must appear as K L (fig. 6)*.

In the same manner as we can produce with *one* piece of mica the phænomena of absorption originating from one retardation, just so we can

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* If we put $b' = \frac{1}{2} b$ in the formula (8), it is evident that the maxima or minima corresponding to b' must come to lie where the maxima of b are. The first-mentioned case, or the appearance in pairs of bright stripes, cannot therefore take place when one retardation is *exactly* twice as great as the other; in such a case it is more likely that the stripes occurring in pairs would appear black. It is, however, evident that we only need increase the greater retardation by one single wave-length in order to make *one* maximum which corresponds to the retardation fall on a minimum.

The other maxima and minima do not indeed then completely coincide one with another, but they come evidently nearer to one another the greater the retardations are; and when these are somewhat considerable, the irregularities arising are so small that the eye is no longer able to discover them. We must suppose in the flame of chloride of copper that the smaller retardation amounts to about 40 wave-lengths of the red light, *i.e.* about 60 of the violet; the greater need only be increased by $\frac{1}{80}$ to $\frac{1}{120}$ above the double value of this magnitude.

imitate with *two* pieces of mica the phænomena arising from two retardations, &c. To perform this it is only necessary that the light which is reflected from one plate of mica on the prism must first have been reflected from the one plate of mica on the other plate. According to what has already been proved, a spectrum is then obtained which contains all the absorptions which have been caused by each single retardation.

The following is, as I have found, the easiest method of performing this experiment, which indeed, properly speaking, explains nothing, but which deserves to be mentioned as a beautiful experiment: I generally take a plate of mica, whose surfaces, besides being even and without faults, incline one towards the other, so that the plate is thicker at one border than at the other. Among the plates of mica which I have examined in this respect, I have found one which possesses these properties in a high degree. As I bent this plate into the form of the surface of a vertical cylinder, and placed it so that the light was reflected on the prism from its thick end, I obtained a perfectly regular prism, with about 120 black stripes; but the surface of the cylinder being turned round its axis, so that the reflecting element gradually advanced toward the other end, the distance between the stripes gradually increased, while at the same time their number diminished, till at last from the thin end I received but a few more than 20. In order to bring the surface of the cylinder to any desired position, I fastened it on to a small cylindrical pillar, A B (fig. 7), which was fixed by wax, or any other glutinous substance, to an even support.

In order to produce spectra which contain two series of absorptions, I placed two such cylinder surfaces in the manner shown in A and B (fig. 8). The light from the lamp C, concentrated by means of a great convex lens D, is carried to the first cylinder surface A; from this it is thrown on to the second, B, and from this further on to the prism E. The light of the flame is hindered from falling on the surface B, by means of a sliding screen F; and by another similar screen G that light is received, which otherwise might easily be reflected from the surface A to the prism. By turning both cylinder surfaces round their axes, I can give to the two retardations any desired relation to one another; and in this manner, as just described, I can change *ad infinitum* the phænomena of absorption.

Very small retardations, such for instance as are smaller than a wave-length, cannot, according to this method, be accomplished, because it is almost impossible to give to the mica the necessary degree of thinness. But in order to produce also the phænomena in which small retardations are presupposed, I use coloured fluids which are inclosed in a cylindrical tube, between two plates of glass whose distance from one another can be altered at pleasure. I have completely imitated, with a red absorbing fluid and a plate of mica, not only the spectrum of iodic gas, but also that which arises through absorption in oxalate of chrome and potash. In these experiments the magnitudes r and r' can be varied up to a certain degree, and consequently the breadth of the black stripes can be changed in proportion to the breadth of the bright ones. For this purpose it is only necessary to let the light fall on the surface of the cylinder, under different angles of incidence. For it is evident that the proportion between the light reflected on the first and that reflected on the second must be greater the smaller the angle of incidence is; and consequently that the black stripes must at smaller angles of incidence be less broad than at greater. If we wish to produce absorptions with a small difference between the intensities of the maxima and minima, it is only necessary to let the light pass *through* a plate of mica instead of being reflected on it. In this experiment the proportion can be considerably varied by changing the angle of incidence.

The most complex of all the phænomena of absorption is undoubtedly the solar spectrum, with its numerous irregularly placed stronger or fainter black stripes. If we suppose with Herschel that these stripes arise from absorption in the atmospheres of the sun and earth, it becomes easy to explain them according to the principles already laid down. Although I have not yet made any experiments with a view to ascertain whether and in what measure the different pressures of the gases act on the position of the arising absorptions, yet I think it highly probable that they have a very considerable influence on it. In such a case it is evident that the light in its passage through both the atmospheres, the density of which varies with the distance from their respective bodies, must suffer an indefinite number of unequal retardations, each of which will produce a certain series of maxima and minima. The cause of the number and also of the irregular position of the black stripes is consequently easily to be conceived. But the (at least apparently) vast difference which takes place between the intensities of the maxima and minima requires a particular explanation, which I will now endeavour to make. We have hitherto considered only two reflecting surfaces; it is however evident that, according to the hypothesis with which I have set out, we must suppose a series of such surfaces which will be greater in proportion to the greater thickness of the absorbing medium. If we call, as before, a the original intensity of the light, r the loss in each reflection, and m the number of reflecting surfaces, we easily perceive that the intensities of the system of waves of light must be as we find them given in the following table (fig. 10), in which A_1 B_1, A_2 B_2, A_3 B_3, &c. represent the reflecting surfaces.

When the thickness of the absorbing medium is rather considerable, m must be a great number; and consequently r must be very small, because otherwise no considerable portion of light could traverse all the m surfaces. All members which are multiplied with r^4, or a still higher power of r, must therefore become so small, in comparison with those multiplied with r^2, that they may be neglected. Thus the intensities of the transmitted rays become

$a(1-r)^m$ . . . . for the retardation 0
$a(1-r)^m \,.\, r^2\,(m-1)$ . . . . $2b$
$a(1-r)^m \,.\, r^2\,(m-2)(1-r)^2$ . . . . $4b$
$a(1-r)^m \,.\, r^2\,(m-3)(1-r)^4$ . . . . $6b$
$a(1-r)^m \,.\, r^2\,(m-4)(1-r)^6$ . . . . $8b$
. . . .
$a(1-r)^m \,.\, r^2(m-n)\,(1-r)^{2(n-1)}$ . . . . $2nb$,

consequently we have

$$u = a(1-r)^m \sin 2\pi\left(t-\frac{x}{\lambda}\right)$$

$$u_1 = a(1-r)^m r^2(m-1)\left[\sin 2\pi\left(t-\frac{x}{\lambda}\right)\cos 2\pi\frac{2b}{\lambda} - \cos 2\pi\left(t-\frac{x}{\lambda}\right)\sin 2\pi\frac{2b}{\lambda}\right]$$

$$u_2 = a(1-r)^m r^2(m-2)\left[\sin 2\pi\left(t-\frac{x}{\lambda}\right)\cos 2\,.\,2\pi\frac{2b}{\lambda} - \cos 2\pi\left(t-\frac{x}{\lambda}\right)\sin 2\,.\,2\pi\frac{2b}{\lambda}\right](1-r)^2$$

. . . .

$$u_n = a(1-r)^m r^2(m-n)\left[\sin 2\pi\left(t-\frac{x}{\lambda}\right)\cos n\,.\,2\pi\frac{2b}{\lambda} - \cos 2\pi\left(t-\frac{x}{\lambda}\right)\sin n\,.\,2\pi\frac{2b}{\lambda}\right](1-r)^{2(n-1)}.$$

If we reckon from this the resultants of all the retarded systems of waves of light, *i. e.* all those just mentioned, with the exception of the first, U′, or the velocity which it represents, becomes

$$\begin{aligned}U' = a(1-r)^m r^2 \Bigg\{ &\sin 2\pi\left(t-\frac{x}{\lambda}\right)\Bigg[(m-1)\cos 2\pi\frac{2b}{\lambda} \\ &+ (m-2)(1-r)^2\cos 2\,.\,2\pi\frac{2b}{\lambda} \\ &+ \,.\,.\,(m-n)(1-r)^{2(n-1)}\cos n\,.\,2\pi\frac{2b}{\lambda}\Bigg] \\ &- \cos 2\pi\left(t-\frac{x}{\lambda}\right)\Bigg[(m-1)\sin 2\pi\frac{2b}{\lambda}\end{aligned}$$

$$+ (m-2)(1-r)^2 \sin 2 \,.\, 2\pi \frac{2b}{\lambda}$$

$$\left. + \,.\,.\, (m-n)(1-r)^{2(n-1)} \sin n \,.\, 2\pi \frac{2b}{\lambda} \Big] \right\}.$$

If we now put the coefficients of $\sin 2\pi \left(t - \frac{x}{\lambda}\right)$ equal to $A' \cos i$, and the coefficients of $\cos 2\pi \left(t - \frac{x}{\lambda}\right)$ equal to $A' \sin i$, we obtain

$$U' = A' \left[\sin 2\pi \left(t - \frac{x}{\lambda}\right) \cos i - \cos 2\pi \left(t - \frac{x}{\lambda}\right) \sin i \right]$$

$$= A' \sin \left[2\pi \left(t - \frac{x}{\lambda}\right) - i \right].$$

Whence it follows that A' becomes the intensity of the resulting system of waves of light.

If we multiply $A' \sin i$ with $\sqrt{-1}$, and add it to $A' \cos i$, and put for shortness

$$(1-r)^2 \left(\cos 2\pi \frac{2b}{\lambda} + \sqrt{-1} \,.\, \sin 2\pi \frac{2b}{\lambda}\right)$$

equal to p, remembering that

$$\cos m z + \sqrt{-1} \,.\, \sin m z = (\cos z + \sqrt{-1} \,.\, \sin z)^m,$$

we obtain

$$A' (\cos i + \sqrt{-1} \,.\, \sin i)$$

$$= a \,.\, (1-r)^m \, r^2 \left(\cos 2\pi \frac{2b}{\lambda} + \sqrt{-1} \,.\, \sin 2\pi \frac{2b}{\lambda}\right) \times$$

$$\times \left((m-1) + (m-2)p + (m-3)p^2 + \ldots . (m-n)\, p^{n-1}\right) .. (9)$$

If we call $\left((m-1) + (m-2)p + (m-3)p^2 + \ldots\right)$ for the sake of shortness S, we have

$$S = \left. \begin{array}{r} (m-1)\left(1 + p + p^2 + p^3 + p^4 + \;.\;.\; + p^{n-1}\right) \\ - \left(p + p^2 + p^3 + p^4 + \;.\;.\; + p^{n-1}\right) \\ - \left(p^2 + p^3 + p^4 + \;.\;.\;.\;.\; + p^{n-1}\right) \\ - \left(p^3 + p^4 + \;.\;.\;.\;.\;.\;.\; + p^{n-1}\right) \\ \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \\ p^{n-1} \end{array} \right\}$$

or

$$S = \begin{cases} \quad\frac{m-1}{1-p}\left(1-p^n\right) \\ -\frac{1}{1-p}\left(p-p^n\right) \\ -\frac{1}{1-p}\left(p^2-p^n\right) \\ -\frac{1}{1-p}\left(p^3-p^n\right) \\ \;\cdot\;\;\cdot\;\;\cdot\;\;\cdot\;\;\cdot\;\;\cdot\;\;\cdot \\ -\frac{1}{1-p}\left(p^{n-1}-p^n\right) \end{cases}$$

or

$$S = \frac{1}{1-p}\Big[(m-1)-(m-1)\,p^n - \left(p+p^2+p^3+\ldots+p^{n\ \ 1}\right) + (n-1)\,p^n\Big]$$

$$= \frac{1}{1-p}\left[(m-1)-(m-n)\,p^n - \frac{p-p^n}{1-p}\right].$$

It is now evident that n, or the number of the rays which have traversed after the second reflection, must be equal to $m-1$, or amount to *one* less than the number of the reflecting surfaces. Bearing this in mind we have

$$S = \frac{(m-1)-(m-2)\,p+p^m}{(1-p)^2}.$$

Although we dare not here suppose m to be, properly, indefinitely great, it must however be so great that we may consider p^m in comparison with $(m-2)\,p$ as evanescent, and $m-1$, as well as $m-2$, equal to m. By this we have

$$S = \frac{m}{1-p}.$$

If we now put this value of S in the formula (9), and instead of p its value, and moreover represent $2\pi\frac{2b}{\lambda}$ for shortness' sake by q, we have

$$A'(\cos i + \sqrt{-1}\,.\sin i)$$
$$= \frac{a\,(1-r)^m\,m\,r^2\,(\cos q + \sqrt{-1}\,.\sin q)}{1-(1-r)^2\,(\cos q + \sqrt{-1}\,.\sin q)}.$$

By separating the real magnitudes from the imaginary, we obtain

$$\sin i = \frac{\sin q}{\sqrt{1 - 2(1-r)^2 \cos q + (1-r)^4}}$$

and

$$\cos i = \frac{\cos q - (1-r)^2}{\sqrt{1 - 2(1-r)^2 \cos q + (1-r)^4}},$$

as well as

$$A' = \frac{a \,.\, m r^2 (1-r)^m}{\sqrt{1 - 2(1-r)^2 \cos q + (1-r)^4}}.$$

If we designate the velocity U, which represents the resultants of all the transmitted rays, we have $U = u + U'$, or

$$U = \left[a(1-r)^m + A' \cos i\right] \sin 2\pi \left(t - \frac{x}{\lambda}\right)$$
$$- A' \sin i \cos 2\pi \left(t - \frac{x}{\lambda}\right).$$

If we reduce this expression to the form

$$U = A \sin \left[2\pi \left(t - \frac{x}{\lambda}\right) - \beta\right],$$

and A, which must then express the intensity of the whole resultant, be determined in the common manner, we have

$$A = \sqrt{A'^2 + 2 A' a (1-r)^m \cos i + (1-r)^{2m} r^2},$$

or, by substituting the value already found of A′ and cos i,

$$A = a(1-r)^m \frac{\sqrt{1 + 2\left(m r^2 - (1-r)^2\right) \cos q + \left[m r^2 - (1-r)^2\right]^2}}{\sqrt{1 - 2(1-r)^2 \cos q + (1-r)^4}} \quad (10)$$

If we differentiate this expression in relation to q, it is clear that A becomes a maximum or minimum as often as $\sin q = 0$, *i.e.* A becomes a maximum when $\frac{2b}{\lambda}$ equals 0, 1, 2, 3, 4 . . . ., &c., and a minimum when $\frac{2b}{\lambda}$ equals $\frac{1}{2}$, $\frac{3}{2}$, $\frac{5}{2}$, $\frac{7}{2}$, $\frac{9}{2}$ . . . &c., *i.e.* under quite the same circumstances as if only two of the presupposed reflecting surfaces were present. Hence it follows, that

$$A;\ \text{maximum} = a(1-r)^m \frac{\left(1 + m r^2 - (1-r)^2\right)}{1 - (1-r)^2}$$

$$= a\,(1-r)^m\left(1+\frac{m\,r^2}{1-(1-r)^2}\right)$$

and

$$\text{A; minimum} = a\,(1-r)^m\,\frac{\left(1-m\,r^2+(1-r)^2\right)}{1+(1-r)^2}$$

$$= a\,(1-r)^m\left(1-\frac{m\,r^2}{1+(1-r)^2}\right).$$

If we now compare the intensities in the maximum and minimum with one another, we have

$$\frac{\text{A; max.}}{\text{A; min.}} = \frac{2\,r+m\,r^2}{1+(1-r)^2-m\,r^2}\cdot\frac{1+(1-r)^2}{2\,r}.$$

Bearing now in mind that r must be an extremely small magnitude the above formula is reduced to

$$\frac{\text{A; max.}}{\text{A; min.}} = 1+\frac{m\,r}{2}.$$

From this then we see that the proportion between the intensities in the maximum and in the minimum must become greater the greater m is, i. e. the denser the absorbing medium is. Hence we obtain a reason for the considerable difference between the maxima and minima of the solar spectrum, when the atmospheres of the sun and earth are the absorbing media. There are, however, many circumstances by which our estimate of the relative intensities in the maxima and minima are rendered very uncertain. The flames of light, which suffice to light our chamber in the evening, become by day, when placed in the light of the sun, almost imperceptible. In the same manner, when we step out of a lighted chamber on a dark evening, the darkness seems to us so deep that we can scarcely recognise any one of the surrounding objects; after a few minutes we perceive them quite well. The heavenly bodies, which by night shine so brightly but by day are quite imperceptible, present a second example of the same kind. Moreover we know that the eye itself changes according to the greater or lesser intensity of the light: the contraction and expansion of the pupils are very likely not the only change which the eye undergoes in this respect; for, indeed, I consider it also probable that even the pellicle of the eye possesses the property of greater or less irritability. For this reason it is easy to conceive that the difference between the intensities in the maximum and minimum may possibly appear to us very great without being so in reality, and that the intensities in the black stripes may be very considerable, although the proximity of the brighter stripe makes it imperceptible to our eye.

I have already made the preparations necessary in order to prove by experiment the identity of the phænomena of absorption and those which must result from the hypotheses assumed by me for their explanation. The formulæ which are required for such an experiment I will now analyse. I have already proved that when light of all wave-lengths traverses a medium which causes a retardation c, all species of light whose half wave-length is

$$c, \frac{c}{3}, \frac{c}{5}, \frac{c}{7}, \frac{c}{9}, \ldots \frac{c}{2m-1}, \frac{c}{2m+1} + \ldots$$

become minima. Now in order to derive from this a formula for the minima which must arise, in consequence of the retardation c, in a spectrum whose external limits are α (the greatest) and β (the smallest), I designate this number by s, and suppose that $\frac{1}{2}\alpha < \frac{c}{2m-1}$, but $\frac{1}{2}\alpha > \frac{c}{2m+1}$, further $\frac{1}{2}\beta < \frac{c}{2(m+s)-1}$, but $\frac{1}{2}\beta > \frac{c}{2(m+s)+1}$. Hence we have $2m - 1 < \frac{2c}{\alpha}$ and $2m + 1 > \frac{2c}{\alpha}$ or $m < \frac{c}{\alpha} + \frac{1}{2}$ and $> \frac{c}{\alpha} - \frac{1}{2}$, and consequently $m =$ the entire number in $\frac{c}{\alpha} + \frac{1}{2}$. In the same manner we have $m + s < \frac{c}{\beta} + \frac{1}{2}$ and $> \frac{c}{\beta} - \frac{1}{2}$, consequently $s =$ entire numb. in $\left(\frac{c}{\beta} + \frac{1}{2}\right)$ — entire numb. in $\left(\frac{c}{\alpha} - \frac{1}{2}\right)$ (11)

If on the contrary we assume the phænomenon of absorption as known, and search for the magnitude of the retardation which causes it, we must first determine in one manner or the other the wave-lengths of the species of light which represent any two minima. If I call these α' and β', and the number of the intermediate absorptions $s - 1$ (i. e. s designates the ordinal number of the minimum whose length of undulation is β', reckoned from that whose length of undulation is α'), and suppose

$$\frac{c}{2m' - 1} = \frac{1}{2}\alpha' \text{ and } \frac{c}{2(m' + s) - 1} = \frac{1}{2}\beta',$$

we have

$$c = \alpha'\left(m' - \frac{1}{2}\right) = \beta'\left((m' + s) - \frac{1}{2}\right)$$

thence

$$m' = \frac{\beta' s}{\alpha' - \beta'} + \frac{1}{2},$$

and consequently

$$c = \frac{\alpha' \beta' s}{\alpha' - \beta'} \quad \ldots \ldots \ldots \quad (12)$$

When c is given it is easy to determine the difference δ between the lengths of undulation for two minima lying close to one another, namely γ and $\gamma - \delta$.

If we then suppose $\frac{c}{2m'-1} = \frac{1}{2}\gamma$ and $\frac{c}{2m'+1} = \frac{1}{2}(\gamma - \delta)$, we have $m' = \frac{c}{\gamma} + \frac{1}{2}$ and $\delta = \frac{\gamma^2}{c+\gamma}$ . . . . (13)

For another length of undulation γ' we obtain in the same manner

$$\delta' = \frac{\gamma'^2}{c+\gamma'},$$

and consequently

$$\delta : \delta' = \frac{\gamma^2}{c+\gamma} : \frac{\gamma'^2}{c+\gamma'};$$

if c is rather considerable, $\frac{1}{c+\gamma}$ becomes very nearly equal to $\frac{1}{c+\gamma'}$, and we then have also very nearly $\delta : \delta' = \gamma^2 : \gamma'^2$ . . . . (14) By means of which formula the comparison of the observed with the calculated phænomena may be effected.

The locality in which I have hitherto performed my experiments has not allowed me to make an exact calculation of the fixed lines present in the solar spectrum, although this is the surest way to determine the relation between the refrangibility and the wave-length, because we can then make use of the exact calculations of Frauenhofer. I have therefore only been able to decide as to the colour corresponding with the length of the undulation by the eye. The calculations then which I have hitherto made can only be considered as approximations; I shall not therefore produce them here. Notwithstanding, they have completely convinced me that the phænomena of absorption and those which must follow from the hypothesis laid down by me are identical. An example of this may, however, be worthy of mention, although the calculation must only be considered as an approximation. In the spectrum of iodic gas, fifteen stripes occupied 9′ 30″ from the orange-coloured to the red; ten stripes between the yellow and green occupied 5′ 30″; I therefore suppose that the distance between two neighbouring stripes, at the limit between the red and orange, amounts to 38″, and at the limit between the yellow and green to 31″. If we now insert in the formula (14), instead of γ and γ', the two corresponding wave-lengths (0·0000246 and 0·0000219 English inch, according to Herschel's table), we have $\delta : \delta' = 38 : 30{\cdot}6$.

As to the rest, it is self-evident that by the undulation-length of a colour I mean the length of undulation *in the absorbing medium*. Since in the examples mentioned the lengths of undulation were taken as they were calculated in the air, and moreover the squares of the lengths of undulation were compared with the distance between two consecutive minima instead of with the differences between the representing lengths of undulation, which would presuppose that the refrangibility of a colour would be proportional to its length of undulation, this can be considered as proving nothing more than that the reasoning and the experiment coincide in showing that the absorptions in the green colour lie closer to one another than in the red.

I must finally remark that, although I have considered the retardations only as arising from reflection between the particles, I also easily conceive the possibility that this idea may be incorrect, and that all the retardations may arise from causes as yet quite unknown to us. But I think I have shown by what I have stated that the phænomena of absorption may be reduced to a simple mathematical principle; and that these phænomena, as belonging properly to the absorbing bodies, point to certain determined magnitudes, which can be given in an absolute measure, and the closer examination of which, whatsoever their cause may be, must always be highly interesting.

Article XXVI.

On the Application of Electro-Magnetism to the Movement of Machines; by M. H. Jacobi, *Doctor of Science, and Professor at the University of Dorpat.*

[Published at Potsdam, 1835.]

PREFACE.

The great discovery of M. Oersted, which has so much extended the limits of physical science, promises to open a new career to practical mechanics. The motive powers which have hitherto served for the movement of machines are not, properly speaking, forces; they are only masses animated by forces. These masses are made to act upon the point of application of a machine, and they consequently can only give it a velocity conformable to their own moving principle. But magnetism enables us to employ immediately a force; the point of application is the force itself. We thus perceive a considerable active force produced without any external influence. The interest of such a phænomenon is increased extremely by the simplicity of the apparatus and by the facility of conceiving its mode of action. But on examining it more closely, we find that the forces which are active in producing the movement present a great complication of circumstances. The study of the phænomena of electricity and magnetism is still in its infancy; and we are not surprised that every day makes us acquainted with new phænomena at once striking and unsuspected. The views which I had conceived of these forces have in part been confirmed during the course of my researches, and they have in part been shaken and even completely changed; as soon, however, as I was obliged to abandon one view, another presented itself which led to the disclosure of fruitful results. For example, the remarkable reaction which prevents the movement from becoming accelerated to infinity has become a new source of power; the exact knowledge of the galvanic action leads to a minimum of the expense attending its maintenance. I have restricted myself in my researches to such experiments as touch immediately upon the object itself; and from the number of these, I shall only publish those which have given results, or at least lead us to hope for them: I have suppressed as much as possible all purely theoretical considerations. As to the practical application, it appears to me decided by my experiments; to go further will be only to augment an effect, with which, laying aside sanguine expectations, we may already be content. It is no unusual thing to have electro-magnets which lift 2000lbs.; mine carried only from 30 to 40 at most: *nevertheless, these feeble magnets furnished me with a mechanical action equal to half the force of a man. To maintain this action, during eight hours, scarcely half a pound of zinc is required, everything being properly arranged.*

I have not yet been able to construct a larger apparatus, and I therefore wished to make as much use as I could of the one I possessed, since it was capable of showing completely the nature of the active forces. My experiments may be easily repeated; all depending upon carefully attending to the construction of the commutator, and likewise that of the galvanic apparatus. Those who are acquainted with electro-magnetic phænomena will easily be able to make the necessary arrangements, and to give the accurate proportion to the several parts. The object of this memoir will be attained if it inspire an interest for a subject which merits it.

Königsberg, August 21, 1835.

MEMOIR.

1.

In November 1834 I had the honour to lay before the Academy of Sciences of Paris a note upon a new electro-magnetic apparatus. That note was read at the meeting of December 1st; and an abstract of it was printed in the Institute, No. 82, of December 3rd, to which I refer. Since that time MM. Botto and dal Negro have claimed the priority of the invention, the former in the Institute (No. 110) of June 17th. The competition in which I find myself engaged with such distinguished men serves only to confirm my conviction of the importance of this new motive power. A discussion as to priority is only of historical interest. It is not astonishing that persons, who had scarcely any communication with each other, should have devoted themselves almost at the same time to the study of the same object. But we ought not to conceal from ourselves that, after the grand discovery of M. Oersted and the experiments of Mr. Sturgeon, who, it seems to me, first gave a great magnetic intensity to soft iron by means of an electric current, and viewing the instantaneous manner in which this magnetism may be destroyed or reversed, by merely changing the direction of the current,—it was not difficult to conceive the possibility that some motion or some mechanical operation might be produced by the electro-magnetic excitation of soft iron. In short we must award the palm to M. Oersted; whilst we who follow him shall have the merit of having known how to apply this new power to practical purposes and the wants of life: and this will be reserved for him who shall best have understood the mechanical and physical principles of this motive power.

2.

In May 1834 I constructed the first magnetic apparatus with a primitive continuous circular motion. It is true that, like M. dal Negro, (with whose labours I regret that I am not better acquainted,) I had several years ago conceived the idea of applying this power to mechanics: but I could not at first divest myself of the idea of making this application by means of an advancing and receding motion, produced by the attractive and repulsive power of magnetic bars,—a motion which, by known means, might have been changed into a continuous circular one. It seemed to me that an apparatus of this kind would have only the merit of an amusing toy, which might find a place in the cabinets of men of science, but would be entirely inapplicable on a large scale with any advantage.

For considering the general equation of active forces applied to the movement of machines

$$\Sigma \int_0^a M ds - \Sigma \int_0^{a'} P ds' = \Sigma m v_{\prime}^2 - \Sigma m v_0^2$$

the magnetic action, during the amplitude a, and represented by $\int_0^a M d s$, could not be perfectly exhausted by the action $\Sigma \int_0^{a'} P d s'$, unless the active force gained during the movement becomes zero, or $\Sigma m v_{\prime}^2 - \Sigma m v_0^2 = 0$. Now the magnetic attraction is a function of the space, the form of which we do not sufficiently know, this function being affected by the nature of the distribution of the magnetism in the body, of whatever form. The law of this distribution is scarcely established with regard to bars of steel of a regular form, magnetized to saturation and deprived of consecutive points. With regard to bars of soft iron of considerable dimensions, magnetized by an electro-conductive helix, we have analogies only, but no experiments. But however this may be, we know well that this function must be expressed by a very convergent series, so that the magnetic attraction will be in an inverse proportion to the square or to the cube of the distance, or, stopping at the three first members, will perhaps be composed of them. The magnetic points then approach each other with an accelerated motion; the active forces increase, and reach their maximum at the instant when the contact is completed: but this force ought then to be destroyed. It will destroy itself by the fixed points of the machine, and by the vibration of the system: but this will be in an unprofitable manner. There will be a complete loss of the active force obtained $\Sigma m v_{\prime}^2 - \Sigma m v_0^2$. We know the ill effects of shocks in the movement of machines, but there is here another inconvenience which is not simply mechanical. The soft iron, by these repeated shocks and vibrations, gradually acquires at the surface of contact the nature of steel; there will be a considerable permanent magnetism, and the transient magnetic force, which alone produces the movement, will be weakened in proportion. A number of experiments which I have made upon the magnetic force of a bar of soft iron bent into a horseshoe (of which I shall speak hereafter) has shown me the great disadvantages of oft-repeated shocks, proceeding from the sudden contact of the armature. But, if we stop at the mechanical principles of magnetism, it may be objected that the active force gained will not be absolutely lost for the purposes of utility; that in part the elasticity of the iron will itself reproduce it; that another portion may be regained by springs properly applied, or by other mechanical methods which may be invented. We leave the appreciation of all these factitious means and of these superadditions to those who are in the habit of constructing machines; they well know their insufficiency, the great loss of working power, and how rapidly all the systems are destroyed, unless the greatest care be paid to the preservation of the active forces. But we must seek the means of this preservation in the nature of the forces themselves. The history of the steam-engine teaches us that its improvement commences with Watt's ingenious idea of stopping the escape of the steam before the piston had accomplished its stroke, and of causing the steam afterwards to act by its own expansion. Watt understood the subject: all he did was to give to the function $P = \phi(s)$, which expresses the action of the steam, such a form as $\int_0^a P\,ds = \int_0^{a'} P'\,ds'$, and thus the active force gained becomes zero, all the prejudicial and destructive vibrations in the machines previously constructed cease, and the power of the motive force is converted for the most part into useful action. I must here cite the valuable researches of M. Poncélet on the construction of hydraulic wheels,—a work founded upon a profound comprehension of the same principles.

These considerations, at once clear and simple, have induced me to reject entirely every apparatus in which magnetism is applied to produce immediately an oscillating motion; these constructions being, as we have seen, as inadmissible as they are impracticable of execution on a large scale.

3.

In the note which I had the honour of laying before the Academy of Sciences of Paris I stated that, in accordance with all experiments, magnetism is a power acting like universal gravitation, solely in some function of space. The integral $\frac{\int_0^a M\,ds}{a}$ comparable with the known number g, represents the mean action furnished by the attraction of two points, and is not at all affected by their relative velocity. The inversion of the poles being effected instantaneously, we should thus have a velocity infinitely accelerated. Now a system moving round an axis, and capable of a continuous circular motion, is that which is alone susceptible of such a velocity. It cannot become uniform, unless some resisting element, or some other action depending on the velocity, is introduced into the system. Putting aside the application to practical use which has to be made of such an apparatus, the obstacles to be overcome, inseparable from the system, consist only in the friction of the pivots in the sockets, and in the resistance of the air. As to the former, repeated experiments have proved that the friction is independent of the rapidity of rotation, at least within the limits of experiment; this resistance, therefore, can in no way contribute to render the accelerated motion uniform. It is in fact the resistance of the air which will act to produce this effect. Although it might be reduced at pleasure, principally by giving a suitable form to the rotatory system, it would not be entirely annihilated. But it will be allowed, that we should have reason to be well satisfied with the mechanical effect of magnetism, if this were the only cause which tended to reduce the accelerated movement to a uniform movement. The limits of such a uniform velocity must be very distant. I do not speak of the great simplicity of a magnetic machine with a continuous circular motion, of the advantages of construction which are gained by being able to transform with ease this motion to any other which the working machine may require. These considerations had strongly impressed my mind, even whilst the means of execution were still unknown to me, but I always kept in sight the practical application, and the object appeared too important for me to exhaust my powers in the construction of see-saw toys, which might claim the honour of being placed in the rank with the electric chime relatively to their effect, and still more relatively to the tinkling with which they are accompanied.

4.

Fig. 1. of the annexed plate represents the magnetic apparatus of eight bars, arranged symmetrically upon a disc moveable round the axis *A*, and of eight fixed bars similarly arranged upon a fixed platform. The arrangement of the bars admits of the greatest variety, provided it be exactly symmetrical, and that it allow the poles to approach each other as nearly as possible. To prevent the action being too oblique,—since the centre of magnetic gravity is probably at some distance from the extremity, as in the ordinary magnetic bars,—it is preferable to make this arrangement so that the axes of the cylindrical bars shall be situated rectangularly, and not parallel, as in the figure. It must be further observed, that there will be some difficulty in forging bars of considerable dimensions into the horseshoe form, so that the axes of the branches be situated exactly

at the same distance, and that the branches themselves be exactly cylindrical. Filing them into shape will perhaps have the disadvantage of hardening too much the surface of the iron, and of rendering it less apt to receive and to part with the magnetism. The form proposed offers a further inconvenience, in the application of the copper wire helices, which have to be previously bent on another cylinder of the same dimension. These helices ought very nearly to touch the bars, which should be covered with silk on account of the insulation which is necessary. In future an arrangement similar to the one in fig. 2 will be preferred, in which f are the fixed bars, and m the bars moveable around the axis a. We shall have the advantage of being able to employ cylindrical bars of soft iron, such as may be had of all dimensions in the shops. It will only be necessary to cut them into equal pieces, and the helices may be strongly wound round the bars by means of the lathe.

5.

As the magnetic attraction decreases rapidly as the distance increases, the integral $\int_0^a M ds$ will always be such a function of the amplitude a, that its value will not greatly differ from a constant, a being rather considerable. Admitting, for an instant, that the magnetic attraction is in an inverse ratio to the squares of the distances, we shall have $\int_0^a M ds = \int_0^a \frac{f\,ds}{d^2+s^2} = \frac{f}{d} \text{ arc } tg\,\frac{a}{d}$, d being the distance of the magnetic centres when the bars are placed the nearest possible; thus d being very small with respect to a, $\int_0^a M ds = \frac{f\pi}{4d}$ or $= \frac{nf\pi}{4d}$, n representing the number of bars. We shall then have for the action of the motor, during one entire revolution, the expression $\frac{n^2 f\pi}{4d}$. The radius of the circle upon which the bars are arranged does not enter into this expression; and for a stronger reason it will not enter into any of the other expressions, if the attraction still decreases more rapidly than the inverse ratio to the square of the distance. Thus the size of the circle for the same number of bars scarcely adds anything to the action of the motor.

I conceived that the system of bars, which in my apparatus are fixed, might also be rendered moveable. The rotation of the two systems will then be in a contrary direction and have the same velocity, the masses being equal. These two motions might be combined by means of conical wheels, in order to produce the motion of a second axis of rotation intended for the work. The action of the motor, during the amplitude a, that is to say from one meeting of the poles to the other, would be as above $\frac{nf\pi}{4d}$, but the poles meeting each other $2n$ times in one revolution, we should have $\frac{2n^2 f\pi}{4d}$, or double the previous action. We might even construct wheel-work in such a manner that the velocities of the systems should be in the ratio of $m:1$, and that the poles should meet $(m+1)\,n$ times, during one revolution. The action would then be $(m+1)\,n^2\frac{f\pi}{4d}$, and this increase would be gained by purely geometrical means. This is a simple deduction from the fact that velocity does not enter into magnetic attraction. I have not as yet availed myself of this advantage in the construction of magnetic apparatus, since there are some remarkable circumstances, as we shall see hereafter, not sufficiently cleared up, and which may give rise to considerable modifications.

6.

The inversion of the poles is an object of the greatest importance. This inversion should take place instantaneously, and precisely at the place where the poles are situated opposite to one another. The mechanism intended to produce this operation should be put in motion by the apparatus itself, but no element should be introduced which is dependent in a geometrical manner upon the rotatory movement of the system. The velocity of the motion, however great it may be, should not at all affect this operation. The well-known *bascule*, an ingenious invention of M. Ampère, which is so advantageously employed in electro-magnetic experiments, cannot be employed in the magnetic apparatus with a continuous circular motion; for the number of inversions, in a given time, cannot be considerable without requiring extraordinary means; and even these means will not guarantee the certain result of an advancing and receding movement, repeated as frequently as may be necessary. I shall not recount here all the attempts I have made, both numerous and expensive, to arrive at the important result of an inversion of the poles, exact and precise, divested at once of every element depending on the velocity. But it is necessary to say that the greatest difficulties arose by employing mercury, as is usual in electro-magnetic experiments to form and to break metallic contact. In the liquid state the adhesion of the mercury to the metallic body plunged into it and afterwards withdrawn, varies with the rapidity of the motion and with the purity of the mercury. Frequently—I may say always—the inversion takes place too soon or too late, and thus gives rise to an attraction or repulsion, in a contrary direction to the rotation. Moreover it is very difficult to preserve the mercury pure when in contact with other metals; and even the purest mercury is disposed to oxidize easily under the influence of the electric sparks. These sparks are produced, under favourable circumstances, on establishing metallic contact, and always on breaking it. The result is, that the surface of the mercury is soon covered with a coating of oxide, which either entirely prevents the metallic contact, or at least weakens it. In employing amalgamated surfaces this effect is produced still more rapidly. Besides I have by incontestible proofs arrived at the conviction, that the simple contact of metals with a clean surface is quite sufficient to conduct the electric current, even of the weakest tension. The contact by means of mercury adds nothing to the energy of this current. It is erroneous to judge of this energy by the brilliancy of the spark, proceeding only from the combustion of the mercury. I have thought right to mention these circumstances, though apparently trifling. In a motor, from which we look to obtain an infinitely accelerated motion, the smallest details should not be disregarded; the most trivial are ultimately of importance.

7.

Fig. 3. represents the commutator, adapted to the magnetic apparatus, so as to produce the inversion of the poles: *a*, *b*, *c*, *d* are four discs of copper fixed upon the axis of rotation *e e*. The discs *a*, *b* and *c*, *d* are united by copper tubes *f*, *f*, entirely insulated from the axis by the interposition of a tube *g*, of varnished wood or any other insulating substance.

The periphery of each disc is divided into eight exactly equal parts, of which four *h* are cut into sectors and filled afterwards by pieces of ebony, forming with the metal an accurate and smooth surface. The discs are arranged upon the axis of rotation, so that the sectors of wood and of metal alternately correspond, as represented by the shaded parts of the figure. *ZZ*, *CC* are bars of copper, formed as levers, very moveable in their supports: they are intended to conduct the current. The arm of the longest lever forms at its extremity an edge, which rests on the periphery of the corresponding disc. The other arm is bent and plunged into a little jar filled with mercury, *k*. The jars *k k* and *k' k'* are united by plates of copper, as represented in figure 1. The action of this commutator will easily be understood. The levers are always in contact with the discs, and are alternately so with the metallic and insulating parts. By their mobility in their supports they yield to the slightest inequality of the surface, and the friction they occasion is very trifling. The helices which surround the moveable bars are united so as to form a continuous wire, the ends of which *l*, *m* are soldered respectively to the systems of the discs *a*, *b* and *c*, *d*. The other helices, wound round the fixed bars, are also united, and the ends *n* and *o* immersed, the one in a jar of mercury *p*, attached to the voltaic apparatus, and the other in the jar *k* of the commutator. Thus all the sixteen helices form only one connecting wire, through the medium of the commutator. The voltaic apparatus consists of four troughs of copper, in which plates of zinc are immersed, all being united as in a pile. The direction of the current is shown in fig. 3 by little arrows; it is reversed each time the poles meet, provided the commutator be so placed that the edges of the levers shall quit one of the divisions in order to pass to the other. This inversion acts, as is seen, instantaneously, and quite independently of the velocity of rotation. The object is too simple, and sufficiently explained by the figures, to render it necessary to enter more into its details. I may add, further, that this same system of the inversion of the poles is applicable to any number of bars, provided that the sections of the discs are equal to them in number. I have constructed, for magneto-electric experiments, a double commutator of eight discs, with seventy-two divisions. In this apparatus there are also four levers similar to the former, which rest upon the cylinders (*f*) that unite the discs in pairs. The other extremities of these levers are likewise immersed in jars of mercury, intended to receive the ends of a connecting wire, which is to be traversed by the voltaic or magneto-electric currents, sometimes in one direction sometimes in the other. The instrument is put in motion by a handle, which can be easily turned twice in a second, and effect in the same time 144 double inversions. It will be easy to change or completely interrupt the electric current 1000 or more times in a second. The nature of this current or of the magnetism will of course be better understood by decomposing it into a rapid succession of pulsations: I am persuaded, for instance, that we should succeed by this means in charging a Leyden jar, or in effecting any chemical decompositions by the thermo-electric current of a single pair of elements.

8.

The magnetic power is produced and maintained, as is well known, by the action of the voltaic apparatus. By using zinc as a positive metal, copper as a negative metal, and water acidulated with sulphuric acid as the conducting liquid, it is the transformation of the metallic zinc into sulphate of zinc which here constitutes the cost of keeping the apparatus in action. It is a matter of the greatest importance to reduce as much as possible this cost. Let us examine what is the relation between the magnetism of the connecting wire and the action of the voltaic apparatus. Since the discovery of electro-magnetism this object has engaged the attention of distinguished scientific men, but it presents so many difficulties and such a complication of circumstances, that we cannot be surprised that the theories and formulæ which they have endeavoured to deduce from experiments differ considerably.

This is not the place to enter into the criticism of these theories; but it appears to me that the theory established by M. Ohm, in a little work entitled "*Die galvanische Kette, mathematisch bearbeitet von Dr. G. S. Ohm* (1827)," and developed more fully in various memoirs printed in the German Journals, presents so much simplicity, and agrees so well with all the phænomena of the voltaic pile, that I have not hesitated to adopt it, in order to obtain from it a general basis for the arrangement of the different elements of the magnetic apparatus. I may be permitted here to state the fundamental principles of this theory.

1. In a closed voltaic circuit the same quantity of electricity passes across each section which is perpendicular to the direction of the current, whatever be the form or the matter of the different parts of the circuit.

2. Whatever change is made in one part of the circuit, this change affects the entire action of the pile, and is not confined merely to the place where the change takes place.

3. The voltaic action, in whatever manner measured, is in the direct ratio of the electro-motive power, and inversely as the resistances which oppose themselves to the passage of the current, or $A = \frac{E}{R}$.

4. The resistances are composed of—

a) the resistance of the solid conductor or of the connecting wire. For the same substance this resistance is directly as the length of the wire, and inversely as the transversal section or as its thickness.

b) the resistance of the liquid conductor: this is in the direct ratio of the thickness of the liquid stratum which separates the positive and negative plates, and inversely as its transversal section, which coincides generally with the surface of the plates. During the action of the pile this last resistance increases, and at the same time the electro-motive power, or E, is affected by it. This is caused by chemical effects which take place and change by degrees the nature of the liquids, the surface of the metals, and the electric tension. But fixing any state of the pile, the law cited always exists. The difficulty of making electro-magnetic experiments comparable with each other, and the still greater difficulty in obtaining absolute measures, consist principally in the continual change of these elements. Thus in expressing by r the resistance of the connecting wire, we shall have $\frac{r\,l}{d}$ for the resistance of a wire, of a length l, and of a thickness d; $\frac{r'\,l'}{d'}$ will likewise be the resistance of the liquid conductor, the surface and thickness of which are respectively expressed by $d'\,l'$. Therefore the action of the current, or the quantity of electricity passing through the pile, will be $A = \frac{E}{\frac{r\,l}{d} + \frac{r'\,l'}{d'}}$.

5. The electromotive force is in the direct ratio of the number of voltaic pairs united in a pile, and at the same time the resistance r' increases in the same proportion. Having one pile of n' pairs, the force of the current will be expressed by $A = \frac{n'\,E}{\frac{r\,l}{d} + \frac{n'\,r'}{d'}}$

6. If the electric current is divided into several branches, the lengths of which, reduced in an inverse ratio to their diameter, may be expressed by l, l', l'', &c., the total action will be the same as if there were only a single connecting wire whose length is expressed by the equation $\frac{1}{L} = \frac{1}{l} + \frac{1}{l'} + \frac{1}{l''}$, &c. Therefore having n wires of the same length, the total force of the current will be expressed by

$$A = \frac{n' E}{\frac{r\,l}{n\,d} + \frac{r'\,l'\,n'}{d'}} = \frac{n\,n'\,d\,d'\,E}{r\,l\,d' + r'\,l'\,d\,n\,n'}.$$

As we can avail ourselves of the magnetizing power of each unity of length of the connecting wire by coiling it round bars of the same dimension, the total power gained by a connecting wire l will be

$$A = \frac{l\,n\,n'\,d\,d'\,E}{r\,l\,d' + r'\,l'\,d\,n\,n'}.$$

From this formula the limits of the action of the current may be deduced, which cannot be increased by the number or the surface of the voltaic pairs, by the length, the diameter, and the number of the connecting branches. Increasing only the surface of the pairs d', the limit of the total power of the current will be $A = \frac{n\,n'\,d\,E}{r}$; increasing the number n', this limit is $A = \frac{l\,d'\,E}{r'\,l'}$.

Again this limit will be, by increasing the length of the wire l, $A = \frac{n\,n'\,d\,E}{r}$; the thickness of the wire d, $A = \frac{l'\,d'\,E}{r'\,l'}$; the number of the connecting branches n, $A = \frac{l\,d'\,E}{r'\,l'}$.

In general, in order to increase the force of the current to any degree, it is necessary to enlarge the surface of the plates, and at the same time the thickness of the connecting wire or the number of the branches. The increase of the number of the pairs requires that of the length of the connecting wire, in order to attain the same end.

The experiments, as accurate as they are numerous, which M. Fechner has made on this subject, and which he has published in his work "*Maassbestimmungen über die galvanische Kette* (1831)," leave no doubt as to the justness of these laws, which express in a very simple manner all the relations of the different elements which constitute the voltaic pile. These experiments have been made for the most part by employing the method of oscillations, which M. Biot was the first to apply ingeniously to this kind of experiments.

9.

In admitting at first that the chemical effects which take place in the voltaic pile, and which represent the expense attending the magnetic apparatus, are in a direct ratio to the active surfaces, it seemed to me of great importance to establish the relation between the surface of a voltaic pair and the weight capable of being supported by a bar of soft iron submitted to the magnetizing power of the current. A bar of soft iron $1\frac{1}{2}$ inch in diameter by 29 inches in length, weighing $14\frac{1}{2}$lbs., was bent into a horseshoe, so that the centres of the branches were seven inches apart. The bar, covered with silk, was covered by a helix of copper wire of $1\frac{1}{4}$ line thick and 35 feet long. The magnetic power was measured by means of a steelyard, and a weight supported by rollers, in order to slip easily over the arm of the lever. The surface of the soft iron armature was of a somewhat convex form, in order that the extremities of the branches, forming a flat and smooth face, should only be touched in the direction of an edge, the position of which formed a right angle with the direction of the lever. The armature was in contact with the extremities of the branches when the lever was placed horizontally. Upon the latter had been marked a scale, the divisions of which indicated the thirtieth part of the sliding weight, to which was affixed an index: it was easy to estimate the tenths of these divisions. I had taken the necessary precautions to avoid as much as possible the errors of observation arising from the disposition of the apparatus. I shall not enter here into the details of the construction of this rather complicated apparatus, which I intend to give elsewhere, as it may be useful for experiments of this kind. The electromotors which I employed consisted of copper troughs three quarters of an inch wide, and sufficiently large to enable me to immerse in them respectively the plates of zinc of 4, 16, 36, 64, 100, 144 square inches. The contact of these last with the copper was prevented by the interposition of pieces of wood. The conducting liquid, of which I had previously prepared a sufficient quantity to serve for a series of experiments, was acidulated with ten per cent. of concentrated sulphuric acid of the specific gravity of 1·840. The experiments, with the same voltaic pair, were made without interruption; but after each one precautions were taken to cleanse carefully the zinc plates, to wash the trough with water and to renew the liquid, in order to restore the same state of action. But subsequent observations convinced me that the original state is restored more certainly by exposing the plates, and especially the negative one, to a current of air, until it is perfectly dried. It will then be no longer necessary to renew the liquid so frequently, especially when the observation is confined to the primitive state. It must be acknowledged that I subsequently found the copper troughs to be ill adapted for electro-magnetic experiments; concentric cylinders, which may be plunged in the liquid, are much better. These cylinders must be fixed firmly enough to remain at the proper distance, without recurring to the interposition of wood or of any other insulating matter. Much more constant galvanic effects may be obtained if the space occupied by the liquid between the two metals be not too narrow; at all events it ought to exceed half an inch. I have also made experiments with voltaic pairs arranged like the calorimotor of Hare, but there were reasons for rejecting these also. It is a very different thing to make an isolated observation, and to put in requisition the galvanic action for whole hours and days. It is in the latter case that for practical purposes measures are required, the necessity of which had not been before anticipated. It will be also necessary to reject the use of copper as a negative metal; the expense of employing silver, platina, or at least copper well plated with silver, gold, or platina, must be no obstacle. The solution of the copper in the sulphuric acid, however weak it may be, and its reduction into a metallic state, by the secondary effects of the nascent hydrogen, give rise to partial galvanic effects, by which the principal action is much affected, and to avoid which the greatest pains must be taken. In fact the motion of the magnetic apparatus was sometimes suddenly slackened or entirely interrupted, and on examining more closely I found that metallic particles of cementing copper or of iron had been deposited all along the pieces of wood interposed, or upon the bottom of the troughs, and thus formed a partial circuit. I shall speak of zinc hereafter. The following is the table of observations which I have made on the magnetic power of the horseshoe bar of iron above dsecribed.

Surface of Pair.	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.	No. 7.	No. 8.	No. 9.	No. 10.	Force	
											mean.	calculated.
square inches	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
4	41·46	40·31	47·19	43·18	45·47	48·88	46·52	55·21	55·21	54·07	47·75	47·3
16	126·32	123·43	125·21	125·67	128·47	130·16	124·39	120·55	130·16	130·16	126·45	126
36	156·98	216·54	180·04	189·73	184·34	185·30	205·91	211 —	157·38	162·77	185 —	182·3
64	201·72	208·29	195·14	197·33	199·99	198·32	201·25	202·92	197·41	203·83	200·62	216·1
100	266·63	243·21	236·45	235·65	229·93	232·75	232·75	228·79	227·11	227·11	236·05	236·3
144	311·8	221·46	210·17	198·88	210·17	198·88	192·23	......	......	......	......	
144a*	258·56	257·66	254·46	252·12	256·22	253·02	......	......	......	......	255·34	249

The values given in the last column have been calculated according to the formula $A=\frac{(233\cdot6)\,x}{20+x}$, the constants of which were found by the method of the least squares. It is true that there are considerable differences between the observations of the same series, but there was no reason to choose those which agreed the best with each other, and to attribute the differences to an error of observation.

10.

I have read in an extract from the memoir of the Abbé dal Negro inserted in the "*Annali delle Sc.*, 1813 [1833?], Marzo e Aprile,

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* The first series of experiments, which were made with the pair of plates of 144 square inches, presented such different values that no use could be made of them. I have sought in vain for the cause of these anomalies. After a fortnight the experiments were repeated, and gave values but little different. This is proved by the Table.

105—120," that this author established a remarkable law, viz. that the magnetizing power is in the direct ratio of the perimeter of the electromotor, and that the surface has scarcely any effect in increasing this power. I did not delay making some experiments in order to confirm this law, which appeared to me of great importance for the œconomical effects of the magnetic machine. Two plates of zinc and copper, 36 inches long and 7 inches wide, were coiled into a helix, and separated from each other to a distance of one fourth of an inch by small pieces of wood in the manner of the calorimotor; the whole was plunged into water acidulated with ten per cent. of sulphuric acid. The mean weight which the bar was capable of supporting, whilst this pair of 252 square inches was employed, was taken from five experiments and amounted to 297·12 lbs.

From the same piece of copper and zinc I also cut two plates, 96 inches long and half an inch wide. These plates were coiled in the same manner and separated to an equal distance. The mean value of the magnetic force, when this electromotor of 48 square inches was employed, was also drawn from five observations and amounted to 133·79 lbs. By employing a liquid much more acidulated the weight might be increased to 180·49 lbs.

These two experiments cannot be classed with the others, as the circumstances attending them differed. But the perimeter of the first electromotive helix being 86 inches long, and that of the second 193 inches, it does not appear that the law of M. dal Negro is confirmed by these two experiments. There are many empirical formulæ in physics incapable of being carried out to the extremes, but they ought at least to be sufficiently general not to fail on the slightest attempt to extend their limits. Besides, I have taken the pains to calculate the experiments of M. dal Negro from the formula of M. Ohm. The fourth column of the following table indicates the results according to the ascertained formula $A = \frac{41{\cdot}55\,x}{14{\cdot}4 + x}$, in which x represents the surface of the pair of plates.

Surface.	Perimeter.	Force observed.	Force calculated.
6 square inches	14 inches	13·85 kilogr.	12·22 kilogr.
12 - -	16 -	18·2 -	18·89 -
18 - -	18 -	22·8 -	23·08 -
24 - -	20 -	24·6 -	25·97 -
30 - -	22 -	25·8 -	28·07 -
36 - -	24 -	29·6 -	29·68 -
42 - -	26 -	30·3 -	30·94 -
48 - -	28 -	32·8 -	32 -
54 - -	30 -	33 -	32·8 -
60 - -	32 -	35·6 -	33·51 -

The second column of the preceding table, which contains the perimeter of the plates, represents at the same time the forces according to the law of M. dal Negro. That distinguished experimentalist did not make these experiments to verify the theory of M. Ohm; but the beautiful agreement of his observations with that theory sufficiently proves that they were made with great accuracy.

11.

Since 1831 Mr. Faraday has published from time to time experiments made with a view to investigate the nature of electricity and of its various effects. These experiments, both from their extent, the certainty and ingenious sagacity which they manifest, and the abundant results to which they have led, deserve to be ranked with the most eminent labours which have ever been made in physics. By a happy chance, which I cannot over-appreciate, these labours coincide with the efforts which I have made to render available the mechanical action of magnetism.

In observing a voltaic pair of plates of copper, silver or platina, and of common zinc plunged into acidulated water, we notice a great development of hydrogen gas. If the circuit be not closed this gas will be developed only on the surface of the zinc; but if the circuit be completed, there will be also a development of gas on the surface of the copper, or in general on the negative plate. This last quantity of gas is incomparably less than the first, and yet it is from this alone that the magnetic power of the connecting wire proceeds. The gas, abundantly disengaged on the surface of the zinc, does not contribute anything to this effect. On taking a plate of amalgamated zinc, instead of common zinc, or some amalgam of zinc, there will be no development of gas except when the circuit is closed; in breaking it this development ceases, the zinc in this combination not being attacked by the acid, or not being able of itself to decompose the water. It is not easy to explain this extraordinary fact. In such a pair of plates all the hydrogen gas, or its equivalent of zinc, serves to produce an electric current, whose magnetic force, calorifying power, and chemical action, are in a direct ratio to the quantity of disengaged gas or of oxidized zinc; and these different effects may equally serve to measure the quantity of electricity passing through the connecting wire, or even through the apparatus. The definite action of electricity, with regard to the chemical action, to decompose bodies, is incontestably proved by the numerous and ingenious experiments of Mr. Faraday. It will not be long before he will prove the law with regard to other effects; but the conviction of genius gives the right to anticipate experiment, and to announce great laws.

Amalgamated zinc is much more positive than common zinc, and its effects are much more decided. Moreover a voltaic pair of plates of this kind possesses a remarkable constancy, provided there are no secondary effects arising from the precipitation of the negative metal upon the positive plate. It may happen that some particle of the zinc may not be well amalgamated; in that case a direct action of the acid upon the zinc takes place, there is a development of hydrogen in that place, the negative metal accidentally dissolved in this liquid will be reduced in it by the gas, and there will be a partial pile, which will affect the principal action. These partial effects will be propagated by degrees over the whole surface, the positive state of which will then rapidly decrease. This will only take place when the negative metal is soluble in the acid.

12.

I have made many experiments on this subject. A thin plate of zinc of seven inches square, and weighing 848 gr., was amalgamated, in order to form a voltaic pair with a plate of copper of the same size. The liquid was sulphuric acid, of a specific gravity of 1·105. There was no development of gas on the surface of the zinc: the bubbles of air which formed there by degrees rose so slowly, that they might with propriety have been disregarded, even if there had not been reason to believe that they were for the most part the atmospheric air contained in the water. After five hours of action the plate was again weighed, and had lost only 112 gr.; during this time the pair of plates had been twice withdrawn from the acid, and dried for five or six minutes near a stove.

The following is the table of the deviations of the needle which denote the decrease of the energy of the current.

Time.	Deviation.	Time.	Deviation.
$8^h\ 12'$	61°	10^h	60°
$8^h\ 22'$	59°	$10^h\ 30'$	58°
$8^h\ 30'$	58°	11^h	57°
$8^h\ 42'$	57½°	the pile	was dried
$8^h\ 56'$	56½°	$11^h\ 5'$	61°
$9^h\ 10'$	55½°	$11^h\ 30'$	60°
the pile	was dried	12^h	59½°
$9^h\ 16'$	62°	$12^h\ 30'$	58°
$9^h\ 30'$	61°	1^h	57°

The following day the experiments were repeated with the same pair of plates. The decrease of the deviation was not so rapid as before, and the original energy always restorable by drying the plates; once it even increased to 65°. At $10^h\ 50'$ in the evening the deviation was still at 55°. The action must have continued through the night, but the next morning the plate was found broken in pieces. Amalgamated zinc is too fragile to be employed in too thin plates. In order to compare the effects, a plate of common zinc of the same size was combined with a plate of copper and plunged into the same acid. The deviation was at first 55½°, after 43′ it lowered to 12°, and on drying the pair of plates 13° was the highest to which it could be restored. On being subjected to the action of the acid for $1\frac{3}{4}^{h}$, the plate had disappeared, and its insoluble parts only remained.

I have also made experiments upon a liquid amalgam of zinc poured into a porcelain basin covering a surface of 48 square inches; instead of a plate I employed a copper wire of 1¼ lin. diameter, coiled into a flat spiral, in order to let the gas escape more easily. The effects of this combination were very extraordinary; for, without anything being touched, the needle had during fifteen hours' action only receded 11½° from 60°, and remained fixed at 49½°. After breaking the circuit, and exposing the spiral to the air for some time, the deviation was restored to 59°. This experiment was the more striking as the multiplier of the galvanometer consisted only of a single coil of copper wire 1¼ lin. in thickness; for it is known that the decrease of the needle is much more feeble on employing a very long and slender wire.

A plate of gilt copper and an amalgam of zinc, composed of one atom of zinc and one of mercury (*Zn. Hg.*), a composition which is solid enough to be used in plates, gave also very good effects, both as to the constancy of the deviation and to its restoration.

In order to try some other compositions, which, according to Ritter, are still more positive than the amalgam of zinc, I had some plates cast, of an equal size, of lead, tin and zinc, of different alloys of these metals, and of different amalgams. The alloys were composed of atom to atom*, and moreover a plate of each composition was also amalgamated at its surface. The direction of the deviation of the needle of the galvanometer determined the place which each alloy ought to occupy. The liquid in which the plates were plunged was sulphuric acid diluted with four parts of water. I must remark, that the slightest change of the surface frequently affects the place of the metals the electrical relation of which does not differ much. It is chiefly in lead and its alloys that this phænomenon is most strikingly exhibited. Lead freshly polished is very positive in relation to lead exposed to the air for some minutes or steeped in any acid. The following is the result of two series of experiments, which I have made with the greatest care.

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* In the alloys it is usual to combine the metals according to some relative proportion of weight. I united them by atoms, bearing in mind the general law of true chemical compositions.

— *Series* I.

Tin.
Alloy of lead with tin. (*Pl. Sn.*)
Lead.
Tin amalgamated.
Lead amalgamated.
Amalgam of tin. (*Sn. Hg.*)
Alloy of zinc with tin. (*Zn. Sn.*)
Amalgam of lead. (*Pl. Hg.*)
Alloy of zinc with tin and lead. (*Zn. Sn. Pl.*)
Alloy of zinc with lead. (*Zn. Pl.*)
Alloy of tin with lead amalgamated.
Zinc.
Alloy of zinc with tin amalgamated.
Alloy of zinc with lead amalgamated.
Alloy of zinc, tin, and lead amalgamated.
Zinc amalgamated.
Amalgam of zinc. (*Zn. Hg.*)
Amalgam of an alloy of tin and lead. (*Zn. Pl. Hg.*)
Amalgam of an alloy of tin and zinc. (*Sn. Zn. Hg.*)
Amalgam of an alloy of tin, zinc, and lead. (*Sn. Zn. Pl. Hg.*)
Amalgam of an alloy of zinc and lead. (*Zn. Pl. Hg.*)

+

— *Series* II.

Tin.
Lead.
Tin amalgamated.
Amalgam of an alloy of tin with lead.
Alloy of lead with tin.
Lead amalgamated.
Amalgam of tin.
Alloy of zinc with tin.
Amalgam of lead.
Tin with lead amalgamated.
Alloy of zinc, tin, and lead.
Alloy of zinc and lead.
Zinc.
Zinc amalgamated.
Alloy of zinc, tin, and lead amalgamated.
Alloy of zinc and lead amalgamated.
Amalgam of zinc.
Amalgam of an alloy of tin and zinc.
Amalgam of an alloy of tin, zinc, and lead.
Amalgam of an alloy of zinc with lead.
Alloy of zinc with tin amalgamated.

+

We see by the above that the alloys, and principally the amalgams, are always positive with relation to the simple metals. Most of the amalgams, excepting those of tin and lead, may be used in plates. As to the chemical action upon these various compositions, it did not take place in the amalgamated zinc and the amalgam of zinc, any more than in any of the alloys and amalgams of tin and lead; but in all the other compositions of zinc the disengagement of gas was very brisk. In the first series, the amalgam of an alloy of tin and lead occupies a very positive place, but the hope of profiting by this is negatived by the second series. In employing the amalgamated plates or the amalgams of zinc, there occur various circumstances the cause of which I have not yet been able to discover. During the voltaic action particles of amalgam are often detached in the form of flakes, which float on the liquid, and are deposited on the copper or on the negative plates, so that these become by degrees amalgamated. By this the action is considerably weakened, or ceases altogether; for it is very remarkable that copper, silver, or platina, amalgamated on their surface, have scarcely any, or at most an extremely weak power of keeping up an electric current with any other metal. I have often remarked that the first deviation of the needle was very strong, and that at length it returned quickly to its first position of equilibrium, without exhibiting any deviation, whilst the voltaic circuit, composed of zinc and amalgamated copper, remained always closed. It appears to me also remarkable that a wire of copper, platina, or iron can be much more easily amalgamated under the influence of sulphuric acid by mercury containing other metals than by mercury entirely pure. It is desirable that this point should attract the attention of scientific men to make similar experiments with more attention.

Pure zinc has nearly the same qualities as amalgamated zinc or the amalgam of zinc, viz. of being very little acted upon by sulphuric acid. It is only subjected to chemical action when it enters into a voltaic combination. I refer, on this subject, to the important memoir of M. Aug. de la Rive, inserted in the *Bibliothèque Universelle*, vol. xliii. 1830. I have not yet been able to procure any pure zinc to repeat these experiments and to employ it in the magnetic apparatus. In zinc foundries pure zinc may easily be obtained in great quantity by re-distilling it until it is purified of the cadmium and other extraneous metals. Its cost would not be much increased, but hitherto there has not been sufficient inducement to employ pure zinc to risk the expense of the repeated distillation. M. Fengler, manufacturing chemist at Myslowitz in Upper Silesia, has constructed the necessary apparatus for preparing pure zinc in large quantities; he could supply it for nine ecus the quintal, provided a quantity of three quintals were ordered, but unfortunately his foundry has since been burnt down. His process consists in interrupting the distillation when all the cadmium is driven off, in then changing the recipient and again interrupting the process as soon as he suspects that the other foreign metals are volatilized or mechanically drawn away. He repeats these operations as frequently as he thinks necessary. The zinc thus prepared should not be re-cast in iron crucibles.

13.

The rapid decrease of the voltaic effects in the ordinary voltaic piles opposes a great obstacle to the application of electro-magnetism. It may be overcome, partly at least, by an assiduous study of these effects. The motion of my magnetic apparatus was always very rapid at the commencement, but its velocity soon diminished, and ceased entirely after a lapse of time which never exceeded an hour. By employing amalgamated plates of zinc I have succeeded at three different times in making the apparatus work successively during 20, 22, and 24 hours without making any change whatever in the pile. The experiments were always interrupted by some accident, and I think that the action would otherwise have lasted still longer. The disengagement of gas was very inconsiderable, and took place only at the surface of the negative plate. The velocity was always at first from 120 to 122 revolutions in a minute, and decreased about half an hour after to 62 revolutions; a circumstance which is attributable to the commutator, which had not then the present construction. During the rest of the time the motion of the apparatus was remarkably uniform, making from 58 to 62 revolutions in a minute. I must however confess that I have obtained so extraordinary an effect only three times. There were always external circumstances, dependent upon the form of the voltaic apparatus, which counteracted the effect. I might be able to master most of these circumstances by constructing a new apparatus, the manipulation of which will be more convenient and the effect more certain.

14.

We have expressed by $A = \frac{E}{R}$ the magnetic force of each section of a wire traversed by an electric current. This force is measured by the deviation of the needle or by the magnetizing power of the connecting wire. By adopting the law of Faraday we may equally measure this current by the disengagement of the gas, which represents at the same time the cost of maintaining in action a voltaic apparatus. If D be the quantity disengaged, we shall have $D = \frac{E}{R}$. From this it follows that recurring to the formulæ of article 8, the œconomical effect may be expressed by the magnetic power of the whole extent of the connecting wire, divided by the development of the gas. This effect is in no respect changed either by the enlargement of the surface, or by the employment of various branches wound spirally around different bars of the same dimension. But by multiplying the helices, and uniting them to form a continuous wire, the œconomical effect may be increased as much as we please. For the disengagement of the gas, in employing n helices or n units of length, will be expressed by $D = \frac{E}{nr + r'}$; but we may put in action the magnetizing power of the whole extent of the conducting wire, and we shall have for the total force $F = \frac{nE}{nr + r'}$ or $\frac{F}{D} = n$. When the magnetic bars are intended to produce a mechanical motion, the increase of the œconomical effect will reach its limit; since by multiplying the number of the bars, the weight of the apparatus and the friction of the pivots in the sockets will be at the same time increased, so that that effect can only be expressed by $\frac{F}{D} = n - \frac{(nr + r')nf}{E}$. The maximum of the œconomical effect obtainable will depend on the value of f or of the friction. By differentiating the second member with respect to n, we shall have for this maximum $n = \frac{E-fr'}{2fr}$.

The bar which was used in the experiments of article 9 weighed $14\frac{1}{2}$ lbs. Being adapted to any moveable apparatus, the friction it occasions would amount at most to $\frac{1}{2}$ lb. It has been found by experiments: $E = 283{\cdot}6$, $r' = 20$, $r = 1$, and $f = \frac{1}{2}$, thus $n = 273{\cdot}6$; that is to say, there would be the greatest possible advantage in employing about 273 bars wound round with helices of the same size. This number varies with the size of the plates: for a surface m, we have $n = \frac{mE-fr'}{2fmr}$. In short the magnetic power available for practical purposes is $\frac{nE}{nr+r'} - nf = \frac{(E-fr')^2}{(E+fr')2r}$.

15.

In employing a voltaic battery, the œconomical effect will be diminished, unless at the same time the helices united in the same wire be multiplied; for Mr. Faraday has proved by the experiments reported in the articles 990, &c. of the Eighth Series of his Researches, that the same quantity of electricity passes through a voltaic battery of any number of pairs of plates which traverses a single pair of the same size. The quantity of gas disengaged at the surface of each plate of the battery is the same as at the surface of a single pair; this at first sight appears astonishing, and seems to contradict numerous experiments which have been made upon the pile; for every one knows that the quantity of gas disengaged by the decomposing apparatus, and at the same time the deviation of the needle, increase up to a certain point, by multiplying the number of plates.

In considering the formula $F = \frac{n'E}{n'r'+r}$, where n' represents the number of pairs of plates, r' the resistance of each pair, and r that of the connecting wire, or of the body which we wish to decompose, we must suppose that in the experiments of Mr. Faraday (990.) the connecting wire of the battery and of the single pair of plates were so short that its resistance r might be entirely neglected in relation to $n'r'$. We should not have obtained this striking result if we had employed a connecting wire of any considerable length, and still less if we had closed the circuits of the pair of plates and of the battery by any decomposing apparatus. Mr. Faraday has established a very exact distinction between the quantity and intensity of electricity set in motion. The first may be measured in different ways; but it will be difficult to find an exact measure for its intensity, nevertheless this would be very necessary for completing the theory. In admitting the important law of equivalents in galvanic decompositions, it appears that we ought to multiply these equivalents by the number of pairs of plates necessary to effect the decomposition. This would perhaps be the true measure, for after all it is necessary to consume a great number of atoms of zinc, in order to decompose a single atom of any other substance less decomposable. In what relates to the difference between quantity and intensity, caloric offers analogies; and in judging of a quantity of gas, we ought always to know its volume and its density. I must here quote another observation of Mr. Faraday, which is found in the Seventh Series, art. 853. He is speaking of a current which is, he says, "powerful enough to retain a platina wire $\frac{1}{104}$ of an inch in thickness red hot in the air during the whole time" ($3\frac{3}{4}$ minutes); and he adds in a note: "I have not stated the length of wire used, because I find by experiment, as would be expected in theory, that it is indifferent. The same quantity of electricity which, passed in a given time, can heat an inch of platina wire of a certain diameter red hot, can also heat a hundred, a thousand, or any length of the same wire to the same degree, provided the cooling circumstances are the same for every part in both cases," &c. This is quite correct, but we may add that it would be necessary to multiply the number of pairs of plates in the same proportion with the length of the wire to obtain a current of the same quantity.

In short in order to heat a wire of 1000 inches to the same degree to which a wire of a single inch would be heated by a single pair of plates, it is necessary to disengage 1000 quantities of gas, proceeding from the same number of pairs. I have thought it right not to suppress this remark, considering that in the practical employment of the voltaic pile œconomy is requisite.

16.

The following is the table of experiments which I have made upon the deviation of the needle with relation to the quantity of gas developed at the surface of the negative plate of a voltaic pair of plates of silver and amalgamated zinc. The specific gravity of the sulphuric acid was 1·25.

Deviation of the Needle.	Time elapsed in the disengagement of 1 cubic inch of hydrogen.	Deviation of the Needle.	Time elapsed in the disengagement of 1 cubic inch of hydrogen.
42° 45′	50″	26° 30′	189″
41° 30′	57″·5	24° 52′	217″
39° 30′	64″·5	23° 52′	231″
34° 45′	89″	23° 7′	246″
32° 22′	108″·5	21° 30′	290″
29°	144″	20° 15′	312″
27° 30′	167″	20° 7′	330″
27° 15′	166″		

The bubbles of air rising regularly enough to serve as a measure, I have reckoned, in another series of experiments, the time which elapsed in developing 10 bubbles of air. The following is the table:

Deviation of the Needle.	Time elapsed in the disengagement of 10 bubbles of hydrogen.	Deviation of the Needle.	Time elapsed in the disengagement of 10 bubbles of hydrogen.
34° 30′	22″·5	18° 22′	80″
32° 30′	25″	15° 15′	101″
31°	27″·5	14° 30′	124″
22°	56″	14° 20′	126″
21° 37′	57″	14° 10′	129″
21° 22′	59″	13° 20′	147″
19° 37′	67″	13°	160″

It is necessary to remark, that there was also a very feeble development of gas even at the surface of the zinc, which was taken into account. But the quantity of gas measured was, I believe, less than the quantity of gas developed; for there was a secondary action, which was manifested by the blackness of the plate of silver, and which we must attribute to a metallic reduction of the oxides dissolved in the acid. As it is very difficult to translate into forces the deviation of the needle*, these tables will not tend to confirm the law of Mr. Faraday: they only show that the deviation of the needle follows the same course as the development of the gas. I shall repeat the experiments, but reversing the process; that is to say, the development of gas will be taken for the most exact measure of the force of the current, and the value of the degrees of the galvanometer will be deduced from it, either immediately, or by some formula of interpolation of convenient application. The experiments cited are not sufficiently rigorous to form the elements of calculation.

17.

To return to the magnetic machine. We had succeeded in obtaining an inversion of the direction of the current, both instantaneous and exact, by the commutator described above in article 7, the effect of which is not at all affected by the quickness of rotation. We had even succeeded in obtaining, at least for some time, a tolerably constant voltaic apparatus. In short, the means have been discovered of reducing the expense of maintenance to a minimum, by preventing the direct action of the acid upon the zinc, an action which cannot be turned to any use, and which, as is known, greatly surpasses that which serves to produce the voltaic current. Thus the most important difficulties in the practical application of electro-magnetism being overcome, it appeared to me time to examine more closely the nature of the forces which I desired to put in use, and principally to seek for the cause which limits a

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* Becquerel's Treatise on Electricity and Magnetism, vol. ii. p. 20.

speed which we had reason to suppose must be infinitely accelerated. This speed had never surpassed 120—130 revolutions in a minute, on employing a pile of four pairs of plates two feet square. We must not lightly abandon conclusions founded on the nature of things, and those to which I refer are drawn solely from the integral $\int_0^a Mds$, expressing the magnetic attraction and supposed to be independent of the speed. Besides, it rests upon the legitimate supposition that the electro-magnetic excitation of the soft iron operates instantaneously. If this were not the case, my apparatus would have shown that magnetism and electricity ought to be attributed to the motion of material particles, or to oscillations much more perceptible than are those of the propagation of sound. In short no one can deny that it is the nature of a force not to require time to act, and that, if its different effects were not instantly perceptible, it would then be some molecular motion, under the influence of mechanical laws, which takes place.

18.

At the end of my first note I said, that in using thermo-electric piles for the movement of machines, there was reason to fear the magneto-electric currents developed by magnetism in motion. The reaction which thence arises would be almost entirely destroyed in the hydro-electric pile, the liquid conductors offering too much resistance to the passage of these currents. These considerations were founded upon detached experiments. On employing a thermo-electric pile, the deviation of the needle was affected by a magnet which had been placed in a helix forming part of the circuit; this was not the case with a voltaic pair of plates of small dimension. The deviation of the galvanometer, extremely sensitive as it is, was not altered by it. This did not surprise me, since the conducting power of liquids is much below that of metals; but in making experiments on the magnetic force of a bar of soft iron, I have sometimes found considerable differences for which I could not account. I was curious to know if these differences proceeded from the weakening of the electric current produced by a pair of plates with a surface of half a square foot, or from the nature of the iron. I therefore inserted in the circuit a galvanometer at some little distance, that it might not be affected immediately by the magnetism of the bar. I was much astonished to see the needle recede upon my applying the armature, and advance as soon as it was taken off; for it was the first time that I had recognised the double office of the connecting wire, viz. that of conducting the voltaic current, and of representing at the same time a common wire subjected to the influence of a magnet in motion. The helix producing a magnet by the voltaic current is at the same time a magneto-electric helix in which a magnet is inserted. This is the explanation of the problem of the uniform rapidity of the magnetic machine; for, being put in motion by the magnetizing power of a voltaic current, it represents simultaneously an apparatus composed of magnets in motion, and capable of producing a magneto-electric current, in a direction contrary to the voltaic current. The first is closed by the pile itself, which, being composed of a single pair only, does not offer too great a resistance to its passage.

In the connecting wire, formed by the union of the sixteen helices of the apparatus, I interposed a galvanometer; and then, by closing the circuit and preventing the motion of the machine, I observed the deviation of the needle: it amounted to nearly 60°. As soon as the motion of the apparatus commenced the needle began to recede, and continued to do so more and more as the speed became more accelerated. The motion having become uniform, at the rate of 60 revolutions in a minute, the needle became stationary at a deviation of about 47°. The needle always advanced when the motion was stopped or retarded; it receded, on the contrary, when it was mechanically accelerated. It appears that the deviation of the needle of 47° corresponds with the state of equilibrium; for the motion having of itself ceased, the needle did not quit this position. Thus in the different experiments, whether the first deviation of the needle exceeded 60°, or was less, it always became fixed at about 47°. The voltaic current having been weakened by the interposition of different branches, until the first deviation amounted only to 47°, the magnetism was not sufficiently strong to produce the movement of the apparatus. Repeated experiments will be necessary to investigate these interesting phænomena.

19.

I imagined that it would be useful to open two passages or two separate branches to the magneto-electric current; one of which should be the pile, and the other a second connecting metallic wire, so long and so thin as not too much to affect the quantity of electricity passing through the principal connecting wire. (Art. 8., No. 6.) There was reason to suppose that the counter-current would rather follow the metallic wire than the liquid of the pile: but it was not so. During the motion of the apparatus, the needle of the galvanometer being fixed at 47°, and the second circuit having been suddenly established, the needle was not much affected by it. It advanced, it is true, but only 1°·5. Neither did the speed of the apparatus sensibly change. On reducing the length of the second wire it was nearly the same. The passage of the counter-current across the metallic wire was proved, at least in part, by the interposition of a second galvanometer. During the accelerated movement the needle of this latter advanced in proportion as the needle of the former receded. This might have been expected, provided the counter-current in the secondary branch has the same direction as the voltaic current: it is quite conformable to the remark which M. Nobili has added to the end of his first memoir, upon the theory of the electro-dynamic induction. (*Antologia di Firenze*, 1832, No. 42.) The ends of the connecting wire surrounding the bars must be considered as the poles of an electro-motive apparatus: moreover the magnetizing power of this counter-current has been proved by making it pass through a helix bent round a bar of soft iron.

20.

In short all tended to prove that the greatest part of the counter-current might be rendered available by employing two apparatus of the same kind, the connecting wires of which, wound spirally round bars of each system, should terminate at the same pile. The counter-current produced by the movement of one apparatus would serve to strengthen the magnetism of the other, and *vice versâ*: the counter-currents would counterbalance each other to destroy their effects. The experiment could be made on a small scale with the bar above described, the branches of which were encircled with separate helices. Fig. 4. shows the form of the experiment. The two helices were connected by the dotted wire *c b* plunged into the little cups filled with mercury *c*, *b*. They thus formed a single connecting wire, the other ends of which *a*, *d* were united with a pile *C Z*. With my hands dipped in acidulated water I took hold of the connecting wire at the place *e*, *f*, and I broke the circuit at the place *g* or *h*. I felt a violent shock. In other respects the experiment was the same as the beautiful one of Mr. Jenkins related by Mr. Faraday. By interposing the multiplying wire of a galvanometer *m* in the circuit, the needle deviated to 48° by the voltaic current. Then applying the armature, it receded from 48° to 40°. The deviation on removing the armature was unobservable, the latter being too firmly attached. Now the helices were connected with the pile in two branches separated by means of the wires *a b* and *c d*. The wire *c b* was withdrawn. I expected, on breaking the circuit, to find that the magneto-electric current excited in the helix *a c* would be conducted quite entire by the helix *b d*, and *vice versâ*; but I was mistaken: the shock was not much less: the needle nevertheless receded. I was struck by this experiment, but after all I believe I may regard this magneto-electric arrangement as an unclosed voltaic pile, consisting of two elements united in such a manner as to form only a single pair of plates, as is represented in fig. 5. The currents whose direction is opposed with relation to the wires *a b*, *c d*, unite in traversing a connecting wire placed in contact with the points *e f*. If the galvanic excitation is not in perfect equilibrium, being stronger on one side than on the other, there will be a deviation of the needle proportional to the difference of the currents which traverse the wires *a b*, *c d*. This agrees with the experiments which Mr. Faraday has related at the beginning of his Eighth Series, on the subject of decompositions produced by a single pair of voltaic plates. In short what is termed tension is the effect of forces equal and contrary in direction. In mechanics such forces destroy themselves, their sum being zero; but in physics it is different.

With regard to the direction of the magneto-electric current which occasions the shock, it is the same as that of the voltaic current. This was proved by a galvanometer, the multiplying wire of which terminated at the points *e*, *f*. There was a deviation on a part of the voltaic current traversing the secondary branch *e*, *f*. On applying the armature, the needle of this galvanometer advanced, at the same time that the needle *m* receded. The contrary effect might be observed on removing the armature by the blows of a hammer.

21.

The following are some further experiments relative to this subject. The extremities of the bar were surrounded with a thin plate of copper, fig. 6, in the circuit of which was placed a galvanometer. On applying the armature, the needle was unaffected by it; but after having wound the ends of the multiplying wire around the points *e*, *f*, and the circuit being thus closed, a considerable deviation took place.

An analogous result is shown in the following experiment. On plunging two thin plates of copper, held firmly in the hands, in the cups *a*, *b*, or *c*, *d*, of the bar, fig. 4, there was no shock when the circuit was broken by the separation of the wires *a b* or *c d*; for the human body formed part of a circuit, in which equal excitations took place on two opposite sides. The thin plates being plunged into the cups *c* and *b*, a violent action took place at the instant of disjunction.

I formed a thermo-electric circuit of bismuth and antimony, in which was interposed a galvanometer: after having heated the two solderings to the same degree, there was no deviation of the needle; but the multiplying wire having been placed so as to form an intermediary branch, and the solderings being on opposite sides, there was a considerable deviation. This would not have taken place if the circuit of bismuth and antimony had been in its normal state, for then it would have had to conduct the greatest part of the thermo-electric current, provided that the multiplying wire was sufficiently long and thin to intercept only an extremely feeble part of it.

It seems to me that there are circumstances which cause metals to lose their conducting power, and that these same circumstances on the contrary increase that of liquids. Is this the state of bodies which Mr. Faraday calls electro-tonic?

22.

In the supplement of No. 105. of the Institute for May 13, 1835, there is a notice of a memoir by Mr. Faraday the publication of which we are looking for. The experiment cited at the end of this notice appeared to me so striking and important in connection with the subject of the present memoir that I did not delay repeating it. Two copper wires, 400 feet long and $\frac{3}{4}$ lin. in diameter, carefully covered with silk ribbon, were coiled together in a helix round a hollow cylinder of wood, $1\frac{1}{2}$ inch in diameter. The ends of these two wires were united in a single one. The effect of this combination was beyond all my expectations; for by employing a voltaic pair of silver and zinc plates, which had only a surface of half a square inch, I obtained at the moment of disjunction a brilliant spark, and a violent shock which could scarcely be borne. The same effects took place when the pair of plates were reduced to a wire of platina and zinc. After having placed a cylinder of soft iron in the hollow of the wooden cylinder, the action was still more considerable. These effects were not much increased by the enlargement of the surface of the pair. A conducting wire of 400 feet having been employed alone, the spark and the shock were much more feeble; but on uniting in a circuit the two ends of the second wire of 400 feet, there was neither spark nor shock. This is perfectly conformable with Mr. Faraday's experiment.

Upon this I made the following experiment: In the hollow of the wooden cylinder I placed a cylinder of soft iron, $1\frac{1}{2}$ inch in diameter, forming the armature of the bar of soft iron. We will call the corresponding extremities of the bar and the armature A, a; B, b. The two wires of 400 feet of the helix coiled round the armature were united in one of 800 feet, the ends of which were conducted by a multiplier to the poles of a voltaic pair of plates about $\frac{1}{4}$ foot square. The helix surrounding the bar terminated at a pile of a foot square, by means of a commutator *à bascule*. The deviation was 16°. The current which magnetized the horse-shoe bar being directed so as to produce in A the same magnetism as in a (A_n a_n, B_s b_s,), the needle advanced to 30°, and on reversing the current so as to produce contrary magnetisms (A_s a_n, B_n b_s) the needle receded from 16° to 10°, returning after a few oscillations to its first position at 16°. By employing a single wire of 400 feet, the other wire not forming a circuit, the deviation of the needle was 21°. By the arrangement A_n a_n, B_s b_s, the needle advanced to $33\frac{1}{2}$°; it receded on the contrary to 13° when the magnetism of the bar and of the armature attracted one another, (A_s a_n, B_n b_s). After having united in a circuit the second wire of 400 feet, the deviation of the needle having been the same as before, that is 21°, the needle advanced and receded by the arrangements above mentioned respectively to 30° and 40°. We see that in this case the needle is rather less affected than in the case of the disjunction of the second wire; but I expected, as a necessary consequence, that the needle would not be at all affected, for I had received no shock nor spark in the analogous experiment. I confess that at present I am unable to enter into an explanation of the striking difference which subsists between the current of reaction and the magneto-electric current.

23.

With regard to the magnetic machine, it will be of great importance to weaken the effect of the counter-current, without at the same time weakening the magnetism of the bars. It is the alternate combination of the pairs of plates or the voltaic pile which permits us to increase the speed of rotation at will. We know that the magnetic power of the current is not sensibly augmented by increasing the number of the pairs of plates, but the counter-current is considerably weakened by it, being forced to pass through a great many layers of liquid. In fact, on using twelve voltaic pairs, each half a square foot, instead of four copper troughs, each with a surface of two square feet, which I had hitherto used, the speed of rotation rose to at least 250—300 revolutions in a minute, a number which I was able only to estimate, having been unable to count them. The acid which I employed was extremely weak, and had served for many previous experiments. The development of gas was imperceptible either by sight or smell. Having immersed two thick copper wires in the cups *p* and *o*, and having taken hold of them with my hands dipped in salt water, I received during the motion of the apparatus violent shocks, and felt an extreme pricking sensation in the upper part of my body. The mechanical effect of the apparatus corresponding to the speed of 250 to 300 revolutions in a minute has been valued at half the force of a man. I shall at a future time apply to it an exact dynamometric apparatus.

I have not been able to make further experiments on this subject, and I am obliged to interrupt my investigations for a time; but from what precedes, I may perhaps be justified in maintaining, that the superiority of this new motor, with regard to the absence of danger, the simplicity of the application, and the expense of the materials necessary to keep it in action, is placed beyond doubt.

Note on the Application of Electro-Magnetism as a Mechanical Power; by I. D. Botto, *Professor of Natural Philosophy in the Royal University of Turin.*

From the *Bibliothèque Universelle*, &c., vol. lvi. Geneva, (1834, vol. II. p. 312. July.)

THE remarkable energy with which the magnetic action is developed in soft iron by induction from electricity in motion is well known.

The possibility of the application of this new power to machines possessing some interest, I have decided on publishing the results which I have obtained relating to this subject*.

The mechanism which I have employed consists first of a lever put in motion (in the manner of a metronome) by the alternating action of two fixed electro-magnetic cylinders exerted on a third moveable cylinder connected with the lower arm of the lever, the upper arm of which maintains a metallic wheel, serving in the ordinary way as a regulator, in a continuous gyratory motion.

The apparatus was so disposed, that, the axes of the three cylinders being perfectly equal and situated in the same vertical plane perpendicular to the axis of motion, the oscillating cylinder placed itself by one of its extremities alternately in contact with and in the direction of, each of the two other cylinders, stationed at the limits of its excursions; and each time, at that very instant, the direction of the magnetizing current in its spiral was changed, the remainder of the circuit preserving the same direction, so as to produce poles of the same name in the fixed cylinders, at the two extremities facing the moveable cylinder. The change of direction which has just been mentioned is obtained by means of the known mechanism of the bascule, the communications of which are interchanged by the motion of the machine itself.

It is evident that from this arrangement the middle cylinder must undergo corresponding alternations of attraction and repulsion, by the effect of which the apparatus is set in motion, as it were by itself, and maintains itself in action by the œconomy of the magnetic forces which animate it, and which are produced by the electric currents.

I endeavoured to operate without the spiral of the middle cylinder, and by causing the two fixed cylinders magnetized alternately to act

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* I ought to state, that the hope of giving a greater extension to my observations, and also the necessity of absenting myself from Turin, have caused me to defer the publication of the facts which I announce, although I should have done so at the end of June. But I have been obliged to decide respecting it, having seen in the last number of the Piedmontese Gazette, that M. Jacobi of Königsberg has succeeded in obtaining a phænomenon of continuous motion by the intervention only of the electro-magnetic power.

upon the latter. But an adhesion which continued after the cessation of the magnetizing currents then contributed to diminish the mechanical effect; whilst in the preceding arrangement the adhesion not only ceased, but to a certain point changed to repulsion, with the same rapidity with which the current, scarcely interrupted an instant by the action of the bascule, precipitated itself (the communication being inverted) into the middle spiral, in a direction contrary to its original one, resuming its ordinary course in the two other spirals.

The motion of the lever and of the regulator, resulting from this arrangement, is perfectly free; at first rather slow, it soon and by degrees acquires the maximum of velocity which the energy of the currents producing it allows of,—a velocity which is afterwards maintained equal to the intensity of the current itself, and as long as the latter remains in action*.

I shall say nothing at present respecting some observations which I have on this occasion collected, upon the employment of different acid and saline solutions, and of sea water.

It is not without especial interest that we contemplate these new effects of a force developed in so singular a manner from the masses of bodies; and it is difficult not to be carried away by flattering anticipations respecting the ulterior applications which the acquisition of this mysterious motive force suggests†.

The dimensions of the apparatus which has just been described are small, and such as the current produced by fifteen elements of nine square inches can put in motion. The electro-dynamic cylinders, which principally determine the limits of the mechanical effect, are one decimetre in length and a centimetre and a half in diameter; these are surrounded by a wire coiled in a spiral, the length of which is 40 metres, and half a millimetre in diameter. The lever is of wood; the upper and lower arms are respectively 35 and 7 centimetres long; the amplitude of its oscillations is 15°. Lastly, the regulator weighs 2½ kilogrammes; and the total weight of the mechanism is about 5 kilogrammes.

Considerations which readily presented themselves regarding the relations between the maximum of the magneto-mechanic effect of the apparatus and the dimensions of its different parts, have made me think

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* There is a great analogy, both with regard to the general arrangement of the apparatus and the nature of the motive power, between the electro-magnetic apparatus of M. Botto, and the electrical clock of M. Zamboni, described in the *Bibl. Univ.*, t. xlvii. p. 183. (1831). It will be recollected that Zamboni's clock is put in motion by a pendulum, alternately attracted and repelled by the poles of two of the dry piles which bear his name.—A. DE LA RIVE.

† The Chev. Avogrado and the Chev. Bidone, who have successively seen the apparatus in motion, did not dissemble the agreeable surprise which they experienced, not merely from the novelty of the fact, but also from the reflections suggested by the general relations which may connect this simple result with the progress of physics and mechanics.

of substituting for the cylindrical form the ordinary U form of electro-magnetic bars, and of augmenting within certain limits the number and magnitude of these pieces, as well as the length of the spirals.

But not having arrived at the termination of my experiments on this subject, I confine myself for the present to pointing out the above-mentioned facts, which I have thought proper to make known, not only as interesting to science, but also because the study of the new class of effects with which it is connected may be considered as fertile in useful consequences in a physico-mechanical point of view*.

Part of a Lecture on Electro-Magnetism, delivered to the Philosophical Society at Zurich, February the 18*th,* 1833; *by the late Dr.* R. SCHULTHESS†.

From a work intitled "Ueber Electromagnetismus, nebst Angabe einer neuen durch electromagnetische Kräfte bewegten Maschine: Drey Vorlesungen von Dr. R. Schulthess. Zurich, 1835."

THOUGH electro-magnetism from its intrinsic importance certainly is one of the most remarkable and interesting discoveries of modern times, yet it would create a much higher interest, and gain in popularity, if it could be rendered practically useful. For some time past I had been occupied with the idea, whether the power of electro-magnets, which without doubt might be infinitely increased, could be applied as a *motive power for machinery*. It was known from Van Moll's experiments, that when the electric current which runs through the spiral of an electro-magnet is rapidly reversed, the magnetic poles are likewise instantly reversed; and that a light iron keeper, which is supported from its poles, falls off at that moment, but is immediately re-attracted. The experiments of Henry and Ten Eyck showed that the power of such electro-magnets might be very greatly augmented. The thought struck me, that a considerably heavier keeper or armature might be suspended from such an electro-magnet, and that by the attraction and repulsion of the same a machine might be put in motion; at the same time the action of the gyrotrope, and thereby the reversion of the poles, might also be effected: and although the distance which the keeper would recede from the magnet could be but very inconsiderable, still I thought that the rapidity with which these motions would follow each other might in great measure compensate for this defect. I was, however,

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* The apparatus mentioned in this Note was constructed by M. Jast, mechanician of the Royal University of Turin, who executes with the same success and the same accuracy all other kinds of philosophical instruments.

† The translation has been communicated by E. Solly, jun., Esq.

filled with mistrust on observing that this thought, which appeared to me so simple and natural, was not mentioned by any of the numerous natural philosophers who occupy themselves so assiduously with electro-magnetic experiments. I could not believe but that these notions must have struck them; but I was forced to suppose that they had either seen immediately the impracticability of them, or that, even if they had made some experiments upon the subject, they had met with insurmountable difficulties in its application. This long deterred me from making any experiments; but in the lecture which I gave before this Society on the 10th of December 1832, I could not refrain, when speaking of the powerful electro-magnets of Henry and Ten Eyck, from asking the question, "whether such a considerable power as that which is obtained by interrupting the electric current and then restoring it, could not be applied with advantage to mechanical science." After that lecture I considered the subject again, and thought I had convinced myself of its practicability; but that even if it were so, the result could not be very important, because the motion of the keeper must necessarily be very small. Notwithstanding, I had a more powerful electro-magnet made than any I had hitherto possessed, with which I intended to try the experiment; and I regarded the expense the less, as this apparatus appeared to me at the same time to be very appropriate for the evolution of the currents observed by Faraday; for this purpose the armature also must be covered with copper wire, and then each time the poles of the electro-magnet are reversed, a magneto-electric current circulates through this wire. In the mean time I found another method whereby the object I had in view might be effected, and which would allow a greater degree of motion to the armature: I thought I could effect this in the following manner. I placed on the table two cylindrical soft-iron horseshoes bound round with similar wires; so that when the electrical current was transmitted through both wires, the similar poles should lie opposite to each other: between these, and at a small distance from either, I placed a cylinder of soft iron, serving for a keeper; and then I expected to see the armature play to and fro between the two electro-magnets when I sent the electric current first round the one and then round the other electro-magnet. After several fruitless rough experiments it succeeded at last, and I therefore then instructed a turner to make an apparatus, that I might be able to repeat, by means of it, in a more easy and perfect manner, these yet very imperfect experiments. I had proceeded so far, when, on the 4th of January, I received the latest part of Baumgärtner's *Zeitschrift*, published at Vienna on the 17th of November 1832. I there observed a treatise, intitled, "*Electro-magnetic Experiments of Salvatore Dal Negro, Professor of Natural Philosophy in the Imperial University at Padua*," (translated from the Italian). The author says in the Introduction:

"Philosophers have already known for some time the power of electricity to make soft iron magnetic. In the year 1825, Sturgeon magnetized cylindrical horseshoes of soft iron by means of copper wires wound round them, connecting the ends of the wires with the plates of an electromotor. Professor Van Moll of Utrecht saw this experiment performed in the physical laboratory of the London University by Mr. Watkins, and he obtained on repetition those remarkable results described in *Bibl. Univ.*, cah. 45. p. 19. This new method of communicating such great attractive power to iron created in me the desire of repeating the experiments, and principally of taking into consideration the application of this attractive power, which it appears may be infinitely increased, to some useful purpose. I give these experiments to the public in the conviction that a force so easily evolved and so very powerful justifies repeated and varied experiments. In my experiments the electromotors employed were without doubt smaller than any hitherto used, and these notwithstanding produced the same results: new circumstances and new laws were observed and discovered respecting the manner of increasing the magnetic power evolved by electromotors, of producing in them currents now similar, and now different, sometimes in the same, sometimes in opposite directions, and by the success of these experiments of setting a lever in motion in different ways, and thus finally enriching natural philosophy with a new motive power."

It is easy to imagine with what avidity I read this notice, partly from joy at seeing my idea, of the practical application of which I still had many doubts, mentioned by another person, and partly somewhat vexed that the priority of my invention, if it really was as useful as my fancy made me think it, was snatched away from me. I therefore read with intense eagerness this paper; but my expectations were in a great measure disappointed; for it was only at the end that Dal Negro gave some short, and to me not altogether comprehensible hints concerning his experiments on the application of the power of electro-magnets to moving machinery, after having described a considerable number of other experiments, the principal object of which was to give with the least possible means to a soft-iron horseshoe the greatest possible magnetic power. He took seven different horseshoes, varying from 0·29 to 5 killogrammes* in weight: the copper wire with which he enveloped them, in from 37 to 64 coils, had a diameter of 8·2 to 8·4 mill.; the zinc plates of the four different electromotors had surfaces $= \frac{1}{4}$, $\frac{3}{4}$, $2\frac{1}{5}$, and $4\frac{2}{5}$ square feet each; the dilute acid employed consisted of $\frac{1}{60}$ of sulphuric acid and $\frac{1}{60}$ of nitric acid in 1 of water. With these electro-magnets Dal Negro obtained remarkably powerful results. The largest, (weighing 5 killog., surrounded with 64 coils of copper wire of 8·4 millim. diameter,) with the armature weighing 2 killog., when connected with the largest electromotor, supported from 108 up to 117 killog. Dal Negro attri-

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* [1 millimetre = ·03937 English inch.
1 killogramme = 2 lbs. 3 oz. 5 dram.—Trans.]

butes the greatness of this effect principally to the weight of the armature, and also to the rounded form of this and also of the poles of the magnet; but he seems to think the great thickness of the wire, namely, 8·4 mill., of no moment. With this important particular the reader is not acquainted till the end of the paper, where a table of the diameter of the wires is added. He infers from his experiments, "that a temporary magnet (as he calls the electro-magnets) can only acquire a magnetic power proportional to its mass;" and says, "experience will show what is the smallest electro-motive surface required to give the maximum of power;" and adds, "these experiments will become the more necessary when electro-magnetic power has been applied to some useful purpose."

The following remarks of Dal Negro on the property of some pieces of iron either not to take any magnetism at all, or only to take it under certain circumstances from the inverted electric current, were to me very mysterious and enigmatical. He says:

I. (1.) "I had several cylindrical soft-iron horseshoes made, of different weight, and experimented with them according to Sturgeon's method; *for the most part none of them were at all magnetic.* Indeed, in a small bar of iron which was cut into four pieces, and the single pieces made into magnets of the above-mentioned size, only one of them became a powerful magnet; the others were little or not at all magnetic."

(2.) "In the same way curved square bars gave no appreciable results: it appears from this that the cylindrical form is essentially necessary to the development of this temporary magnetism. I also endeavoured, without success, to magnetize hollow cylinders."

(7.) "During the first experiments it often happened that when the weight which the magnet could support had reached its maximum, all on a sudden the horseshoe would become incapable of re-acquiring magnetism, *not even so much as to be able to support the keeper again.* Van Moll also appears to have observed this phænomenon."

"Fortunately it appeared that by continually weakening [*abstumpfen*] (?) the same magnet, one is enabled to repeat the experiments, and each time make it support a considerable weight."

III. (5.) "It is remarkable that I did not observe with these two magnets (namely, the two strongest,) the phænomena mentioned in the first part of this treatise, No. (7). I am much inclined to believe that this depends upon the magnet being made to support the greatest possible weight for a longer or shorter time. But here I must not omit to mention, that often, when I removed from the magnets the helices which I had been using, either for the purpose of altering the number of coils or the thickness of the wire composing them, *the magnets for several days would not take up the least magnetism.* On continuing these experiments I obtained the same phænomenon with the magnet C (weighing 0·29 killog.): the original coil had been removed, but immediately replaced by a smaller number of coils. To this magnet (even after 14 days had elapsed) I could in no way communicate any appreciable magnetic power."

To me these statements are very enigmatical; at least I have never observed anything similar in my own experiments; and not only different horseshoes of soft iron, but several varieties of hardened steel have always appeared to me very susceptible of electro-magnetism; steel of course in a less degree than iron, but notwithstanding much more so than I had expected from the observations of others. Every time when in my experiments no action was observed, or at most only a very feeble action, I found that either the circuit was somewhere interrupted, or the battery was too weak.

I was most interested by the last portion of Dal Negro's paper; it is as follows:

"As I had been successful in producing temporary magnets of very great power with very small electromotors, I endeavoured to apply this new power to moving machinery. I will now briefly state by what means I endeavoured to set a lever in motion. I first used a magnetic steel bar, placed vertically beneath one end of a temporary magnet: the bar vibrated from the attractions and repulsions which took place between its south pole and the north and south poles of the electro-magnet. In the same way a motion may be effected in a horizontal plane. I also set in motion a similar bar, by allowing a piece of iron, set free from the magnet at the moment when its power became = 0, to fall on one of its ends; after this it was immediately re-attracted. This can be effected in two ways; the one may be employed when a quick motion is to be produced, and the second when a greater force is wanted: in the first case the weight falls only just out of the power of the magnet's attraction; and the instant the weight has fallen upon the bar or lever, it is re-attracted by the magnet that the action may be repeated: this weight is always very small in comparison with that which the magnet can support whilst in contact. In the second case the whole weight which the magnet can carry is employed, and use is made of the force which draws it to the magnet. This can be done in several ways. One of them forms a very powerful electro-magnetic ram. I shall not fail to make known the action of this new machine, and hope thereby to satisfy those in particular who are endeavouring to set a machine in motion at the least possible expense."

I must confess that I cannot from this too short and uncertain description form any clear idea of Dal Negro's process, and I am therefore very curious to see his forthcoming paper. I could only clearly understand his first method of setting a lever in motion, and I determined to make these experiments as soon as I had finished those which I had previously commenced. I hastened the completion of the apparatus so that I was enabled to exhibit it before the Mechanics' Society on the 18th of January. The construction of it is as follows (Plate VII.):

On a small board A′ A′, resting on four feet, are placed the two similar electro-magnets G G′; each of them weighs about the $\frac{1}{8}$th part of a pound, and is wound round with 80 convolutions of copper wire of 0·5 line in diameter, covered with silk: they can be made to recede from or approach each other, and are fixed in their places by wooden screws J J′. The board has in the middle a hole B B, cut in it where the poles lie; in this a frame E E is hung, made out of four laths joined together, forming an oblong, of which the long sides are vertical. Under the upper side, and parallel with it, is the iron cylinder or armature K K′. Fixed in the side laths, and about $1\frac{1}{2}$ inch below it, is placed a stout iron wire F parallel with the cylinder and passing through the sides of the frame, and two pieces of wood C C fixed in the under side of the board A′A′; this wire serves as an axis, which allows to the frame a pendulum-like motion. That part of the frame which is below the axis is twice as long as the upper part, and weights O O may be placed on its base E″ E″. The electric current was conveyed to the electro-magnets through a gyrotrope P P, standing on the board A A, which serves as a basis to the whole machine. The wires from the electromotors are connected with the two middle cups of mercury *dd′*, in each of which dips the central portion of a wire bent into the form of an anchor Q Q. These two wires are fixed to a wooden bow, by the motion of which the alternate ends of the two bent wires dip either into the cups on the one side or into those on the other; into the one cup *d*, dip the wires from *f*, coming from the one plate of the electromotor; and into the other cup *d′*, those from the other plate. The motion of this bow is effected in a very simple way by means of the motion of the frame E E′ E″, so that when the iron cylinder is attracted by the electro-magnet on the right hand, the bow of the gyrotrope is driven by the lower part of the frame to the left hand, by which motion the left-hand electro-magnet is brought into action. The working of this little self-acting apparatus is so quick and efficient, that another small machine, for instance, a wheel, might very easily be set in motion by it. Before the result was quite successful, there was still another difficulty to be overcome. It is a well-known fact that electro-magnets, when the connexion of the wires with the electromotors is interrupted, do not instantly lose all their magnetism, but are capable of carrying a considerable weight for some time. The bad effects which this remaining magnetism would have on the motion of the armature between the two electro-magnets would undoubtedly be greatly counteracted by the magnets being placed with the dissimilar poles opposite to each other. In this form of arrangement it is evident that the magnet in action would be much stronger, and assist in destroying the remaining weaker magnetism in the other magnet. There would therefore be a moment when the magnetism became = 0, and at that moment I expected that the armature would be disengaged, and then be attracted to the active electro-magnet. However, this interval was of such momentary duration that the armature remained attached to the passive magnet. I then took, instead of the armature of soft iron, a steel magnet of exactly the same form and shape, and placed it so that its poles were always opposite to the similar poles of the electro-magnets; but even with this alteration the same result took place; it also happened when the electric current was sent at the same time, but in an opposite direction, to the other electro-magnet. As the power of the electro-magnet was considerably greater than that of the steel magnet, I could not expect to obtain more powerful effects than I had obtained with the soft iron. At last I determined to prevent any possible contact between the armature and the electro-magnets: this I effected by wrapping the armature up in paper, so as always to keep it at a small distance from the poles of the magnet. The result was now quite satisfactory. I also enveloped the steel magnet in the same way, and it appeared to me that with the first-mentioned armature the motion was quicker and more energetic than with the latter. If we consider that electro-magnets have already been made which were capable of carrying 20 cwt., and that there is no reason to doubt that they may be made infinitely more powerful, I think I may assert boldly that *electro-magnetism may certainly be employed for the purpose of moving machines.*

On the Influence of a Spiral Conductor in increasing the Intensity of Electricity from a Galvanic Arrangement of a Single Pair, &c. By Professor HENRY, *of New Jersey, U. S.**

IN the American Journal of Science for July 1832, I announced a fact in Galvanism which I believe had never before been published. The same fact, however, appears to have been since observed by Mr. Faraday, and has lately been noticed by him in the November number of the London and Edinburgh Journal of Science for 1834.

The phænomenon as described by me is as follows: "When a small

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* Read before the American Philosophical Society, Feb. 6th, 1835.—This has been annexed to the preceding papers as being referred to in them, and as a slight notice of it only has appeared in this country: see Phil. Mag. and Annals, vol. x. p. 314.

battery is moderately excited by diluted acid, and its poles, terminated by cups of mercury, are connected by a copper wire not more than a foot in length, no spark is perceived when the connexion is either formed or broken; but if a wire thirty or forty feet long be used instead of the short wire, though no spark will be perceptible when the connexion is made, yet when it is broken by drawing one end of the wire from its cup of mercury, a vivid spark is produced. If the action of the battery be very intense, a spark will be given by a short wire; in this case it is only necessary to wait a few minutes until the action partially subsides, or no more sparks are given; if the long wire be now substituted, a spark will again be obtained. The effect appears somewhat increased by coiling the wire into a helix; it seems also to depend in some measure on the length and thickness of the wire. I can account for these phænomena only by supposing the long wire to become charged with electricity, which, by its reaction on itself, projects a spark when the connexion is broken*."

The above was published immediately before my removal from Albany to Princeton; and new duties interrupted for a time the further prosecution of the subject. I have, however, been able during the past year to resume in part my investigations, and among others, have made a number of observations and experiments which develop some new circumstances in reference to this curious phænomenon.

These, though not as complete as I could wish, are now presented to the Society, with the belief that they will be interesting at this time on account of the recent publication of Mr. Faraday on the same subject.

The experiments are not given in the precise order in which they were first made, but in that which I deem best suited to render them easily understood; they have, however, been repeated for publication in almost the same order in which they are here given.

1. A galvanic battery, consisting of a single plate of zinc and copper, and exposing one and a half square feet of zinc surface, including both sides of the plate, was excited with diluted sulphuric acid, and then permitted to stand until the intensity of the action became nearly constant. The poles connected by a piece of copper bell-wire, of the ordinary size and five inches long, gave no spark when the contact was broken.

2. A long portion of wire, from the same piece with that used in the last experiment, was divided into equal lengths of fifteen feet, by making a loop at each division, which could be inserted into the cups of mercury on the poles of the battery. These loops being amalgamated, and dipped in succession into one of the cups while the first end of the wire

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* Silliman's Journal, vol. xxii. page 408.

constantly remained in the other, the effect was noted. The first length, or fifteen feet, gave a very feeble spark, which was scarcely perceptible. The second, or thirty feet, produced a spark a little more intense, and the effect constantly increased with each additional length, until one hundred and twenty feet were used; beyond this there was no perceptible increase; and a wire of two hundred and forty feet gave a spark of rather less intensity. From other observations I infer, that the length necessary to produce a maximum result, varies with the intensity of the action of the battery, and also with its size.

3. With equal lengths of copper wire of unequal diameters, the effect was greater with the larger: this also appears to depend in some degree on the size of the battery.

4. A length of about forty feet of the wire used in experiments first and second, was covered with silk, and coiled into a cylindrical helix of about two inches in height and the same in diameter. This gave a more intense spark than the same wire when uncoiled.

5. A ribbon of sheet copper, nearly an inch wide and twenty-eight and a half feet long, was covered with silk, and rolled into a flat spiral similar to the form in which woollen binding is found in commerce. With this a vivid spark was produced, accompanied by a loud snap. The same ribbon uncoiled gave a feeble spark, similar in intensity to that produced by the wire in experiment third. When coiled again, the snap was produced as at first. This was repeated many times in succession, and always with the same result.

6. To test still further the influence of coiling, a second ribbon was procured precisely similar in length and in all other respects to the one used in the last experiment. The effect was noted with one of these coiled into a flat spiral and the other uncoiled, and again with the first uncoiled and the second coiled. When uncoiled, each gave a feeble spark of apparently equal intensity; when coiled, a loud snap. One of these ribbons was next doubled into two equal strands, and then rolled into a double spiral with the point of doubling at the centre. By this arrangement, the electricity, in passing through the spiral, would move in opposite directions in each contiguous spire, and it was supposed that in this case the opposite actions which might be produced would neutralize each other. The result was in accordance with the anticipation: the double spiral gave no spark whatever, while the other ribbon coiled into a single spiral produced as before a loud snap. Lest the effect might be due to some accidental touching of the different spires, the double spiral was covered with an additional coating of silk, and also the other ribbon was coiled in the same manner; the effect with both was the same.

7. In order to increase if possible the intensity of the spark while the battery remained the same, larger spirals were applied in succession.

The effect was increased, until one of ninety-six feet long, an inch and a half wide, and weighing fifteen pounds, was used. The snap from this was so loud that it could be distinctly heard in an adjoining room with the intervening door closed. Want of materials has prevented me from trying a larger spiral conductor than this; but it is probable that there is a length which, with a given quantity and intensity of galvanism, would produce a maximum effect. When the size of the battery is increased, a much greater effect is produced with the same spiral. Thus when the galvanic apparatus described in the first article is arranged as a calorimotor of eight pairs, the snap produced on breaking contact with the spiral last described resembled the discharge of a small Leyden jar highly charged.

8. A handle of thick copper was soldered on each end of the large spiral at right angles to the ribbon, similar to those attached to the wires in Pixii's magneto-electric machine for giving shocks. When one of these was grasped by each hand and the contact broken, a shock was received which was felt at the elbows; and this was repeated as often as the contact was broken. This shock is rather a singular phænomenon, since it appears to be produced by a lateral discharge, and it is therefore important to determine its direction in reference to the primary current.

9. A shock is also received when the copper of the battery is grasped by one hand, and the handle attached to the copper pole of the ribbon with the other. This may be called the direct shock, since it is produced by a part of the direct current. It is, however, far less intense than that produced by the lateral discharge.

10. When the poles were joined by two coils connected by a cup of mercury between them, a spark was produced by breaking the circuit at the middle point; and when a pair of platina wires was introduced into the circuit with the large coil and immersed in a solution of acid, decomposition took place in the liquid at each rupture of contact, as was shown by a bubble of gas given off at each wire. It must be recollected that the shocks and the decomposition here described were produced by the electricity from a single pair of plates.

11. The contact with the poles of the battery and the large spiral being broken in a vessel containing a mixture of hydrogen and atmospheric air, an explosion was produced.

I should also mention that the spark is generally attended with a deflagration of the mercury, and that when the end of the spiral is brought in contact with the edge of the copper cup or the plate of the battery, a vivid deflagration of the metal takes place. The sides of the cup sometimes give a spark when none can be drawn from the surface of the mercury. This circumstance requires to be guarded against when experimenting on the comparative intensities of sparks from different arrangements. If the battery formerly described* be arranged as a calorimotor, and one end of a large spiral conductor be attached to one pole, and the other end drawn along the edge of the connector, a series of loud and rapid explosions is produced, accompanied by a brilliant deflagration of the metal; and this takes place when the excitement of the battery is too feeble to heat to redness a small platina wire.

12. A number of experiments were made to determine the effect of introducing a cylinder of soft iron into the axis of the flat spiral, in reference to the shock, the spark, &c.; but no difference could be observed with the large spiral conductor; the effect of the iron was merged in that of the spiral. When, however, one of the smaller ribbons was formed into a hollow cylindrical helix of about nine inches long, and a cylinder of soft iron an inch and a half in diameter was inserted, the spark appeared a little more intense than without the iron. The obliquity of the spires in this case was unfavourable to their mutual action, while the magnetism was greater than with the flat spiral, since the conductor closely surrounded the whole length of the cylinder.

I would infer, from these experiments, that some effects heretofore attributed to magneto-electric action are chiefly due to the reaction on each other of the several spires of the coil which surround the magnet.

13. One of the most singular results in this investigation was first obtained in operating with a large galvanic battery. The whole instrument was arranged as a calorimotor of eight pairs, and a large spiral conductor introduced into the circuit, while a piece of thick copper wire about five inches long united the poles. In this state an explosion or loud snap was produced, not only when the contact was broken at the spiral, but also when one end of the short wire at the other extremity of the apparatus was drawn from its cup. All the other short moveable connectors of the battery gave a similar result. When the spiral was removed from the circuit and a short wire substituted, no effect of the kind was produced. From this experiment it appears that the influence of the spiral is exerted through at least eight alternations of zinc, acid, and copper, and thus gives to a short wire at the other extremity of the circuit the power of producing a spark.

14. The influence of the coil was likewise manifest when the zinc and copper plates of a single pair were separated from each other to the distance of fourteen inches in a trough without partitions, filled with diluted acid. Although the electrical intensity in this case must have been very low, yet there was but little reduction in the apparent intensity of the spark.

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* This battery consisted of eighty-eight elements or pairs, composed of plates of rolled zinc nearly one-eighth of an inch thick, nine inches wide, and twelve inches long, inserted into copper cases open at top and bottom.

15. The spiral conductor produces, however, little or no increase of effect when introduced into a galvanic circuit of considerable intensity. Thus when the large spiral used in experiment seventh, eighth, &c. was made to connect the poles of two Cruikshank's troughs, each containing fifty-six four-inch plates, no greater effect was perceived than with a short thick wire: in both cases in making the contact a feeble spark was given, attended with a slight deflagration of the mercury. The batteries at the same time were in sufficiently intense action to give a disagreeable shock. It is probable, however, that if the length of the coil were increased in some proportion to the increase of intensity, an increased effect would still be produced.

In operating with the apparatus described in the last experiment, a phænomenon was observed in reference to the action of the battery itself, which I do not recollect to have seen mentioned, although it is intimately connected with the facts of magneto-electricity, as well as with the subject of these investigations, viz. When the body is made to form a part of a galvanic circuit composed of a number of elements, a shock is, of course, felt at the moment of completing the circuit. If the battery be not very large, little or no effect will be perceived during the uninterrupted circulation of the galvanic current; but if the circuit be interrupted by breaking the contact at any point, a shock will be felt at the moment, nearly as intense as that given when the contact was first formed. The secondary shock is rendered more evident, when the battery is in feeble action, by placing in the mouth the end of one of the wires connected with the poles; a shock and flash of light will be perceived when the circuit is completed, and also the same when the contact is broken at any point; but nothing of the kind will be perceived in the intermediate time, although the circuit may continue uninterrupted for some minutes. This I consider an important fact in reference to the action of the voltaic current.

The phænomena described in this paper appear to be intimately connected with those of magneto-electricity, and this opinion I advanced with the announcement of the first fact of these researches in the American Journal of Science. They may, I conceive, be all referred to that species of dynamical *Induction* discovered by Mr. Faraday, which produces the following phænomenon, namely: when two wires, A and B, are placed side by side, but not in contact, and a voltaic current is passed through A, there is a current produced in B, but in an opposite direction. The current in B exists only for an instant, although the current in A may be indefinitely continued; but if the current in A be stopped, there is produced in B a second current, in an opposite direction however to the first current.

The above fundamental fact in magneto-electricity appears to me to be a direct consequence of the statical principles of "Electrical *Induction*" as mathematically investigated by Cavendish, Poisson, and others. When the two wires A and B are in their natural state, an equilibrium is sustained by the attractions and repulsions of the two fluids in each wire; or, according to the theory of Franklin and Cavendish, by the attractions and repulsions of the one fluid, and the matter of the two wires. If a current of free electricity be passed through A, the natural equilibrium of B will be disturbed for an instant, in a similar manner to the disturbance of the equilibrium in an insulated conductor by the sudden addition of fluid to a contiguous conductor. On account of the repulsive action of the fluid, the current in B will have an opposite direction to that in A; and if the intensity of action remains constant, a new state of equilibrium will be assumed. The second state, however, of B may perhaps be regarded as one of tension; and as soon as the extra action ceases in it, the fluid in B will resume its natural state of distribution, and thus a returning current for an instant be produced.

The action of the spiral conductor in producing sparks is but another case of the same action; for since action and reaction are equal and in contrary directions, if a current established in A produces a current in an opposite direction in B, then a current transmitted through B should accelerate or increase the intensity of a current already existing in the same direction in A. In this way the current in the several successive spires of the coil may be conceived to accelerate, or to tend to accelerate each other; and when the contact is broken, the fluid of the first spire is projected from it with intensity by the repulsive action of the fluid in all the succeeding spires.

In the case of the double spiral conductor, in experiment sixth, the fluid is passing in an opposite direction; and according to the same views, a retardation or decrease of intensity should take place.

The phænomenon of the secondary shock with the battery appears to me to be a consequence of the law of Mr. Faraday. The parts of the human body contiguous to those through which the principal current is passing, may be considered as in the state of the second wire B; when the principal current ceases, a shock is produced by the returning current of the natural electricity of the body.

If this explanation be correct, the same principle will readily account for a curious phænomenon discovered several years since by Savary, but which I believe still remains an isolated fact. When a current is transmitted through a wire, and a number of small needles are placed transverse to it, but at different distances, the direction of the magnetic polarity of the needles varies with their distance from the conducting wire. The action is also periodical; diminishing as the distance increases, until it becomes zero; the polarity of the needles is then inverted, acquires a maximum, decreases to zero again, and then resumes the first polarity; several alternations of this kind being observed*. Now this is precisely what would take place if we suppose that the principal current induces a secondary one in an opposite direction in the air surrounding the conductor, and this again another in an opposite direction at a great distance, and so on. The needles at different distances would be acted on by the different currents, and thus the phænomena described be produced.

The action of the spiral is also probably connected with the fact in common electricity called the lateral discharge: and likewise with an appearance discovered some years since by Nobili, of a vivid light, produced when a Leyden jar is discharged through a flat spiral.

The foregoing views are not presumed to be given as exhibiting the actual operation of nature in producing the phænomena described, but rather as the hypotheses which have served as the basis of my investigations, and which may further serve as formulæ from which to deduce new consequences to be established or disproved by experiment.

Many points of this subject are involved in an obscurity which requires more precise and extended investigation; we may, however, confidently anticipate much additional light from the promised publication of Mr. Faraday's late researches in this branch o science.

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* Cumming's Demonferrande, p. 247; also Edinburgh Journal, October 1826.

NOTE.

[For an account of some recent investigations relative to the subject of the preceding Articles, the reader is referred to "An Inquiry into the Possibility and Advantage of the Application of Magnetism as a Moving Power: By the Rev. James William M'Gauley, in the Report of the Dublin Meeting of the British Association, 1835." See Phil. Mag. and Annals, vol. vii. p. 306. A further communication was made by the same gentleman at the Bristol Meeting, 1836. —EDIT.]

Article XXVII.

A singular case of the Equilibrium of Incompressible Fluids; by M. Ostrogradsky.

(Read to the Academy of St. Petersburgh, February 19, 1836.)

From "*Mémoires de l'Académie Impériale des Sciences de St. Petersbourg,*" vol. iii. part 3.

In mechanics there is no other distinction made between different bodies or different systems of bodies, besides that which relates to their masses, their positions, and their possible displacements. These displacements, together with the mass or quantity of the inertia of each element being given, we have all that is requisite as well as indispensable to enable us to treat of the equilibrium and movement of any system.

That a system subjected to the action of any forces may remain in equilibrio, it is necessary that the forces should be incapable of producing any of those displacements of which the system is susceptible. Now, as the forces, though capable of producing all the displacements of which the whole momentum is positive, are yet unable to produce any of those which correspond to the zero or the negative momentum, the equilibrium of the system consequently requires that the whole momentum should be zero or negative for all the possible displacements. From this leading principle we may in the easiest and simplest manner derive the condition of equilibrium of a system without knowing anything more than the masses and the possible displacements. More particularly with respect to the equilibrium of liquids, we have, for instance, no need of the experimental principle known by the name of the principle of *equal pressure*, which, before the publication of the *Mécanique Analytique*, mathematicians were accustomed to consider as the basis of the theory of the equilibrium of fluids. It is sufficient to know how a liquid mass can be displaced, and this is the only datum by means of which, in the *Mécanique Analytique*, the equations relative to the equilibrium of liquids are deduced. But Lagrange having neglected the consideration of the displacements, accompanied by an augmentation of volume, though such displacements are evidently possible, was unable to deduce from his analysis the essential condition, that the quantity which represents the pressure must necessarily be positive. This condition being added, the theory of Lagrange will be the most satisfactory of all those in which the liquids are considered as continuous masses; and if there is anything further to be remarked, it is that the incompressibility of the differential parallelopipeds is there taken as the condition of the incompressibility of the fluid, though it should be directly expressed that any portion (whether finite or infinitely small) of the liquid mass cannot be diminished. It is undoubtedly true that, as any volume may be supposed to consist of differential elements, the incompressibility of these elements involves that of the volume as a necessary consequence. But it would still be desirable to see how the calculus would directly express the incompressibility of any portion of the liquid volume.

In order to show this, let x, y, z represent the coordinates of a point of the liquid, which, because of their variability, will belong to all points. Any portion of the liquid volume may then be denoted by the expression $\int dx\,dy\,dz$, the integral being taken between the proper limits. It will then be necessary to find an expression which will represent $\int dx\,dy\,dz$ as suffering no diminution during any displacement that the liquid undergoes. For this purpose, let δx, δy, δz represent the projection of the space which the point (x, y, z) should have traversed in consequence of a displacement either actually made or only supposed in the liquid, on the coordinate axes x, y, z respectively. The point would after the displacement (whether positive or not) correspond to the coordinates $x + \delta x$, $y + \delta y$, $z + \delta z$, which, for the sake of brevity, we shall represent by X, Y, Z respectively. Every other point of the volume $\int dx\,dy\,dz$ being similarly displaced, the whole volume would assume another position in space, and its different points would be determined by the coordinates X, Y, Z, which may be regarded as functions (of x, y, z) altogether arbitrary. The volume $\int dx\,dy\,dz$ would, in its new position, become $\int dX\,dY\,dZ$, and consequently undergo the variation $\int dX\,dY\,dZ - \int dx\,dy\,dz$, which we are now about to develop.

In order to effect a better comparison of the integrals $\int dX\,dY\,dZ$ and $\int dx\,dy\,dz$ with one another, we must reduce them to the same variables and the same limits. This will be done by transforming X, Y, Z into x, y, z by means of the known formulæ. For this purpose we have

$$dX\,dY\,dZ = \left\{\frac{dX}{dx}\frac{dY}{dy}\frac{dZ}{dz} - \frac{dX}{dx}\frac{dY}{dz}\frac{dZ}{dy} + \frac{dX}{dy}\frac{dY}{dz}\frac{dZ}{dx} - \frac{dX}{dy}\frac{dY}{dx}\frac{dZ}{dz} + \frac{dX}{dz}\frac{dY}{dx}\frac{dZ}{dy} - \frac{dX}{dz}\frac{dY}{dy}\frac{dZ}{dx}\right\} dx\,dy\,dz,$$

and the variation of the volume will consequently become

$$\mathrm{S}\left[\frac{d\mathrm{X}}{dx}\frac{d\mathrm{Y}}{dy}\frac{d\mathrm{Z}}{dz}-\frac{d\mathrm{X}}{dx}\frac{d\mathrm{Y}}{dz}\frac{d\mathrm{Z}}{dy}+\frac{d\mathrm{X}}{dy}\frac{d\mathrm{Y}}{dz}\frac{d\mathrm{Z}}{dx}-\frac{d\mathrm{X}}{dy}\frac{d\mathrm{Y}}{dx}\frac{d\mathrm{Z}}{dz}\right.$$
$$\left.+\frac{d\mathrm{X}}{dz}\frac{d\mathrm{Y}}{dx}\frac{d\mathrm{Z}}{dy}-\frac{d\mathrm{X}}{dz}\frac{d\mathrm{Y}}{dy}\frac{d\mathrm{Z}}{dx}-1\right]dx\;dy\;dz:$$

by substituting for X, Y, Z their values $x+\delta x$, $y+\delta y$, $z+\delta z$, and rejecting (on the same principle as they are rejected in the differential calculus) all the infinitely small quantities, except those of the lowest order, we have as the variation of the volume

$$\mathrm{S}\left(\frac{d\,\delta x}{dx}+\frac{d\,\delta y}{dy}+\frac{d\,\delta z}{dz}\right)dx\;dy\;dz:$$

or, if we consider the possible displacement alone, the volume can only increase or continue unchanged. The foregoing variation must then be either zero or positive for all the possible displacements, and must be so whatever be the volume under consideration, that is to say, whatever be the limits of the integral

$$\mathrm{S}\left(\frac{d\,\delta x}{dx}+\frac{d\,\delta y}{dy}+\frac{d\,\delta z}{dz}\right)dx\;dy\;dz:$$

which cannot be the case unless we have

$$\frac{d\delta x}{dx}+\frac{d\,\delta y}{dy}\frac{d\,\delta z}{dz}\geqq 0$$

for all possible displacements. We might have employed the polar or any other coordinates whatever. We might likewise, if it were necessary, express the invariability of a portion of a mass, &c.

The geometers who have treated of the equilibrium of fluids in Euler's manner have considered the equilibrium of the differential parallelopipeds also, but the equilibrium might be determined for any portion of the volume, whether finite or infinitely small. Let us imagine, in the interior of the liquid, any volume at pleasure. The condition of equilibrium of this volume must be established in virtue of the moving forces applied to it and of the pressures on its surface. If we employ dm to represent an element of its mass answering to the coordinates x, y, z, and X, Y, Z to represent the accelerative forces parallel to the coordinate axes, the moving forces will be $\mathrm{X}\,dm$, $\mathrm{Y}\,dm$, $\mathrm{Z}\,dm$, and even other elements will be acted upon by similar forces.

This being supposed, let p be the pressure at the surface of the volume in question. If ds represents an element of this surface, and λ, μ, ν the angles formed with the coordinate axes by the normal to ds produced beyond the volume, $p\,ds$ will be the pressure sustained by the element ds, and $-p\cos.\lambda\,ds$, $-p\cos\mu\,ds$, $-p\cos\nu\,ds$ the components of that pressure. Now, each element of the volume being subject to the action of the forces $X\,dm$, $Y\,dm$, $Z\,dm$, and each element of the surface to that of the forces $-p\cos.\lambda\,ds$, $-p\cos\mu\,ds$ $-p\cos\nu\,ds$, the equilibrium of the volume must be determined by the mass of the invariable system. For this purpose we shall suppose that the volume has become inflexible, and is invariably connected with the origin of the coordinates: we shall transfer to that point all the forces $X\,dm$, $Y\,dm$, $Z\,dm$, $-p\,ds\cos\lambda$, $-p\,ds\cos\mu$, $-p\,ds\cos\nu$, and consider the couples to which this transfer will give rise. All the forces transferred to the origin of the coordinates will be reduced to three.

$$\int X\,dm - \int p\,ds\;\cos\lambda$$

$$\int Y\,dm - \int p\,ds\;\cos\mu$$

$$\int Z\,dm - \int p\,ds\;\cos\nu$$

which must vanish in case of equilibrium. This being supposed, we have

$$\int X\,dm = \int p\,ds\;\cos\lambda$$

$$\int Y\,dm = \int p\,ds\;\cos\mu \quad \ldots\ldots \quad (1.)$$

$$\int Z\,dm = \int p\,ds\;\cos\nu$$

The integrals which contain the element (dm) of the mass are referred to the entire volume of the liquid, and those which contain ds have reference only to the surface of that volume. The forces $X\,dm$, $Y\,dm$, $Z\,dm$, in consequence of being transferred to the origin of the coordinates, will give the couples $(xY - yX)\,dm$, $(yZ - zY)\,dm$, $(xX - xZ)\,dm$, which will be found respectively in the planes of xy, yz, zx. The forces $-p\,ds\cos\lambda$, $-p\,ds\cos\mu$, $-p\,ds\cos\nu$ will likewise give, in the coordinate planes, the couples

$$-(x\cos.\lambda - y\cos\mu)\,p\,ds,\quad -(y\cos.\nu - z\cos\mu)\,p\,ds,$$
$$-(z\cos\lambda - x\cos\nu)\,p\,ds.$$

The momenta of all the couples situated in the plane of xy being added together, and those of the couples situated in the planes of yz and zx being likewise added together, all the couples will thus be reduced to three.

$$\int (xY - yX)\,dm - \int (x\cos\lambda - y\cos.\mu)\,p\,ds$$

$$\int (yZ - zY)\,dm - \int (y\cos.\nu - z\cos\mu)\,p\,ds$$

$$\int (zX - xZ)\,dm - \int (z\cos\lambda - x\cos.\mu)\,p\,ds$$

As these must vanish in case of equilibrium, we have

$$\begin{aligned}\int (x\,\mathrm{Y} - y\,\mathrm{X})\,d\,m &= \int (x \cos.\mu - y \cos.\lambda)\,p\,d\,s\\ \int (y\,\mathrm{Z} - z\,\mathrm{Y})\,d\,m &= \int (y \cos.\nu - z \cos.\mu)\,p\,d\,s \quad .\;.\;(2.)\\ \int (z\,\mathrm{X} - x\,\mathrm{Z})\,d\,m &= \int (z \cos.\lambda - x \cos \mu)\,p\,d\,s.\end{aligned}$$

Now if we have an integral such as

$$\int \left(\frac{d\,\mathrm{P}}{d\,x} + \frac{d\,\mathrm{Q}}{d\,y} + \frac{d\,\mathrm{R}}{d\,z}\right) d\,\omega,$$

P, Q, R being functions of x, y, z, and $d\,\omega$ a differential volume, and if this integral is to be taken in the extent of a volume V, we shall have, as is known,

$$\int \left(\frac{d\,\mathrm{P}}{d\,x} + \frac{d\,\mathrm{Q}}{d\,y} + \frac{d\,\mathrm{R}}{d\,z}\right) d\,\omega = \int (\mathrm{P} \cos.\lambda + \mathrm{Q} \cos.\mu + \mathrm{R} \cos.\nu)\,d\,s.$$

The latter integral is taken only for the surface of the volume. We shall have as a consequence of the foregoing formula

$$\int p\,d\,s \cos.\lambda = \int \frac{d\,\mathrm{P}}{d\,x}\,d\,\omega$$

$$\int p\,d\,s \cos.\mu = \int \frac{d\,\mathrm{P}}{d\,y}\,d\,\omega$$

$$\int p\,d\,s \cos.\nu = \int \frac{d\,\mathrm{P}}{d\,z}\,d\,\omega$$

$$\int (x \cos \mu - y \cos \lambda)\,p\,d\,s = \int \left(x \frac{d\,\mathrm{P}}{d\,y} - y \frac{d\,\mathrm{P}}{d\,x}\right) d\,\omega$$

$$\int (y \cos.\nu - z \cos.\mu)\,p\,d\,s = \int \left(y \frac{d\,\mathrm{P}}{d\,z} - z \frac{d\,\mathrm{P}}{d\,y}\right) d\,\omega$$

$$\int (z \cos.\lambda - x \cos.\lambda)\,p\,d\,s = \int \left(z \frac{d\,\mathrm{P}}{d\,x} - x \frac{d\,\mathrm{P}}{d\,z}\right) d\,\omega$$

The equations of equilibrium (1) and (2) will become

$$\int \mathrm{X}\,d\,m = \int \frac{d\,\mathrm{P}}{d\,x}\,d\,\omega$$

$$\int \mathrm{Y}\,d\,m = \int \frac{d\,\mathrm{P}}{d\,y}\,d\,\omega$$

$$\int \mathrm{Z}\,d\,m = \int \frac{d\,\mathrm{P}}{d\,z}\,d\,\omega$$

$$\int (x\,\mathrm{Y} - y\,\mathrm{X})\,d\,m = \int \left(x \frac{d\,\mathrm{P}}{d\,y} - y \frac{d\,\mathrm{P}}{d\,x}\right) d\,\omega$$

$$\int (y\,Z - z\,Y)\,d\,m = \int \left(y\,\frac{d\,P}{d\,z} - z\,\frac{d\,P}{d\,y}\right) d\,\omega$$

$$\int (z\,X - x\,Z)\,d\,m = \int \left(z\,\frac{d\,P}{d\,x} - x\,\frac{d\,P}{d\,z}\right) d\,\omega,$$

and as the preceding equations must arise, whatever may be the limits of the integrals, we must necessarily have

$$X\,d\,m = \frac{d\,P}{d\,x}\,d\,\omega$$

$$Y\,d\,m = \frac{d\,P}{d\,y}\,d\,\omega$$

$$Z\,d\,m = \frac{d\,P}{d\,z}\,d\,\omega,$$

or $\left(\text{by making } \frac{d\,m}{d\,\omega} = \varrho\right) \frac{d\,P}{d\,x} = \varrho\,X, \frac{d\,P}{d\,y} = \varrho\,Y, \frac{d\,P}{d\,z} = \varrho\,Z$:

ϱ, which expresses the ratio of the mass $d\,m$ to the volume of $d\,\omega$, is but the expression of the density.

Let us now particularly consider a homogeneous liquid, the surface of which is entirely free and suffers no external pressure, we shall have at first for all the elements of the liquid, whatever dx, dy, dz may be,

$$d\,p = \varrho\,(X\,d\,x + Y\,d\,y + Z\,d\,z),$$

and then, for the surface

$$0 = X\,d\,x + Y\,d\,y + Z\,d\,z.$$

The last equation shows that the resultant of X, Y, Z is normal to the surface of the liquid. It is obvious that in the expression $X\,d\,x + Y\,d\,y + Z\,d\,z$, the differentials $d\,x$, $d\,y$, $d\,z$ belong to the passage from a molecule of the surface to another molecule situated also at the surface; but if we passed from a molecule at the surface to a molecule situated in the interior, we should have $X\,d\,x + Y\,d\,y + Z\,d\,z > 0$; which requires that the resultant of X, Y, Z should be directed towards the interior of the liquid mass. Thus it appears necessary for the equilibrium of a homogeneous liquid mass, that the differential $X\,d\,x + Y\,d\,y + Z\,d\,z$ should be exact for all the points of the mass, that the resultant of the forces X, Y, Z should be normal to the surface in all the elements of the surface, and that it should always act in the direction of the interior of the liquid. If these conditions are not fulfilled, it might be supposed that the liquid mass could never remain in equilibrio. I have, however, observed a case in which, although the last of these conditions is not satisfied, yet there is certainly an equilibrium.

Let us suppose that the liquid forms a spherical shell of any given thickness, and that each of its molecules is attracted towards the centre by a force proportioned to a function of the distance between the molecule and the centre, the equilibrium will necessarily take place. For the molecules situated at the same distance from the centre of attraction must all be moved in the same manner: if one of them approaches the centre all the others must approach it also, and within the same distance; and they cannot approach it in such a manner, that all of those situated on the same spherical surface described from the centre of attraction would retain the same motion; for the consequence of this would be a diminution of volume. The liquid will thus remain in equilibrio; but it is evident that the force which attracts each molecule situated in the interior surface of the shell has its direction outside of the liquid mass. Let $f(r)$ represent the attractions, a the radius of the inferior surface, and b the radius of the superior surface, we shall have

$$dp = -f(r)\,dr,$$

whence

$$p = \int_b^r f(r)\,dr = \int_r^b f(r)\,dr.$$

The pressure on the inferior surface will then be $\int_a^b f(r)\,dr$, and this pressure certainly differs from zero, a fact which is contrary to the generally received opinion.

Here then we have a singular case of equilibrium, which the established theory of liquids is not sufficient to explain, and which shows therefore that this theory is not yet sufficiently comprehensive.

Article XXVIII.

On the Origin of Organic Matter from simple Perceptible Matter, and on Organic Molecules and Atoms; together with some Remarks on the Power of Vision of the Human Eye; by Prof. C. G. Ehrenberg.

(From Poggendorff's *Annalen der Physik und Chemie*, vol. xxiv. p. 1.*)

THERE have been philosophers who have considered the magnitude of the elementary particles of bodies not to be so extremely minute as to be beyond the reach of the human senses; and there have been chemists who have conceived it to be possible to follow the successive combinations of the primitive substances or simple matter up to the formation of living organisms, indeed, have even given them a place in the class of observed facts. Others have thought that they perceived a peculiar process of fermentation, the product of which was the formation of minute animal and vegetable bodies, and to which process the name of infusorial fermentation has been given. The probability of obtaining organic bodies by chemico-synthetical means has of late gained ground chiefly because we had advanced so far as the preparation, almost synthetical in appearance, of some organic products by chemical means, and have observed galvanic processes or capillary actions, which are very similar to, perhaps quite the same as, certain organic phænomena. As this is one of the most interesting and important subjects of human inquiry, and has excited hopes of great and speedy results, it may be useful, in order that our inquiries may not deviate from the right road, to direct the attention of philosophers and chemists to some physiological experiments which I read in the Academy of Sciences of Berlin, and which I made known last year in a zoological memoir, extracts of which have been given in other journals devoted to physics, but which, so to speak, I myself will endeavour to clothe in a physical dress.

I. *Critical examination of the* Generatio Æquivoca.

I have for a long series of years been occupied in investigating the conditions of the *generatio æquivoca* of organic bodies. For this purpose it was necessary to subject to careful observation, as to their vital relations and primitive conditions, those organic bodies of whose origin a *generatio primitiva* or *spontanea* is asserted.

1. *Fungi.*—By careful examinations of the fungi and of mould, the

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* The Editor is indebted for the translation and communication of this paper to Mr. W. Francis.

systematic result of which I made known in the year 1818 in an inaugural dissertation, under the title *Sylvæ Mycologicæ Berolinenses*, I first discovered, in 1819, the real germination of the seeds of fungi and of mould, which has indeed been of late hypothetically received and described here and there, but of which the experiments and correct observations of the meritorious Florentine botanist, Micheli (1788), adduced in support of it, furnish no satisfactory proof. He saw, for instance, fungi grow where he had purposely sowed supposed seeds; but it is known that often fungi are found where no seeds have been purposely sown; and it remained doubtful to every accurate naturalist, whether, notwithstanding the precautions related by Micheli, those fungi had really originated from the so-called seeds which had been sown, or whether both the intended sowing and the origination of similar fungi coincided in time and place, solely because the conditions necessary to the *generatio spontanea* were promoted by it. The more important and influential the consequences were which might be supported by these observations, the more necessary it was to submit them to rigid criticism. The complete investigation of the germination of single seeds and their growth could alone remove the doubt, which necessarily increased with the general diffusion of the idea of a *generatio spontanea*, of the correctness of that observation; and this nobody had made. I at that time followed up these ideas with more careful observations than those of Micheli, and was so fortunate as not only to establish the fact, but also to discover the conditions under which the observation of the real germination of mould seeds may easily be repeated at pleasure in every forty-eight hours.

I made known these experiments in 1820, in a German notice in the *Regensburger Flora*, or Botanical Journal, part II. page 535, and more completely in a Latin paper (*De Mycetogenesi Epistola. Neesio ab Esenbeck scripsit Ehrenberg. Nova Acta Nat. Cur.*, vol. x.) addressed to the President of the Leopold's Academy of Bonn. I have there given figures of the seeds of fungi, their germination and their gradual development, to the completion and formation of fresh seeds; and the same experiments have been repeated by several others (see Fr. Nees Von Esenbeck in the *Flora* or Bot. Journal, 1820, page 531, and Schilling, in Kastner's *Archiv*, vol. x. p. 429. 1827. The latter gives the observation in 1827 as his own discovery). With this observation the tendence of the fungi and mould to a cyclical development was established, and the necessity of a *generatio primitiva* was removed as far from those as from other plants. These small bodies, which withdraw themselves from common view, entered into the series of the other greater natural bodies, so that the strangeness of their frequently enigmatical appearance may be referred to the requisite fineness of observation, and the insurmountable difficulty of such observation in open nature, whilst a piece of rotten wood and a single rotten pear, &c., as a soil for their growth, permit us to have before our eyes, in our room, the cycle of the development of these forms.

Continued investigations of the most minute organized beings have of late more and more confirmed me in the opinion that, not only in all these forms, besides the supposed *generatio spontanea*, a cyclical development may be ascertained by examination, but they even compel me to declare that all observations and experiments in support of the *generatio spontanea* are by no means sufficiently careful and faultless to produce conviction; and that the idea of a *generatio primitiva* of organic bodies, to have the value of an ascertained fact, must be proved afresh by more accurate observations.

2. *Intestinal Worms.*—The idea of the *generatio primitiva* being founded not on the fungi and moulds alone, but more particularly on the inexplicable origin of the intestinal worms and the infusoria, I at a subsequent period especially directed the whole of my attention to these forms. In the years 1820 to 1826, and in 1829, I collected, in my voyages in Africa, Western Asia, and Siberia, as many geographical observations as possible of all the smallest existing organisms; and by means of the great quantity of my unbiassed observations pursued for so many years under very different circumstances, I became more and more disinclined to the notion of the *generatio spontanea*, as I acquired a far clearer insight into the highly perfect organization of these so-called organic ultimate forms, molecules, or minutest organic beings, which has disproved the necessity of their primitive origin, and opposes to it possibilities and realities entirely different.

On examining the intestinal worms, I everywhere found the entire structure of almost all these animals so decidedly adapted to an oviparous propagation, that I should rather be led to ascribe the apparent anomaly and mysteriousness in their origin to their essential relation to the inner parts of living animal bodies to which they are limited, and the great difficulty of the direct observation of their cyclical development arising from that cause, than to an entirely peculiar power of nature, which is in action there alone where human investigations are excluded and the senses do not reach. Organs of copulation and production, clearly developed and never deficient, and the development of which surpasses for the most part those of other organic systems, plainly point in the intestinal worms to a predominant cyclical development, in the same manner as it is exhibited in the larger organisms, and make their *generatio primitiva* very improbable, for which indeed there is no other argument than the difficulty of observation. The occurrence of intestinal worms in the interior of organic bodies does not appear to me more remarkable or incomprehensible, considering the numerous frequently crude animal aliments beginning with the chyle and the milk, than the relative rarity of those parasitical organisms, considering their enormous predisposition to increase by eggs. We seldom find, indeed, animal bodies or human corpses without any worms, especially when we diligently look for them. We find them also as seldom in such abundance as we might expect from the apparent fecundity of these animals, which are not capable of any voluntary limit. There must therefore be great and insurmountable difficulties for the development of the hundreds and thousands of eggs which are often found in each individual of these parasites. I would therefore not object to the older opinion, that the eggs of intestinal worms are propelled by the circulation of the fluids into all parts of the body, but develop themselves there only where the particular conditions requisite for this purpose are favourable. The smaller diameter of the finest vessels through which they have to pass does not appear to me to present any important difficulty, because these, as we see in every inflammation, become easily and quickly expanded as soon as they are irritated; and these eggs may, as excretive bodies, like every body which is foreign to our organism, act in an irritative manner, and may be taken up by the embouchures of the absorbents and be propelled along with increased activity through them: that this is the case with mercury, pus, and other matters, has been already received as an observed fact *. It is even probable that the eggs of the Entozoa and their propulsion through the vascular system may be an important morbid matter hitherto overlooked, and which causes a part of the phænomena comprehended under the name scrofula. In bodies which are particularly favourable to the development of worms there must necessarily be an innumerable quantity of secreted eggs of those parasites, which, if they are not expelled by the intestinal canal or by the *primæ viæ*, must, as foreign bodies, produce disorders. If the absorption takes place entirely or for the most part in the lymphatics, it would occasion their general or sole influence upon that system. Obstructions in the lymphatics, but especially in their reticular tissue, the glands, which lead to local congestions of lymph, inflammations, and morbid appearances of various kinds, become in this manner very easy of comprehension; and these assuredly deserve the attention of medical science, not as speculations but as realities. Thousands of eggs of intestinal worms, whose existence in many bodies cannot be denied, must perish, as they are rarely developed in such great quantities, from the difficulty of their attaining the place and conditions favourable for their development; while only some, very often none, ever actually attain those conditions. This relative proportion of the number of intestinal worms and of their eggs to the

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* Müller (*Physiologie*, vol. i. p. 17) alleges that Ehrenberg endeavours to weaken the *generatio æquivoca* of intestinal worms, but proves nothing; and, I think, with reason; for, according to his view, the eggs are taken up by the lymphatic vessels and carried to all parts of the body. But how is that possible? They are evidently too large to enter into the lymphatics; and how can they circulate in those blood-vessels which are only 0·00025 of an inch in diameter, and so reach the products of secretion, such as milk, yolk, &c.? For this must be supposed, since intestinal worms have been found in the fœtus of mammalia and in hens' eggs, &c.—W. F.

organs of the larger animals is also found to exist. There are very often observed in animal dissections a small number of full-grown worms, filled with an innumerable quantity of eggs without any young in their proximity; and I was often astonished to find in the considerable number of my dissections of animal bodies (I have brought from Africa alone intestinal worms of 196 species of animals, all of which I have myself dissected, and of some from 40 to 50 individuals,) only a few alive, although these were completely filled with eggs. Thus from laborious observations this opinion has become more and more firmly fixed in my mind, that it is much more astonishing how the great fecundity of the Entozoa should be so limited by the living organs, than that it should be possible that living worms should inhabit them, and, considering their diffusion, escape observations which are generally superficial. The Epizoa present to the observer quite a different proportion, although these can for the most part be limited voluntarily by the animals. The circumstances favourable to their cyclical development in most cases overcome those which limit it; and a careful observer might follow with ease the formation and development of their innumerable eggs. It is not the small size of the Entozoa which forms the difficulty, but solely their inaccessible station in the interior of living animal bodies.

3. *Infusoria.*—Of an entirely different nature is the difficulty of observation in the Infusoria, the third strong hold of the *generatio æquivoca:* it lies in their minute size. Observations of Infusoria, which I pursued with great zeal and repeated on every occasion, showed me the necessity of a more definite determination of their forms, which I endeavoured to acquire by drawings and measurements of them. These severe and often-repeated investigations of individuals enabled me frequently to recognise the most decided traces of a higher internal organization than had previously been ascribed to them. In the year 1819 I had already observed that the motion of the zoological monads (*Monas pulvisculus*) was by no means a mere rolling effected by a change of the centre of gravity, as it was thought to be; but I perceived, from the throwing off of very minute particles of the dirty water, and from an apparent whirling at the anterior part of the animalcule, the presence of oarlike cilia, which at times even became visible. Some of these observations I made known in 1820, in an Appendix to a Memoir by my friend Friederich Nees von Esenbeck in the Regensburg Botanical Journal, part ii. p. 535. My friend and subsequently fellow-traveller Dr. Hemprich often witnessed my observations and experiments, and has also given, in his *Grundrisse der Naturgeschichte*, 1820, p. 289 to 291, a summary account of what I had ascertained at that time (see preface, p. viii.). I was not then myself desirous of making publicly known any of those observations, because I saw on the one hand that they were capable of being carried to much greater perfection, and on the other hand, I possessed at that time only a very incomplete thirty-shilling wooden com-

pound microscope from Nurenberg, which I had, according to my own views and wants, rendered more powerful, the same indeed with which I had already discovered the germination of the seeds of moulds, which however was far inferior to the ingenious microscopes of that time. Those observations appeared, however, to my friend to be so unquestionable, that he would not forego the opportunity of making known the principal points in his book.

From 1820 I made my observations in Africa with a microscope made by Hofmann of Leipzig, of the cost of about £6, which, with a greater magnifying power, gave a much better image; and from the year 1824 I used together with that an English microscope by Bleuler, which cost about £15, and the power of which was still higher. With these instruments I followed up the critical investigation of the *generatio primitiva* with increasing care; and the more exact my observations were, the colder I became towards the idea of an instantaneous coagulation of primitive substances into an organized being. In the whole series of years, during which I have sometimes for days together continued those observations, I *never, not even in a single instance,* saw the subitaneous origination from slime, cellular tissue of plants, &c., of those minute organic bodies to me so well known; still much less had I observed the gradual development of elementary outlines that had suddenly originated, of Entomostrati and other larger animalcules; which was an extraordinary delusion of M. Fray, who took the *exuviæ* and fragments of minute dead animals for sketches and rudiments of new generations*.

My observations pursued in Africa convinced me more and more that the origin of the most minute organized beings must also be cyclical; for although circumstances did not enable me there to bring to a completion my investigations into the structure of the Infusoria, yet I found always a manifest repetition of forms similar to those which I had determined by drawings and measurements, and not at all that unlimited variation of them which we should expect from the idea of a metamorphosis of destroyed organic substances into undetermined elementary forms of life. Thus the basis on which my observations proceeded continually became firmer. Corti's discovery, that the eggs of some Infusoria (*Brachionus*) burst when the young ones creep out, and leave

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* *Essai sur l'Origine des Corps organisés et inorganisés, par* Fray. *Paris,* 1817; *p.* 71. "*J'ai vu des monocles, des polypes, des vers et d'autres animaux, qui n'étaient encore qu'ébauchés; la forme extérieure était jetée, mais l'intérieur n'avait pas reçu tous les globules actifs qui devaient le constituer. Ces esquisses étaient encore immobiles.*" This puts one in mind of the celebrated ancient Egyptian frogs, which were said to come into existence after the inundation, and to hop about with only their fore part developed, while their hinder part was still mud. Times are altered; for he that sees such frogs at the present time, does not, even in Egypt, stand with trembling awe at a distance from them, but lays hold of them, and finds that under the mud of the hinder part something more than outlines are hidden. So it was with me at the Nile; for I came there indeed as a sceptic.

behind an empty egg-shell, a true chorion, I had previously made without a knowledge of the original observer; and I had even remarked that the eggs were suspended by delicate threads, by means of which they were carried about by the animalcule, as in the crabs. I noticed also the entire intestinal canal, by means of the whirling of the mouth aperture and the secretions of the anal aperture; and, in the wheel animalcules (*Rotatoria*), when it was completely filled, its whole course. Some time after I also occasionally perceived traces of beautiful red-coloured eyes in the wheel animalcules and *Brachionæ;* and recognised more and more clearly a masticating apparatus in all the forms which I examined, and free muscles in some. In 1827 my views respecting the wheel animalcules had made a progress to the extent which is represented in the third and sixth plates of the Phytozoa of my *Symbolæ Physicæ.* The same decade of engravings I laid before the Association of Naturalists in Berlin in 1828, but without text. I had used Bory de St. Vincent's nomenclature in these plates, although I did not approve it, solely because I considered the innovations required by my observations as a useless increase of synonyms until they had been brought to maturity.

The reputation of Chevalier's microscope, from Selligue's intimation that at a cheaper rate it would produce greater effects than those in general use, induced me to purchase one in 1828; and with it I endeavoured to arrive one step nearer to that physiological goal which I had so unremittingly pursued during ten years. A review of the Infusoria showed me not only that my earlier observations were no delusions, but confirmed them, and increased my conviction of their evidently high organization; I convinced myself especially that the supposed traces of eyes in some wheel-infusoria, *Rotifer* and *Brachionus*, were distinct and constant. Being now accustomed to this new instrument, I made great use of it on the journey to Siberia which I made in 1829 with Alexander von Humboldt: the extensive series of accurate observations, drawings, and measurements made during this journey, allowed me, as soon as I returned to Berlin, to institute, with the greatest advantage, comparisons with the observations formerly made in Leipzig, Berlin, Africa, and Arabia: and as I had now no longer any fear that the advocates and invstigators of the *generatio æquivoca* might be in possession of better instruments; having, moreover, already possessed myself of a most astonishing series of details of structure, I became gradually convinced of the probability of an universal high organization, even in the infusoria, and so-called elementary molecules; of their cyclical development, and of the numerous errors of earlier observers. I found especially the great incongruity between the statements respecting generation and structure, made by those who pretended to have actually seen the *generatio primitiva,* and who stated that they had observed the spontaneous origin of organic bodies from primitive substances, or its gradual formation, without however having noticed their complex internal structure; whilst at the same time, I myself, who for a series of years had acquired a progressively deeper insight into the organization of these minute forms, which were said to be developed, destitute of organs, or imperfect, could never get a sight of their spontaneous or gradual origin from molecules, slime, vegetable tissue, &c.

A comparison of my observations on the bell-animalcule (*Vorticella Convallaria* and other species of this genus), which had been made under geographical circumstances the most various, greatly confirmed my impressions; and the repetition, with redoubled care, of my investigations of their gradual individual change, removed all doubts in regard to a whole series of beings of various kinds, in which I discovered a determinate cyclus of forms, deriving their origin from one another. These data, of which I became convinced, in beings so minute and so simple in appearance, incited me to direct my observations, with increasing care, to this point, and gave me a certain anticipation of much more interesting results near at hand. High organization, and cyclical development of the molecules, were to me clear truths, floating in my imagination, and capable of more substantial proofs: my only search was for the means to produce them. Fortunate was the thought which brought to my recollection the coloured nutritive substances, already often tried by me without success. Confident of the result, I put various colouring substances into the water containing the infusoria, and awaited their reception into their organs of nutrition. The first experiments with common water-colours failed, although I had selected many different colouring matters; my conviction however of a better result was so strong, that I no longer attributed its want of success to the organization of the animalcules, but to unsuitable colouring matters. Other experiments also proved unsuccessful. One day however I remarked, in experimenting, a whitish sediment at the bottom of the small glass plate on which I had mixed water colours with the water containing the infusoria; and as in general the colours of the shops are mixed with white lead, I made choice of some pure colours, and such as I supposed to be least disagreeing with animal organization; such I considered indigo, carmine, and sap-green, as they are all of purely organic origin. With these I began my experiments anew, and here also the clew was found; all infusoria, even the smallest, soon filled themselves with the colouring matter. The opacity of the water caused by the colour, enabled me to distinguish much more clearly than the opacity caused by mud, which I had made use of in my former experiments on the cilia, not only the presence of the cilia, but also any separate part of the body to which the vortex caused by the cilia carried the nutritive particles, and where they were received into the interior of the body.

From the transparency of all these animalcules, I could see very plainly in their interior, either a developed simple intestinal canal, or clearly defined, vesicular, coloured pouches similar to stomachs. In those animalcules which had fully charged themselves, I could also as plainly perceive in other parts of the body, particularly the hinder parts, secretions of the superfluous matters through a separate anal aperture. These traces of organization in the most minute beings were now no longer useless indications of organs, but evidently real organs performing their functions—cilia, mouth, intestine, anal aperture. This result, long sought for indeed, and as it were extorted, to a degree of clearness beyond expectation, urged me immediately and eagerly to employ the same means for the investigation of all the species of infusoria found in my neighbourhood, which I had before observed in various ways, and of which I possessed drawings, in order to elucidate their organs of nutrition, and to delineate and determine their form; whilst at the same time it was evident that the knowledge of their organization was by no means completed by the discovery of the organs already mentioned. The eggs, muscles, and eyes of the wheel-animalcules rendered probable the existence of similar systems in the others also; and hence I not only most carefully reviewed in every manner all the species occurring near Berlin, as to organs of nutrition, but I also endeavoured to throw more light on the whole organization of these minute beings; thus I ascertained with much greater certainty the true relation of the free muscles of movement, the whole course of the intestine, the course and form of the female and male generative organs, the great frequency of the beautiful red eye-points, regular branching traces of vessels, and besides, little glandular bodies and fibres in the interior, which are not at all opposed to the character of nerves. In the throat I distinguished teeth so evident, that these alone, had they been sooner discovered, would have sufficed to render credible the perfection of the rest of the organization, according to the type of larger animals. In the smallest infusoria, which up to that time had always been considered as homogeneous globules, I recognised not only an apparent internal belly, and at times an apparent intestinal canal, with mouth and anal aperture, but also, at least in one genus (*Euglena*), red points similar to eyes.

Of great importance however was, amongst other acquisitions, the light thrown on their organs of reproduction. I perceived a very fine granular reticulate matter, filling the intervening spaces of the intestinal canal, to be secreted through the anal aperture, which characterized itself plainly as a laying of eggs: the animalcules became at the same time smaller, plicated, and edged, sometimes changed their form very remarkably, but still actively continued to swim. Of a similar character appeared to be the old observation, often repeated by me, of a spontaneous partial solution of the smaller infusoria into fine granules, their living motions all the while continuing; and I searched for analogies in the *Coccus*, in which the death of the mother takes place before the young ones break the egg, and in the tape-worms (*Tænia*), the hinder parts of whose body separate *after*, or even *at* the production, while the anterior part continues to live. Finally, I established and found in these minute infusoria a fourfold mode of reproduction: by eggs, gemmation, transverse separation, longitudinal separation; while in the wheel-animalcules, only eggs, or living young from eggs, are produced. The smallest Monads observed by me, which yet exhibited internal organs of nutrition evidently filled, were $\frac{1}{2000}$ lin. in diameter. These measurements are made with a glass-micrometer, by Dollond, which indicates to the $\frac{1}{10000}$ of an inch. The granules of the ovarium of those minute infusoria which were observed to produce young, were in their relative magnitude to the mother animal in the proportion of 40 to 1, or as 80 to 1. The eggs of the wheel-animalcules were in general as 3 or 4 to 1.

In this way, and by the means which I have stated, I was enabled to establish at once the doctrine of the infusorial animalcules more completely and accurately than it had been up to that time; and the easily visible colouring of the nutritive organs, from the transparency of these bodies, might well induce others to participate in the results obtained. This elucidation of the infusorial world I gave in an academical memoir read in Berlin, and have circulated since 1830 about one hundred copies by the booksellers. The separate copies have the distinct title *Organisation, Systematik und geographisches Verhältniss der Infusionsthierchen, von C. G. Ehrenberg;* Berlin, 1830. In this folio work, which is accompanied by eight copper plates, I separated the so-called infusorial animalcules, according to their organization, into two quite distinct classes, one of which is distinguished by the great number of ventral cells, and to which on that account I have given the name of *ventral animalcules* or *many-bellied infusoria* (*Polygastrica*); the other, which is distinguished by wheel organs and a simple intestine, I have called *wheel-animalcules* (*Rotatoria*). The whole of the results of my observations which I have there given are included in the following fifteen positions:

1. All infusoria are organized and in part, probably all, highly organized animals.

2. The infusoria form two quite natural classes according to their structure; can be separated scientifically according to their structure; and permit no identification of their forms with greater animals, however similar they often may appear.

3. The existence of infusoria in the four quarters of the globe and in the sea has been proved; and they form the chief number, perhaps the chief mass, of animal organisms endued with life on the earth.

4. Some species are the same in the most remote parts of the world.

5. The geographical diffusion of infusoria on the earth follows the already known laws of other natural bodies. Southwards there are more varying forms, supplying the place of those of other parts of the globe, than westward and eastward, but they are nowhere wanting; the influence of difference of climate is not confined to the larger kinds.

6. The salt water of the lakes of the Siberian steppes does not exhibit any peculiar infusorial forms varying in any remarkable manner.

7. The water of the sea supports other and larger forms than the river waters; many however are identical; in none of those known does the magnitude exceed a line.

8. In the atmospheric vapour which is precipitated as rain and dew, I have never been able to observe, and I believe no one else *with certainty*, living infusoria. (I have related some recent experiments of mine on this subject.)

9. In the deep subterranean places where atmospheric air, but scarcely a minimum of reflected light, finds entrance, are found families of the same infusoria as at the surface.

10. Direct observations in support of the *generatio primitiva* have all, as it now appears, been deficient in the requisite exactness. Those same observers who supposed that they had seen the spontaneous origin of minute organized beings from primitive matters, have quite overlooked the very complicated structure of these organisms. Here a great error cannot be doubted, and the delusion is evident. This is perhaps less to be ascribed to the fault of the observer's precipitancy than to the weak powers of the instruments employed, or the want of practice in their use. Observations on the origin of crustaceous animals and insects from primitive substances are the echos of the olden time, when caterpillars grew from the leaves.

11. The idea that man was dependent, even if only in part, upon the will of those infusoria of which he was composed, is proved to be absurd, from the fact that the infusoria must seek their food, lay eggs, and never combine into a fixed and growing state*. (Some it is true unite at times into heaps, but these separate again into individuals.)

12. The development of all those infusorial forms which I have been able to observe is cyclical, quite certain, but at times abounding in varying forms, and from that cause delusive and demanding careful observation.

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* This may appear an exaggeration, but we need only refer the reader to Dr. Carus's paper *on the Kingdoms of Nature*, etc. in page 246 of the present volume, where he says,

"If we now reflect likewise how in the infusoria and Priestley's matter, the rudiments of the animal kingdom appear as so many animated globuli, we shall thence perceive that the largest animal bodies themselves must be viewed as an innumerable aggregate of infusoria, but at the same time united into a living whole." W. F.

13. The results of my observations call to mind the old physiological proverb, *Omne vivum ex ovo.* In my observations pursued with so much zeal for twelve years, I never witnessed the spontaneous origin of one infusorium from slime or vegetable tissue; but have often enough seen the laying of eggs, and the young come out of the larger eggs. Supported by such experience, I am of opinion that these animals are never formed by *generatio primitiva*, but originate from eggs. Whether then the eggs dispersed about are only in part the product of laying, or in part the product of a *generatio primitiva*, is a question which is not quite ripe for determination*.

14. The active motions and contractions in plants and their parts, especially in *Algæ*, ought not to give rise to the supposition of an animal nature, even when they are called infusorial or animal motions. Internal nutritive organs, and a definite oral aperture for the reception of solid substances, which may be demonstrated, distinguish the apparently most simple animal from plants. I have never seen in my numerous experiments the motive algæ seeds take up the smallest quantity of solid nutriment; and thus the fruit-strewing alga may be distinguished from the monads which swarm round it in the same manner as the tree from the bird.

15. Finally, I call attention to this fact, that experience displays an unfathomableness of *organic* creations referred to the smallest portion of space, in like manner as the heavenly bodies are to the largest portion, the preternatural limits of both requiring optical assistance. Hypotheses may be started even so far as to the existence of primitive substances; it cannot yet be brought before our experience. The milky way of the smallest organization passes through the genera *Monas*, *Vibrio*, *Bacterium*, and *Bodo.*

In a more recent memoir read in 1831, which will appear in a few days and the finished engravings for which I have already by me, I have given the following most important additions which I have lately made on the same subject. Hitherto I had only been able to observe in the ventral animalcules (*Polygastrica*) the motive and nutritive organs, and the ovarium. I had found traces of eyes only in one genus, namely in the genus *Euglena.* I have lately found them more often in the same class, so that I can now name seven genera possessing them which contain sixteen species. Among these forms are some monads which are only $\frac{1}{192}$ lin. in diameter. From this discovery then the traces of an isolated nervous system are demonstrated down as far as the monads.

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* This latter sentence, the restricting of the *generatio primitiva* to the formation of eggs, appears to me to be modified by my subsequent observations of the development and astonishing productiveness of the infusoria; for it now appears to be a subject of much greater wonder why we do not find more infusorial eggs in water and everywhere, since there is cause for the formation of an innumerable quantity in the common way.

I have also made a first attempt to divide the two classes of infusoria in a greater degree by their internal organs. The nutritive system gives in each of the two classes only four distinctions. According to this, the *Polygastrica* fall into *Anentera* (possessing no intestine), *Cyclocœla* (with intestines forming a circle), *Orthocœla* (the intestines straight), and *Campylocœla* (the intestines crooked). The *Rotatoria* fall into *Trachelogastrica* (long-throated without a belly), *Cœlogastrica* (long intestine, without belly and with short throat), *Gasterodela* (with bellies), *Trachelocystica* (with bladders). The intestine of the latter is very peculiar.

The *Rotatoria* alone, according to subsequent observations, may be divided according to their masticatory organs, and fall then into three groups: *Agomphia* (without any teeth), of which there are but few; *Gymnogomphia* (teeth not fastened to anything),—this contains the greatest mass; *Desmogomphia* (teeth connected). Those with unconnected teeth (*Gymnogomphia*) fall into two great natural and equal divisions, viz. *Monogomphia* (one-toothed) with a tooth in each jaw, and *Polygomphia* (many-toothed). Those with connected teeth (*Desmogomphia*), whose teeth are not free but fixed on a cartilaginous base, fall also into two natural divisions, viz. *Zygogomphia*, with teeth placed in pairs, and *Lochogomphia*, with teeth in rows; so that the following *schema* may be made:

Agomphia.	Gymnogomphia.		Desmogomphia.	
	Monogomphia.	Polygomphia.	Zygogomphia.	Lochogomphia.
I.	II.	III.	IV.	V.

I have there stated my opinion as to the employment of these organic differences for the purpose of systematic arrangement.

I have also in the same paper recorded my observations on the development and production of individuals in the infusoria, particularly in regard to time. The result of those observations I consider to be one of the most important of the whole series in this memoir. A single individual of *Hydotina senta*, one of the Rotatoria which I had described at length, and given an engraving of in my first memoir, I kept separate for the space of eighteen days, during which time I observed it with the greatest care, and as it was already developed when I isolated it, and did not die of age but accidentally, we may fix the duration of life at above twenty days. An individual of this kind is, however, capable in every twenty-four to thirty hours of a *quadruple* increase when the circumstances are favourable; in that space of time it is able to develop four eggs, from the first activity of the ovarium to the creeping out of the young. This quadruple increase, if there is no hindrance, and if the individual animalcule lays forty eggs in ten days, gives in the space of one day in the tenth power (that is on the tenth day) a million individuals from *one* mother, on the eleventh day four millions, on the twelfth sixteen millions, and so forth. Although the fecundity of the Rotatoria is the greatest which has ever yet been observed in nature, and surpasses by far that of insects, yet it does not by a vast deal reach that of the polygastrical infusoria. In *Paramæcium Aurelia*, which is about $\frac{1}{12}$ of a lin. in size, and which I observed with certainty during several days of a long duration of life, I have seen within twenty-four hours, by simple horizontal division, the *octuple increase of one* individual, which would allow the possibility of the double of that increase. As however these animals increase not only by separation but also by eggs, and secrete these eggs not singly but in masses, and besides in addition to these form buds, we have such a possible immense increase of a single individual in forty-eight hours, that we may leave off counting and speak of innumerability*.

Who now under such circumstances can wonder if within the space of two or three days fluids swarm with such animals? Is it not more natural to wonder how it is that often it does not happen? We no longer now require a *generatio æquivoca* to explain these phænomena; they belong to those within the reach of experience and observation; and where anything astonishing of this kind may occur, the observer must take great care that the fault of superficialness be not laid to him. If therefore I supposed in my first paper that the *generatio æquivoca* of infusoria might still be ascribed to their eggs, it now appears to me from observations on the development and increase of individuals, that the necessity of such an hypothesis, and even its probability, must vanish. I now indeed believe that the *generatio primitiva* may *as an ever-existing subject of experience* have undergone its mortal combat. Upon this subject however I cannot undertake to solve all the problematic points relative to it, of which there are many and important ones, particularly in relation to geology, as they are generally *subjective*, and but rarely *objective*; but I wish here to urge the consideration of the *indefinitely small*, as a main position for all branches of natural history, from which perhaps at a subsequent period I may also develop my more special views.

II. *On Atoms and Molecules as subjects of experience.*

Atomic philosophers have of late years, partly by their ingenious theories for the explanation of the phænomena of light, and partly by their ingenious as well as fertile atomic calculations of the doctrine of porportions in chemistry, obtained an undeniable practical superiority over dynamists, how much greater soever the satisfaction and recompense of the latter may seem to themselves to be; and hence it has happened that in the doctrine of the smallest particles of bodies we

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* This rapidity and great capability of increase of the infusoria might also be worthy the attention of analysing chemists, as they might quickly produce a great influence on organic substances, namely on some colouring substances. Boiling heat, or a few drops of alcohol, precipitate the infusoria, and they may then be removed as slime with certainty by filtration. It is necessary to operate quickly.

have become rather too bold. Not content with regarding atoms as ideal unities, or as indefinitely small magnitudes, we have endeavoured to find for individual atoms or for certain minute groups of them an expression of a proximate finity, and even to fix its magnitude and to determine it by numbers. Indeed it appears that but little is wanting in our days to induce bold theorists to attack in good earnest the material primitive particles of bodies, to clutch them fast, and to build up with them even to organic structures, and thus to sport with them.

Newton indeed thought he might assume the elementary particles of colours in bodies of a certain magnitude and perceptible to sense. He says, p. 64, Prop. vii., "For if those instruments (microscopes) are or can be so far improved as with sufficient distinctness to represent objects five or six hundred times bigger that at a foot distance they appear to our naked eye, I should hope that we might be able to discover some of the greatest of those corpuscles. And by one that would magnify three or four thousand times, perhaps they might all be discovered, but those which produce blackness." If we now suppose that Newton had rightly estimated the natural acuteness of the vision of the human eye, his elementary particles for the red colour must not amount to less than $\frac{1}{36000}$ of a line in diameter, as will be seen lower down; and between this magnitude and that of $\frac{1}{144000}$ all the elements of colours except black would be found. It is however probable that Newton supposed the power of vision of the human eye to be less, and therefore the size of the elementary particles to be much greater. However we must here not forget, as Herschel has already remarked in his Optics, that Newton distinguished the elementary particles of colours from atoms, as later philosophers have also done, although he does not express himself to that effect. In that passage Newton does not speak of atoms but of colouring particles. (*Newton's Opticks* (1704), book ii. part iii. Prop. vii. p. 64.

The small magnitudes which have been employed for the explanation of the phænomena of light in the undulatory theory give a great definiteness to the calculation; they can however only be regarded as hypothetical and not as real demonstrated magnitudes, as the whole theory, even though it possesses great probability, is in want of full confirmation. The smallest lengths of a wave of light which can be shown by an exact calculation, do not amount to more than the $\frac{1}{100000}$ of an inch, or about $\frac{1}{8000}$ of a line. Now as the particles of æther must be considerably smaller than their undulations, there is in that number a limit, arbitrary indeed, but yet determinate, for its maximum, which gives an expression for its smallness. If from the imponderability of very great condensed masses of light or of æther we were to form a conclusion as to the smallness of the elementary corpuscles as ponderable objects, we should be obliged to place the limits of that maximum at a still much greater distance. All these however, even when they admit of a definite numerical expression, are still hypothetical magnitudes.

The phænomena of colours between glasses almost in contact with each other admit also of an inference as to the magnitudes of the so-called elementary particles of colours. The smallest space which gives the white colour was already fixed by Newton at $\frac{1}{178000}$ of an inch, which is rather more than $\frac{1}{14000}$ of a line; and Hauy has reckoned, from the different refractions of light of mica, that a plate of mica which would produce the same effect as that of a stratum of air must be $\frac{1}{400000}$ of a millimetre thick, or $\frac{1}{900000}$ of a line.

Mr. Robert Brown made several admeasurements of inorganic solid bodies, and also of organic ones, in the years 1827 and 1829, and fixed the size of the smallest particles which could be observed, and which he himself saw in spontaneous motion and of round form, at $\frac{1}{20000}$ to $\frac{1}{30000}$ of an inch, or $\frac{1}{2000}$ to $\frac{1}{2500}$ of a line in diameter. (*Brief Account of Microscopical Observations, by R. Brown*, 1828, and *Additional Remarks on active Molecules, by R. Brown*, 1829, p. 3.*)

Sir J.F. W. Herschel says, in his Optics, 1829, p. 680, that he had seen bodies which were magnified by an Amici's microscope to 3000 times their diameter, from which however we were not at all to suppose that the object even approached to its solution into atoms.

M. Dumas the chemist has however given to elementary organic particles very considerable magnitudes. In the year 1825 he taught, from his own observations, that with a good microscope the elementary globules of dead organic masses might be seen and counted; that they formed, by means of simple combination and an augmentation of the mass by increasing numbers, living bodies, becoming gradually larger and more organized, the first forms of which were infusoria, and which might again be divided into the elementary parts by means of an electric shock, by which they took a strawberry-like form (*un aspect framboisé*). (*Diction. Class d'Hist. Natur.*, art. GENERATION, p. 195.)

One of the editors of the *Annales des Sciences Naturelles*, who does not give his name, in tom. v. p. 80, 1825, fixes the magnitude of the elementary particles of all organic substances at $\frac{1}{300}$ of a millimetre, or $\frac{1}{676}$ of a line in diameter. In the same work, p. 81, the author thinks that, in accordance with the present state of chemistry, it is possible by synthetical means to prepare an artificial organic matter; and says, "Could we by these means obtain infusorial animalcules, Bonnet's theory of reproduction would be overthrown"!

There appeared in Kastner's *Archiv f. Naturlehre*, xii. p. 348, 1827, an express chemico-microscopical exposition by M. Kœlle. He says, "Zymom consists of microscopical globules, and with glyadin forms gluten" (p. 350). "Zymom is that matter from which, by a concurrence of favourable circumstances, originate the lowest forms of

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* Phil. Mag. and Annals, vol. iv. p. 161, and vi. p. 161.

organic nature" (p. 352). "Globules of milk and blood are Zymom; gelatine, caseum, starch, sugar, &c., contain Zymom" (p. 350). "Silica first takes a vegetable formation, and from the Zymom formed by this process originates further animal life" (p. 358). "Vegetable substance can be immediately changed into an infusorium" (p. 360). "From Zymom may be produced, when favourable conditions concur, infusoria of this or that form" (p. 358). "The first infusorium, the lowest animal creature, is a living Zymom globule" (p. 358). "Zymom is in a certain relation an egg" (p. 360). "The yolk of an egg consists merely of Zymom combined with slime" (p. 357). "This is no hypothesis but a fact" (p. 361)!

That the commencement of many organisms is an action of putrefaction or of fermentation, and therefore a chemical process, is a very ancient opinion, and could not fail to be revived in a refined form. M. Gruithuisen has in the eighth volume of Gehlen's *Journal der Physik*, 1809, p. 519, characterized the formation of the smallest organisms as a peculiar act of fermentation; and enumerates, together with the vinous and acetous fermentation, the infusorial fermentation which forms organisms. In days of yore Autochthones might have been thought to originate in this manner; afterwards fermentation was left for insects and weeds; but since their manner of living and of reproduction has been better investigated, such an origin is no longer found to be either necessary or admissible with regard to insects or the larger plants. It was then thrown upon the fungi and infusoria, on account of the great difficulty of observing them; from which however, in accordance with what has been stated, it must now also be rejected.

M. Berzelius, who had to treat on the same subject in his Classical Manual of Chemistry, but who does not offer any observations of his own, adheres to the data given by other observers, that dead organic matter when moistened with water creates infusoria; and he finds no improbability in Professor Hornschuch's idea that the *prima germina rerum*, which he conceives to be the infusoria, might develop by various external influences into other very different bodies. He has however followed in the doctrine of organic atoms the representations of Dumas and Milne Edwards; and those organic atoms which by his doctrine of chemical proportions have become so eminently useful and of such extensive influence, are, in proportion to the imaginative capacity for abstraction of various minds, unities more or less ideal, whose use in theory seems destined for a long time to come to be of the most important value in the practical development of chemistry. (*Animal Chemistry* (German translation by Wöhler, p. 6), and *Chemistry*, vol. iii. pp. 31 and 179.)

Very recently the well known physicist M. Munke of Heidelberg has himself made several observations with one of Plösl's microscopes, after the manner of the earlier philosophers, and in truth not very profound, on the nature of the organic bodies in infusions; and he imagines he has arrived at this result, that a transition takes place from vegetable to animal life, and *vice versa*. (*Isis*, 1831, p. 1083.)

The examples mentioned in manuals of physics of the great divisibility and ductility of different bodies are for the most part small magnitudes merely in appearance. A gold leaf, thin as it may appear, is about $\frac{1}{1000}$ to $\frac{1}{2000}$ of a line in thickness.

It has not here been my intention to give a collection of the opinions of natural philosophers and chemists respecting atoms, but to call to memory only a few of those statements as to which I am best informed and most certain, of the magnitudes of the smallest particles of bodies which have been observed and calculated, in order to add to them the results of more recent observations which I am now making known, and to lay down a scale for them. The most recent theoretical statements do not give any very great degree of minuteness to the ultimate particles of bodies; the observations of Mr. R. Brown very nearly approximate to those statements.

The common opinion that infusoria or mould could be made by pouring water on dead organic matter I must pronounce to be completely contradicted by the whole series of my observations. It is true the phænomenon is very deceptive; but if we observe carefully, there appear, even with the very same treatment, at one time some kinds of infusorial forms, and at another time others; and I have never had it in my power to produce certain forms with certain infusions, although this is found stated in all manuals as true, and succeeded (by their own account) with all earlier observers. There are however, according to the results which I have obtained, certain common forms, which are most generally diffused, the eggs or individuals of which may be present in all liquids, even in some, perhaps only the noxious, parts of plants, and of which at times the one form, at times another, may rapidly increase according to the eggs or single individuals which were present in the water, or had been introduced into it. M. Blainville in the *Dict. des Sci. Naturelles*, art. Zoophytes, also from experience pronounces against the *generatio æquivoca* in infusions: "I have often taken great pains without any success to produce any kind of organic body in small glasses by spontaneous development, although other glasses by its side containing the same water were soon filled with them. Besides the discovery of this error respecting infusorial fermentation, which not only proves false in fact, as is also manifest from the development of the forms, my investigations respecting the minutest organic particles have led me to recognise the following minute magnitudes as really existing and perceptible to the senses."

I could plainly distinguish with a microscope magnifying nearly 800 times zoological monads or animal organisms, which were filled by the above-mentioned process with colouring nutritive substances, and which possessed voluntary motions, but the entire and greatest diameter of whose body only amounted to the $\frac{1}{1500}$ or $\frac{1}{2000}$ of a Parisian line*. The smallest animal form, to which I have given the name of *Monas Termo,* is the same being as that which Otto Fr. Müller has delineated among the infusoria. I could perceive in the greatest individuals of this animal form as many as six, and in the smallest as many as four, internal sacs coloured by blue indigo, which at times did not occupy quite half of the dimension of the animal. Such a sac therefore of the *Monas Termo,* if the animalcule measures $\frac{1}{1500}$ of a line, and if we suppose only four sacs occupying the half of it (therefore not one of the smallest), is $\frac{1}{12000}$ of a lin. in size, which is five times smaller than the minutest particles observed by Brown. At the upper part of this animal is perceived, as in all the monads, a powerful pushing aside of particles still smaller than themselves, when these come near to them; and it is therefore probable that they have a fringe of ten to twenty cilia near the anterior part of the mouth aperture, as in *Monas Pulvisculus,* and especially in the other still larger monads. Further, if even we suppose the single colouring particles with which the bellies are gradually filled not to be numerous, it would be against all probability not to think that they were filled by several particles. Let us however only suppose each sac to be filled by three colouring atoms,—which from the roundness made perceptible by the motion communicated to them we may well admit,—this alone affords a proof of the existence of material colouring particles of red and dark blue moving freely in water, which measure $\frac{1}{36000}$ of a line, or $\frac{1}{432000}$ of an inch, in diameter; and calculating these objects from the smallest of the animalcules, which by actual observation were found to be $\frac{1}{2000}$ of a line in size, and which sometimes contained four coloured points in the hinder part of the body, these particles, which cannot be distinguished individually by the eye with a magnifying power of 800, but yet are to be recognised as corporeal, would amount to $\frac{1}{48000}$ of a line, or $\frac{1}{576000}$ of an inch, which exceeds the molecules of Brown nearly twenty times in smallness.

The above-mentioned transparent cilia about the mouth of these monads (perceivable only by their action) may also approach in fineness those just cited; for if they were not less than $\frac{1}{38400}$ of a line, or the multiple of forty-eight, with my magnifying power of 800, there would be no optical reason, except their transparency, why I should not see them with that power, as will be evident from the sequel of the memoir. I shall moreover direct attention to the fineness of other parts of these *organic living* beings. The smaller monad-bellies are seen isolated in

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* I have already mentioned that I make use of a glass micrometer which measures $\frac{1}{10000}$ of an inch.

the body and with sharp outlines. In larger infusoria which are $\frac{1}{48}$ of a line or more in diameter, these internal receptacles are recognised as evident membranaceous sacs, which often make their appearance isolated when the animalcule is pressed or when it divides itself, and which have been supposed to be separate infusoria, internal monads. From this fact of the reception of nutritive substances by the smallest monads, we have no reason to admit any other office for the organs of nutrition. The sharply-outlined coloured points in the interior of the body of monads are to be regarded as small filled membranaceous vesicles or bellies. Now in the larger infusoria of similar structure we can discern, when two such bellies touch one another, that the thickness of the ventral partition is in comparison with the diameter of the belly extremely thin; that the former is seldom perceptible; and that the membrane forms around the contents a mere mathematical spherical superficies. Scarcely any one therefore would suppose a greater proportion than 20 to 1 in the smallest. Granting however the thickness of the partition to be only $\frac{1}{10}$ of the diameter of the belly, this would amount to $\frac{1}{160000}$ of a line, or $\frac{1}{1920000}$ of an inch, in individuals of *Monas Termo* $\frac{1}{2000}$ of a line in size, in which the bellies measure but $\frac{1}{8}$ of the whole length of the body, and are therefore $\frac{1}{16000}$ of a line in diameter;—and since there is reason to look for vessels in these partitions, this therefore places the organic atoms at a distance which I must, since they are purely hypothetical, pass over, and leave as a subject of direct subsequent inquiry.

There are still more powerful reasons for the probability of many much smaller magnitudes. According to my observations mentioned in one of the foregoing pages, there is in the body of the polygastrical infusoria a substance finely granular and reticular,—either in appearance from the impressions of the numerous small bellies, or really so,—which surrounds the whole of the intestinal canal and the masticating apparatus, and is secreted partly upon the solution of the individual forms, partly by the anal aperture, without any prejudice to the continuation of their being*. This perceptible substance I have taken to be the ovarium: the granules of this ovarium in *Kolpoda Cucullus* are to the mother animalcule as 40 to 1, others as 80 to 1; and they appear to grow finer with the decreasing size of the body, being no longer of themselves visible. Now it is probable that only their transparency and the weak power of the microscope hinder us from finding such an ovarium in the monads in every other respect so similar. We must not however overlook that there may be young monads still inclosed in the egg, or just come out from it, the diameter of the whole length of whose body would be but the $\frac{1}{60000}$ to $\frac{1}{80000}$ of a line; these may have bellies which then, in like proportion, would be in diameter $\frac{1}{480000}$ to

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* See supra, p. 563.

$\frac{1}{640000}$ of a line; the partitions of these monad-bellies will be but $\frac{1}{4800000}$ to $\frac{1}{6400000}$ of a French line in diameter, &c. I have besides seen rudiments of eyes in the monads to which I have given the name of *Microglena monadina*, and which are $\frac{1}{192}$ of a line in diameter. These often fire-red eye-points appear in larger infusoria to be a fine granular red pigment, but whose granules, perhaps first covered by many finer pigment granules, are small lenses, &c.; and although I will not lay particular stress on the fineness of these parts, yet the existence of eyes, however rudimentary they may be considered, allows of the possibility that even in the smallest species they may be present, and tends to lead us to a not so proximate finity of organic molecules; it may even give a useful hint for considerations on the elements of colours* and the theory of light.

Finally I must not pass over in silence a direct observation which has confirmed me in the opinion that the small organic, apparently hypothetical magnitudes really deserve great consideration. By the kindness of Professor Enslen, who in the park near Berlin has a peculiar contrivance for the public use of a solar microscope, I have made with him several experiments. By observing *Monas atomus* well filled with indigo, I discerned among them wandering shadows of smaller monads, which could not by a vast deal reach to $\frac{1}{2000}$ of a line, and which I could not at all discern in the same water with the most powerful magnifier of Chevallier's microscope: perhaps their transparency might be one reason. Whether these animalcules then be the brood of *Monas atomus*, or various species of still unknown infusoria, it follows from the observation, that $\frac{1}{2000}$ of a line is not at all the limit of organized beings for observation. It was on this account that I called the smallest monad, which increases so plentifully in animal solutions, and which resembles the *Monas Termo*, page 94 of my memoir of 1830, the Twilight-Monad; because from this point a new sytem of organized beings may easily be opened by means of increased power of vision.

Let not these calculations be disregarded as appearing to be playful; they are so far in earnest that they are founded on the contemplation of nature, and are not to be considered as a groundless speculation.

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* In regard to the phænomena of colours in high achromatic magnifying powers, I will just mention by the way that in some very small beautiful green-coloured globular infusoria from $\frac{1}{120}$ to $\frac{1}{200}$ of a line in size, and especially in *Microglena volvocina*, at a power of 400 or 800 I always see a fiery red ring round the animalcule. This ring is evidently an optical spectrum; but how to be explained? Perhaps the animalcule is thickly covered with fine hairs; and this hairiness causes perhaps, by refraction of light or play of colours, this phænomenon, which is only apparent in the periphery. Other similar beautiful green animalcules never exhibit a red ring; moreover this red is quite similar to the yellowish beautiful red which the pigment of the eyes in the Rotatoria and Polygastrica exhibit.

They plainly demonstrate an unfathomableness of organic life in the direction of the smallest conceivable space; and if the word infinity be too much for what we know at present, let the word unfathomableness, which I have purposely employed, avert from me the reproach of exaggeration, and establish the point of view which the physical, chemical, and physiological inquiries of our days, should they be rendered fruitful by new powers, have to take, and what deviations they have to avoid.

III. *An attempt to form a judgement respecting the Power of Vision of the Eye and the Ultimate Power of the Microscope.*

I will now connect with the above paper, for its illustration and confirmation, a few considerations on the power of the human eye, and on the confidence and the hopes which we may found upon microscopical observations and optical instruments. Up to the present moment, so far as I know, we have never been able to fix a constant measure for the ultimate possible power of the microscope. M. Amici, in a letter to the Baron von Zach in the year 1824, (Ferrusac, *Bullet. des Sc. Mathem.*, p. 221,) has calculated the limit of vision according to the power of the eye, and stated that a space of $\frac{1}{12}$ of an inch becomes imperceptible to the naked eye at the distance of 28 feet. Lately the angles of vision for the different colours have been calculated by M. Plateau, but I have no knowledge of any result from similar observations having reference to the microscope. I will endeavour to lay down, without any pretensions, my investigations respecting the limit of vision by the microscope made in a different way; and I shall rejoice if they make an addition to this branch of our knowledge and are not wholly without utility.

In the numerous opportunities which I have had of watching persons eager for knowledge who were desirous of acquainting themselves by personal observation at my house with the wonderful structure of the infusoria, I found to my astonishment the difference of the power of vision of individuals by far more nearly coincident than I had expected, and than it is generally stated to be. When once I had placed the delicate object in the right point of vision of the instrument, or directed the attention of the naked eye to a very minute object, fifteen or twenty persons, to whom I often showed these things at the same time, saw completely alike and with the same clearness what I myself saw: they very seldom took another, and then but little different, distance of the object from the eye, according to what they required. In order to be quite certain that I was not deceived by the politeness or shame of those who might not willingly say they had seen nothing, I have often desired the observers to delineate the objects seen, or minutely to describe them; by which I learnt with certainty that they saw the object exactly the same and quite as distinctly as I myself had seen it, and almost always without its being in the first place necessary to change the position of the microscope. These observations, continued carefully on a great number of persons, with the most various distances of vision, made it seem probable to me that there is a nearly fixed common limit for the power of vision of the unclouded and healthy human eye, which will admit of our forming a conclusion as to the ultimate power of the microscope. Upon this I made various observations, in order to find out how far the variations of myopical and presbyopical eyes possessed an influence on the general expression of that power; and I have convinced myself various times that the not unfrequent opinion that myopical people could see more and more distinctly than other persons is quite unfounded. The result of my experience is twofold:

1. There seems to be a normal power for the human eye in reference to the seeing of the minutest particles; and the deviations from it appear to be much more rare than is generally believed.

We can only speak of those who at some distance or other can see distinctly. Among more than 100 persons that I have observed, there were those who in the general relations of vision could see most distinctly, not capable of distinguishing more than I myself saw; and those who represented themselves weak-sighted or long-sighted were in general capable of seeing in the same degree as I did, only they wanted the object more minutely pointed out; and besides, in seeing with the naked eye it was necessary to have the object approached to or removed from their eye.

2. The smallest square magnitude attainable in general by the human eye in its natural state amounts for white on black ground, as well as for black on white or light-coloured ground, to $\frac{1}{36}$ of a Parisian line in diameter. It is possible by the greatest condensation of light and excitement of the attention to recognise magnitudes between $\frac{1}{36}$ and $\frac{1}{48}$ of a line, but without sharpness and certainty*.

This is the limit of the power of the natural human eye for coloured bodies, of which everybody can satisfy himself, as I have done, by strewing on white paper very fine black particles of dust, for instance, of dried ink, water colours, &c., and then taking the smallest of them with a fine point and placing it on a glass micrometer, which at least gives the $\frac{1}{48}$ of a line. Sun- and lamp-light also allow easily of our observing black particles and the like, with or without a mirror, on the glass micrometer on a light ground. Bodies which are smaller than those mentioned, notwithstanding all attempts, cannot be discerned with the naked eye singly, but may be when placed in a simple straight row.

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* To insist on $\frac{1}{49}$ would not be worth while. The next proportions worth notice were $\frac{1}{60}$ or $\frac{1}{72}$ of a line, and I have never been able to make any trial whether it could be seen by any one.

If there are several near to each other or several rows, they make a joint impression on our eye and deceive us, as if we saw a greater simple body or surface*. The general distance which good eyes maintain when striving to recognise these minute bodies, I found by measurement to be from four to five inches, often six inches; the latter is however the general distance for very sharp-sighted persons. Myopical persons seldom bring the same objects to within four inches, and still more rarely to three, &c., and become then for the most part like others. Any one whose distance of clearest vision is four inches cannot by a greater approaching of the eye to the object increase his power of vision, but feels pain, and does not see distinctly. If we have once hit on the object, we may remove it considerably without losing sight of it. I myself cannot see $\frac{1}{24}$ of a line at twelve inches' distance black on white; but having found it at from four to five inches' distance, I can remove it to twelve inches and still see the object plainly. This phænomenon is founded on the known power of the eye to accommodate itself somewhat to distance; we can often discern small objects at a greater distance when our attention is directed to the place, or when they move. Similar phænomena are found in the air-balloon in a clear sky, and a ship in the horizon; we easily see them after they have been pointed out to us, but the faculty of rapidly descrying depends on custom and on the acuteness of sight, without permitting of any conclusion as to the power of vision in general. When any person is more strongly excited by visual impressions than another he discerns them quicker, but he does not on that account see more than the other, who because he does not receive these impressions so vividly discovers them slower. I often employ a glass to seek for objects which I wish to examine with the naked eye, in order to give them another position with a pointed instrument; this fact has relation only to finding the place of a body, and merely furthers quickness of discernment. Myopical eyes always find out things more easily, because they are less separated, and their field of vision is smaller. Probably there is still a higher degree of the absolute power of vision of the human eye to add, which is that of the discerning of luminous bodies. In the dark, small luminous bodies appear, as is known, much larger than they are; and these, whether they be themselves luminous or only reflectors of light, are capable according to the strength of the light of easily affecting the human eye at a much less magnitude than the $\frac{1}{48}$ of a line. I have never had the opportunity of observing self-lighting magnitudes which were really of so small a diameter that I could direct attention to a limit in regard

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* I am accustomed in this manner to discern very fine cilia of the infusoria. As soon as they are moved they form a small apparent surface, which is perceptible; as soon however as they leave off, their fineness is often such that the power of vision does not reach them with the microscope.

to this. How far astronomy may afford us data which may be calculated with exactness, and applied to this subject, I will not discuss; for even if the measurement of the strength of light did not give a retardation, subject always to the prevailing theory of light and hypothetical, no certain conclusion can be deduced for the want of direct knowledge as well of the magnitude as of the distance of the heavenly bodies. Bodies which reflect light can indeed be examined, but the result of my experience has no particular bearing as to optical instruments. Metallic lustre, which is a very powerful reflector, may, according to my experiments on gold dust, be discerned with the naked eye in common daylight up to the $\frac{1}{100}$ of a line, therefore double in proportion to colours. But this same bright surface when magnified 380 times, appears then dull and uneven, and the corpuscle acts only as a black one, or becomes transparent with a leek-green hue. The coloured transparency of gold seems to commence at the thickness of $\frac{1}{2000}$ of a line in diameter, and is evidently not the consequence of porosity. Fluid metals might probably form the smallest magnitudes attainable; but even could we see with the microscope the reflected light of the multiples of the magnifying power with 100, &c., the outlines of bodies so minute would still remain the more undiscernible the stronger their reflection of light is in proportion to their magnitude. The dust of diamonds upon a black ground in concentrated solar light, may probably afford the ultimate square power of reflected light; I have however hitherto not been able to make any observations on this point*.

With lines it is very different. Non-transparent threads $\frac{1}{400}$ of a line in thickness, by holding them towards the light, may be discerned with the naked eye. Spider-threads measure $\frac{1}{300}$ to $\frac{1}{2000}$ of a line; threads of the silk-worm $\frac{1}{200}$. The latter in the cocoon are double.

The result of these inquiries is as follows:

Optical instruments enable us to see with distinctness of coloured square parts of a line only multiples of their magnifying power with the number thirty-six, or at most, but then not distinctly, up to the number forty-eight. The limit for lines and light impressions lies much further, and is determined by the intensity of the light.

The most accurate microscope, with a distinct magnifying power of

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* I obtained the finest particles of gold by scraping gilt brass. By filing pure gold I always obtained much coarser particles.

The thickness of leaf gold used for gilding is quite imperceptible to the naked eye, like the edge of a very sharp knife. If they are distinguished, it is by means of the inequalities of the bent border, and this is an optical deception.

In the gray mercurial ointment the quicksilver forms, if well prepared, almost equal globules of about $\frac{1}{500}$ to $\frac{1}{1000}$ of a line in diameter, which are hindered from uniting by the covering of grease. These *finest* quicksilver globules are not to be distinguished by the naked eye even in sunshine, but form with the white lard a gray colour. Larger globules may often be perceived in it. The fat covering may probably dim the metallic lustre.

100, never according to this enables us to see, with every effort, square objects smaller than corpuscles of $\frac{1}{3600}$ to $\frac{1}{4800}$ of a line in diameter. If however all circumstances are not happily combined, as is often the case, it is impossible to discern even much larger magnitudes. A distinct magnifying power of 400 allows of the possibility of distinguishing square magnitudes which are of $\frac{1}{14400}$ to $\frac{1}{19200}$ of a line in diameter, or which amount to nearly the half of the length of a wave of light in the undulatory theory. With a magnifying power of 1000 we should be able to discern square bodies which are of $\frac{1}{36000}$ to $\frac{1}{48000}$ of a line, and we should then distinguish Newton's elements of the red colour, or be convinced that they do not exist. With a magnifying power of 3000, like that which Amici's microscope is said to possess, we should be able to distinguish the $\frac{1}{72000}$ to the $\frac{1}{144000}$ of a line, and with this must discern almost all Newton's elements of colours; nevertheless we should not yet be able to ascertain the thickness of the partition of the belly of a monad, but must presume its presence only from the act of the holding together of the nutritive particles.

To pass over Amici's improvements of the microscope, which have become so important, but which unfortunately are still too expensive, a field has been opened to mechanism by what Selligue has made public respecting compound microscopes; and by the method of advantageously combining several simple achromatic object glasses, already applied by him to simple microscopes, and to other combinations, and which has been executed by Chevalier in Paris, and by Plösl in Vienna, with so much ability and success; from which it appears that by employing the other auxiliary means, the power of vision of the eye may be still very greatly raised by increasing the degree of magnifying power which is compatible with distinctness: and the more we look for a speedy improvement of optical auxiliary means from the praiseworthy emulation of the distinguished, and often so completely scientific mechanists and opticians of our time, the more it behoves the observer to tell his views and wants, openly and freely owning his ignorance of the practical details.

There are, as is obvious, in regard to mechanical discoveries and improvements, two things especially for mechanists to keep in view. On the one hand judicious treatment of the object *con amore,* without looking to a high price; and secondly, the simplifying of improvements already discovered, and the greatest possible diminution of the necessary cost by these means, in order to diffuse their application. It is not indeed to be expected or desired that men who are capable of producing permanent works of art should employ their time in rendering their mechanism more simple; but the incitement of others to the simplification of their discoveries, and the multiplication of the simplest constructions, which not only include the newest principles, but also the highest power, should raise in them a stronger interest for diffusing as much as possible the use of their instruments than it actually does; and then it is little to be feared that their own satisfaction, honour, profit, and advantage would be taken from them. I have observed the greatest power, although with some inconvenience, in one of Plösl's best microscopes which our Academy of Sciences possesses. A better still, one of Amici's best microscopes, Berlin is wholly destitute of, or it is not known to me, although it is very desirable that the examination of such an one were not inaccessible to the scientific persons of this town.

It is exceedingly gratifying, that in addition to the recent very careful and successful exertions of M. Ober-Bergrath Schaffrinsky, which unfortunately have not extended further, MM. Pistor and Schiek also, whose great scientific accuracy is universally acknowledged, have resolved to apply their care to the microscope; and those already produced are, as I have convinced myself, so excellent, that they are little inferior in power to my Chevalier's, and in convenience are evidently superior.* I will here remark that in the microscope, clearness in small magnifying powers, however great it may be, is no superiority, but a property indispensably requisite to the character of a good one; and that the term superiority can only be applied to the greatest distinctness and convenience along with the highest powers.

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* M. Ehrenberg, it appears, was then not acquainted with Schiek's new microscope, and a letter alluding to this subject is found in the same volume of Poggendorff's *Annalen*, p. 188, where he says, "The sharpness and magnifying power which M. Schiek has succeeded in giving to this convenient and elegant instrument, filled me with true inspiration; and since its properties are founded on a rule determined by him for the combination of the object lenses, and several instruments finished at the same time gave the same clearness of image, I think it my duty, and advantageous to science, to make known the results of a comparison of it with the best instruments in this town." He then enumerates the good and bad qualities of Chevalier's and Plösl's microscopes, and says, "The microscope of Schiek unites the chief merits of those microscopes; large field, extremely sharp and clear light, which leaves nothing to wish for, even in the highest powers; magnifying power equal to the highest of Plösl—twice as high as that of Chevalier, and moreover a much greater focal distance. Besides this, it possesses a most convenient and elegant form, without being weak in its framework." In another place: "The great clearness of the image, and the *due strength of light*, is an advance; but the union of all these good properties in such a degree is a still greater one." Since Ehrenberg wrote this he has had the opportunity of comparing Schiek's instrument with those of Dollond and Amici. His opinion was that it surpassed them in conveniency and elegance, and quite equalled them in power, largeness of the field, and focal distance.

M. Ehrenberg adds that his measurements were made in the same plane with the object, and not at a distance of five, eight, or twelve inches of the eye from it; and informs whoever wishes to make a comparison, that his eye-distance from the object was, with a power of 380, 10″ 6‴; with a power of 800, 1′ 5″. He also suggests that in comparing the power of microscopes, the mean length of view of ordinary sight should be taken as a point of measurement at 8″; and states that his magnitudes refer to absolute measure, and require no reduction.—W. F.

Mechanists and less experienced observers are very often in error about this, and imagine that by clearness of the small magnifying powers they compensate for the strongest. Since, according to the statement of Herschel, Amici's microscope magnifies with clearness 3000 times, it should be the endeavour of opticians not only to imitate the form and method of this instrument, but more especially to multiply and simplify in every possible way that property of the highest distinct achromatic magnifying power. For the general study of the finer organization of all organized beings it is needful to possess in our days a clear achromatic magnifying power of at least 300 to 400 times in diameter. Microscopes which are of less magnifying power are, notwithstanding all art, beauty and clearness, not to be recommended to students and public establishments. Costly screw micrometers, more than three or four achromatic object glasses, which may be used single or together, reverberatory apparatus, and glass prisms, are mostly unnecessary, and of advantage but rarely and in limited cases; and their use not proportionate to their cost. On the contrary, there are various things, especially a fine glass micrometer, and separate powerful magnifying eye-glasses, and double mirror, which really help and supply the wants of the active and careful observer.

I cannot promise myself much from the solar microscopes until they are further mechanically developed. Those which I have had the opportunity of seeing may certainly be considerably improved, and their power increased. I conclude by again repeating the observation, that there are, putting aside all inorganic bodies, even in the kingdom of organic bodies whose constituent parts or molecules are generally considered to be the coarsest, magnitudes capable of direct proof which are in diameter $\frac{1}{16000}$ of a line; and others that can be proved indirectly which may be less than a six millionth part of a Parisian line in diameter; that the ideas often expressed respecting atoms, as subjects of experience, are somewhat too confident; finally, that the power of the microscopes which we at present possess does not in its maximum amount to more than to make distinctly visible long opake threads of $\frac{1}{1,200000}'''$ diameter, and square superficies or globules of $\frac{1}{144000}$ of a line in diameter; and that for these latter they must be increased forty times in order to satisfy what is required for reaching directly those minutest *parts* of *organic bodies* whose existence has been inferred from simple deductions; and that we are not to entertain a thought of perceptible or ever attainable simple matter, or material primitive constituent particles.

ADDENDA.

1. As regards the Spermatazoa, which might be regarded by many persons as very important in the decision of the question of the

primitive origination of the Entozoa, and which have not been mentioned in the memoir, I will only remark that these bodies are methodically inoculated into every animal being in which they have hitherto been discovered, which implies nothing wonderful, but only much that is obscure, and which hereafter may be gradually cleared up by an increased power of the microscope.

2. On the perception of the smallest bright bodies I have had an opportunity of obtaining a few more results. On pressing small globules of quicksilver on a glass micrometer, I easily obtained smaller globules of $\frac{1}{100}$ to $\frac{1}{2000}$ of a line in diameter. In the sunshine I could only discern the reflection of light and the existence of such globules as were $\frac{1}{300}$ of a line in diameter with the naked eye; smaller ones did not affect my eye either in sunshine or with a Chevalier's reverberatory lamp. I however remarked, at the same time, that the actual bright part of the globule did not amount to more than $\frac{1}{900}$ of a line in diameter. Spider-threads of $\frac{1}{2000}'''$ in diameter were still discernible from their lustre.

3. I have lately made some experiments on the dust of diamonds, and found that a diamond superficies of $\frac{1}{100}$ of a line in diameter presents a much more vivid light to the naked eye than one of quicksilver of the same diameter. I have not yet been able to find smaller particles of diamond dust possessing a good lustre. The smallest particles were from $\frac{1}{1000}$ to $\frac{1}{3000}$ of a line in diameter; but even under the microscope no lustre was to be perceived. This, perhaps, was owing to the treatment. The result of these supplementary observations is, that fluid metals, since a small part only of their globular superficies shines strongly, make perceptible only very small particles of light; that in proportion much smaller lamina, especially diamond lamina, may be at least as easily discerned as considerably larger metallic globules. Whether the proportion is as 1 to 3, further investigations must teach us. Particles of light having a linear form constitute the utmost limit of the power of vision; and the luminous or light-reflecting corpuscles are the fixed stars of the microscopic world.

Article XXIX.

On the Application of Circular Polarization to Organic Chemistry; by M. M. Biot and Chevreul.

On the Application of Circular Polarization to the Analysis of the Vegetation of the Gramineæ; by M. Biot.

(Read before the Royal Academy of Sciences of Paris, July 1st, 1833.)

From the *Nouvelles Annales du Muséum d'Histoire Naturelle*, vol. iii., p. 47, *sq.*

It being my intention to show by experiment in what manner indications derived from circular polarization may be usefully employed in chemical researches, principally in those of organic chemistry, the innumerable transformations effected in carbonated products by vegetable life appeared to me to be one of the objects of study best adapted for the attainment of this end. For these products, so various in their appearances and physical properties, being, under an infinity of circumstances, composed solely of carbon and water united in different proportions, their mixtures, combinations, and transmutations, offer excellent tests of a method which distinguishes them individually by inspection alone, and thus ascertains their presence without altering them. Now organic chemistry was deficient in precisely these characters recognisable by inspection, the consequence of which was its difficult, I may even say its often uncertain progress; because, being unable to recognise bodies otherwise than by isolating them, and this isolation being effected only by the intervention of special agents applied to the combinations or mixtures of which they form a part, the choice and appropriation of the tests to be employed for each case could only be determined by the conjecture, more or less probable, of their presence; and there is often danger of modifying these products by thus acting upon them, or even of creating new ones by uniting the principles of which they are formed; so mobile are the combinations upon which they depend, and with such facility do they become converted into each other.

The indications (*caractères indicatifs*) furnished by circular polarization certainly will not remove these last-mentioned difficulties, which are inherent in the subject; but in very many cases they will abridge and reduce them to those which are inevitable, by in the first instance furnishing the chemist with the properties capable of being immediately observed, predicable of the molecular condition of the combinations on which he has to treat; then by rendering equally visible and observable all the changes by which that primitive state may be altered, so that he may be aware of their occurrence as soon as they take place; and lastly, by affording characters of the same order for distinguishing the greater number of the organic products which he isolates. We do not here pretend to supply chemical tests, but simply to illustrate in many cases the convenience of their application, and to characterize immediately the consequences resulting from them by sensible effects; for it is definitively chemistry and chemistry alone by which the products can be isolated and resolved into their component parts.

The employment of this method, as the Academy has seen, has already enabled me to discover the singular modifications that the foliaceous organs of exogenous trees produce in the ascending sap which supplies them with nourishment in their first development; and it afterwards assisted me in distinguishing the elaborated products which these same organs convey under the cortical layers to nourish, or even perhaps to form the new cellular tissue. Persons conversant with the study of vegetable physiology can alone give to these researches the generality necessary for the deduction of its laws. All my ambition has been to offer them an experimental method of tracing these mysterious operations. The results that I now offer to the Academy are directed to the same end, and are intended to confirm those previously obtained, while they at the same time render them complete.

The long duration of exogenous trees is accompanied by a proportionate retardation of the total development of the phænomena of their vitality. The trunks of the *Gramineæ*, the existence of which is completed in a year, presents in this narrow circle the whole series of the analogous phænomena. From this class I have selected rye and wheat, with the intention of examining the various phases of their vegetation.

From the researches on germination of physiologists and chemists, we have learned what takes place immediately after the birth of these plants. The amylaceous globules (*globules féculasés*) deposited in the perisperm of the grain around the embryo are emptied, and the dextrine which they contain is converted into sugar, which serves as nourishment to the young stem until its foliaceous organs and roots are developed. But when this first supply of aliment is exhausted, the young plant is left to procure such as will continue its development.

Now the nature of these new alimentary products, the modifications which they undergo in the various parts of the plant, and the manner in which these various parts contribute successively or simultaneously to nourish the seed, and to supply it with the substances of which it is to be composed, by transmitting the new alimentary products to the fecundated ovary, have not I believe been hitherto experimentally determined.

It is necessary here to distinguish the solid materials, the fixation of which constitutes the skeleton of the plant, from the juices and soluble products, which being unceasingly formed, destroyed, and renewed, are conveyed by the life to every part of the vegetable, and conduce to its nutrition. The fixed materials may be known by the analysis of the dead or withered vegetable; but even among these we have to distinguish those which are essential to the existence of the plant, and those which have been accidentally raised from the earth by the roots, with the water in which they were dissolved, or held in a state of sufficient tenuity to be transmitted through the vessels and the vacuous spaces of the cellular tissue, I shall be careful not to commit myself in these complex questions, for which all the assistance of chemistry and of the microscope is scarcely sufficient, I shall confine my remarks to a few of the alimentary products of plants which are known to be composed by them, and conveyed into their various parts whilst undergoing the metamorphosis produced by vitality.

My first trials upon rye were made on the 3rd of May, upon young shoots, in which the ear was already developed but not yet flowering, and indeed far from it. The roots, the stems, and the ears were separately treated with water, and the extracts submitted to the tests of circular polarization; then these extracts concentrated but not desiccated were treated with alcohol, and the substances whether precipitable or non-precipitable were in the same manner submitted to the tests of polarization. Finally, these substances thus isolated were brought into contact with yeast in order to ascertain those which were or which were not fermentable; after which their *rotation* was observed, to discover whether it were diminished, increased, or altered in direction.

The extract of the roots presented indications of an exceedingly feeble rotation directed towards the left. As the extract of the stems acted in the same direction, I thought that these feeble indications might be attributed to the roots not having been rigorously separated from them. I had not then observed that similar almost neutral mixtures may be produced by sugars having contrary rotation, which are detected and rendered discernible by fermentation when one of their elements is cane sugar. The experiment must be renewed and completed by the aid of this process the following year.

The extract from the stems contained a mixture of grape sugar *turning to the left*, cane sugar *turning to the right;* and a substance precipitable by alcohol, which possesses the characters of gum of being completely soluble in water, and of directing the rotation to the left. These three substances originally mingled in the extract produced a resultant of rotation towards the left; this resultant was considerably weakened when the precipitable substance was separated, to the point of making the alcoholic extract appear almost neutral. But when the alcohol was expelled by heat, and the residuum of the extract brought into contact with yeast, a lively fermentation took place, and developed a strong rotation towards the left, thus detecting the mixture of grape sugar, not solidified, with cane sugar, which mutually concealed each other before the latter was interverted. The substance exhibiting a rotation to the left, and precipitable by alcohol, experienced also the alcoholic fermentation by contact with yeast, this property being either proper to it or arising from a small quantity of sugar which might have been entangled with it in the precipitation. But the effect of the fermentation was only to weaken the rotation, without altering its direction.

Twelve days after, on the 15th of May, the ears being more developed, but still far from flowering, the stems again presented the mixture of these three substances. But the proportion of cane sugar was increased, for it determined the resultant of the rotation in its proper direction, towards the right, before fermentation. When this sugar was destroyed in the extract by boiling it with sulphuric acid, the influence of this acid changed the direction of the rotation of the substance precipitable by alcohol, which passed from the left to the right. This, as M. Persoz and myself have shown, is also a property of gum.

The extract from the ears before flowering presented characters very different from the extract from the stems. Neither cane nor grape sugar was detected in it, but only sugar of starch (*sucre de fécule*), which the fermentation enfeebled without changing. Alcohol also produced a precipitate in it, but of a different quality to that of the stems, for it was not soluble in water, or only so in a very small degree; and this precipitate viewed with the microscope appeared formed only of shreds of cellular tissue and the remains of integuments similar to those which cover the globules of starch, without any sensible mixture of pulverulent matter. These results agreed with M. Raspail's observations, that the pericarp of the *Cerealia* before fecundation is filled with starch (*fécule*) in very small grains, the soluble matter of which is progressively absorbed by the ovary, and serves as nourishment to it when the fecundation is effected. But as the extract of the ears made previously to fecundation here presents us with sugar of starch, not with dextrine, it is evident that the globules of the pericarp must either contain this sugar ready formed and prepared to be absorbed by the young ovary, or that the globules are accompanied by a principle analogous to diastase, which breaks them and converts their dextrine into sugar, as in germination.

After fecundation is effected the composition of the ears is greatly altered. On the 15th of June the young grains of rye, taken from the ears, contained grains of starch ready formed, which were visible with the microscope. They burst under the influence of sulphuric acid and disengaged a substance soluble in water and precipitable by alcohol, which is ascertained to be dextrine by the great energy of its rotatory power compared with its density. Sugar of starch ready formed is also found in it, the fermentation of which enfeebles the rotation without changing it. There is nothing [in its polarizing action] which indicates the existence of cane or grape sugar.

The nature therefore of these two sugars which are contained in the foliaceous parts of the plant become changed like that of gum, by traversing the collars of the ears; and they serve as materials to the young grain, by which they are formed into dextrine, and the other products which compose the perisperm.

I have made analogous experiments upon the young shoots of wheat, but guided by the preceding, I have taken them more in division, applying them separately to the various foliaceous organs which in the rye I had studied as a whole. In these organs I found diversities of composition, of which I had no suspicion.

I commenced my experiments on the 19th of May, upon young shoots of wheat in which the ears were not yet developed. Suspecting that the composition of the leaves was different from that of the stem, and that they were destined to nourish it after fecundation, in the same manner as the leaves of trees nourish or form the new annual layer of bark and alburnum, I carefully detached the cylindrical stalk from the vaginating leaves which surround it, and treated these two parts separately by the processes which I have just described, viz. by water, alcohol and fermentation.

The stems, like those of the rye, presented three carbonated substances, viz. grape sugar turning to the left, cane sugar turning to the right, and a substance turning to the left which may be precipitated by alcohol. The relative proportions of these three principles varied considerably with the progress of vegetation. On the 20th of May their mixture produced a resultant of rotation directed towards the right, showing that cane sugar was predominant in it; but on the 4th of June, the ears having left the stems and flowered, the resultant of the stems had passed to the left, and was afterwards constantly maintained in that direction, evincing that the cane sugar had become relatively less abundant. It will presently be shown that in the ears it had passed in excess.

The leaves furnished results very different from those of the stems; they contained indeed a mixture of grape and cane sugar and a substance precipitable by alcohol and soluble in water after that precipitation, but, contrary to the stems, the proportion of cane sugar considerably exceeded that of grape sugar; besides, the precipitable matter having exerted a rotation to the right seemed to be dextrine, while in the stems the precipitable substance had a rotation to the left, and appeared by this character analogous to gum.

The leaves preserve this state of composition as long as their vitality continues, but when fecundation is effected they may be seen gradually to become yellow and to wither completely. This effect first takes place in the lowest leaves, and in each commences at the apex of the leaf and gradually extends to the point of insertion. When the leaves are completely withered, if they are gathered and submitted separately to the tests that have been described, nothing can be found but some insensible or nearly insensible traces of the saccharine principles and of the precipitable substance with which they previously abounded. Whence it appears that at the period of which we are treating these carbonated principles pass into the stem and serve it as an aliment, in the same manner as the analogous principles, elaborated by the leaves of exogenous trees, descend beneath the living cortical layer into the first external layers of the alburnum, in order to nourish the young cylinder of wood and bark, which like a hollow stem is annually formed and moulded upon the ancient skeleton of wood.

In rye and wheat the basis of the stems therefore derive nourishment partly from the leaves which are attached to them and partly from the soil. The summit of the stem may also be supplied with aliment by its own leaves, and may raise the inferior sap ; but the ear, when it has left the stem, and especially when it has been fecundated, appears to exercise a powerful faculty of absorption upon the juices contained in the summit, which must remove them rapidly, in proportion as they are furnished by the base of the stem. To satisfy myself that this was the fact I divided the stems of wheat, from which the leaves had been removed on the 4th of June, into two parts, the ear being in full flower. Of the two extracts thus formed, that of the bases contained nearly twice the quantity of sugar contained in the extract of the summits, the densities being equal. At this period also of full efflorescence the saccharine principles are abundant in the ears of wheat. They exist in them in the state of sugar of starch and cane sugar, adjoined to a substance precipitable by alcohol, which is perfectly soluble in water, and has a rotation to the right like dextrine, but having less rotating energy and susceptible of modification by fermentation. The presence of cane sugar in the ears is ascertained by the rotation of the extract, which though strongly directed towards the right before fermentation, is suddenly thrown towards the left, and becomes very feeble as soon as that phænomenon is completed. There was nothing to indicate the existence of this sugar in the ears of rye before flowering, nor in the young grains of rye, though the stems also contained cane sugar. Could it arise from a difference of quality proper to the two plants ? Whatever it might be they each present this remarkable result, that the grape sugar of the stems does not pass in that state to the ears.

As has been remarked above, in proportion as the fecundated ear is enlarged the lower leaves become yellow and withered by transmitting their carbonated products to the stem. The base of the stem also withers and becomes yellow in its turn, while the superior part which is still green continues to nourish the ear, as is well known to agriculturists. This fact, taken in conjunction with the preceding results, explains several practices in agriculture, and shows in what their good effects consist.

Thus when the base of the stem is withered, if the *Cerealia* be cut before the grain is ripened, it continues to receive nutriment and to be ripened at the expense of the stem, as if it still remained adhering to the soil. When the stems are dry the grain may therefore be brought to maturity without its being exposed to the losses of spontaneous shedding; at least when there is reason to hope that the rains will not fall and destroy it upon the earth, upon which it has been prematurely extended. The advantages of thus anticipating a retarded harvest have been enlarged upon by skilful agriculturists, and the application of the principle has been commenced.

Secondly, since the leaves and stems of green plants form sugar and other soluble carbonated products, which are to be absorbed by the seed, which, as I have just stated, occurs in wheat, rye, and, as I have ascertained, in several other herbaceous plants, as well as in the leaves of exogenous trees, if they are buried in the earth in that state of verdure, it is evident that they will enrich the soil with all these products, so eminently conducive to the nourishment of the young plants to be produced from it. Now since it is proved by experiment that the green parts of vegetables decompose the carbonic acid of the air and appropriate the carbon, it becomes infinitely probable that this absorption contributes to form the mass of their saccharine and gummy products, in addition to the juices which they may draw from the earth by their roots; and this probability is increased when we see how considerably the carbonated products of the leaves differ from the products of the stems, which derive their aliment more particularly from the earth. It is then the natural and legitimate conclusion that one part of the solid mass of plants is furnished during their life from the carbon of atmospheric air, so that by burying them green in the earth more is rendered to the soil than it has yielded.

Those only who are versed in chemistry and vegetable physiology can enter deeply into the grand phænomena of the absorption and fixation of atmospheric principles in plants, whether immediately by their own organs or by the intermediation of inorganic substances capable of absorbing those principles, and of afterwards conveying them to plants in the nascent state. The application of lime in this mode of intermediate action has already been suggested, and my own observations furnish evidence in confirmation of the propriety of the suggestion. Probably analogous effects of absorption and successive transmission may be produced by other substances, either upon the carbonic acid or the azote of the air, and their discovery would immensely extend our means of fertilizing the earth. The processes of investigation that I have here applied to products formed by living vegetables would be serviceable in these useful researches by manifesting in numerous instances, by sensible physical characters, the existence of the principles which have been introduced into them.

When the annual circle of vegetation has been completed I shall collect into a single memoir the results that have thus been obtained, accompanied by a detail of the experiments which have been employed to determine them. This collection of examples, added to my previous researches and to those which I have pursued in conjunction with M. Persoz, will be sufficient to show in what manner indications of circular polarization may be rendered subservient to organic chemistry and also the manner of applying them to that purpose. The only task that I proposed for my own performance will then be accomplished, and I shall expect from the active dexterity of our chemists the almost unlimited developments which it appears to promise.

Examination of an Optical Character, by which, according to M. Biot, Vegetable Juices capable of producing Sugar analogous to Cane Sugar, and those capable only of producing Sugar similar to Grape Sugar, may be immediately distinguished; by M. CHEVREUL.

From the *Nouvelles Annales du Muséum d'Histoire Naturelle*, vol. iii. p. 307, *sq.*

1. I HAVE judged it expedient to devote a special memoir to the development of the reasons upon which the opinion expressed in the preceding report [on several papers relative to the chemical and physiological history of starch,] respecting the importance of the optical character proposed by M. Biot to be applied to organic chemistry reposes; in order to show that this opinion has not been formed without due consideration, and that it is in fact only an application of the views which I have elsewhere explained long ago, upon the relative importance of the various properties suitable to be employed as characters in the definition of chemical species, considered individually and collectively.

2. To attain this object I proceed to examine the optical character proposed by M. Biot:

First, in relation to the objections which may be urged against its importance in organic analysis, and in the definition of species.

Secondly, in relation to its probable utility in distinguishing the various arrangements of the atoms or particles of a particular species, and in estimating the alterations which may occur in bodies of determinate species mixed together; and to (in my apprehension) its real use as a reagent or indication in the determination of chemical species of organic origin.

§ 1.

On the objections which may be urged against the importance of the optical character.

ARTICLE I.—*On the objections which may be urged against the importance of the optical character in immediate chemical analysis.*

3. We examine in succession the cases; first, in which a juice causes a deviation in the plane of polarization to the left; secondly, in which the deviation is to the right; and lastly, in which there is no deviation. We shall afterwards treat of the difficulty of estimating the quantity of active matter from the density of the liquid in which it is dissolved, a difficulty which arises in the two cases of deviation.

a. Deviation to the left.

4. When a deviation to the left in the plane of polarization is observed, how is it to be *immediately ascertained* whether this property belongs to gum Arabic, or to grape sugar not solidified, since it is common to them both? How are we to be certain that the property of the juice proceeds from only one of these substances, and is not the result of the activity of them both? Lastly, what certainty is there that it is not caused by other bodies than the gum and the grape sugar not solidified?

b. Deviation to the right.

5. There is the same uncertainty if the subject of observation be a deviation to the right; for the dextrine of Biot, cane sugar, sugar of starch of the first formation, sugar of starch of the second formation, and solidified grape sugar have all the property of producing a deviation to the right in the plane of polarization.

6. This is not all: sugar of starch of the first formation and cane sugar have nearly the same energy, so that, as M. Biot acknowledges, recourse must be had either to alcoholic fermentation which interverts the plane of polarization of cane sugar to the left, and which leaves unaltered that of sugar of fecula, or to sulphuric acid which produces the same results. M. Biot gives the preference to the last method, because he says that fermentation is an operation not sufficiently understood. But as sulphuric acid develops sugar of starch and grape sugar with principles which are not saccharine, may not the employ-

ment of it, at least in some instances, be productive of error? In short, since it is necessary to have recourse to fermentation or sulphuric acid, it is evident that the character of circular polarization does not furnish the means of *immediately distinguishing* the vegetable juices which yield sugar analogous to cane sugar, and those which only yield grape sugar; and that therefore it has not the advantage of giving a more precise indication in organic analysis than that furnished by the chemical processes, which are liable to the objection of disturbing the equilibrium of the elements of substances, which by their means have been separated from each other.

c. *Case in which there is no deviation.*

7. M. Biot quotes a case in which he found a *fluid of an extremely saccharine quality without rotation*[*], because it contained at the same time grape sugar solidified and not solidified. He remarked that time produces an alteration in the solidified sugar, gradually diminishing its property of rotation to the left and directing it towards the right; thus the same body spontaneously experiences a molecular alteration which has a tendency to cause it to pass successively through a series of states marked by the signs + 0 and —. After such a result, how is it possible to imagine that the extreme states distinguished by the signs + and — could be precise characters for other bodies proper to cause their immediate recognition in the juices of plants?

d. *Difficulty of estimating the quantity of an active principle from the density of the fluid by which it is held in solution.*

8. The action of deviating from the plane of polarization whether to the left or to the right, being the product of all the active molecules contained in the liquid upon which the experiment is performed, it follows that in the most simple case, that in which the activity emanates from only one principle, when we would determine the specific nature of this principle, it will be necessary to attend to its proportion relative to the solvent; for as quantity may compensate for the feebleness of the action, two solutions may have the same rotatory power, though one may contain a principle much less energetic than the other.

9. How is this proportion to be ascertained? According to M. Biot, by taking the density of the liquids; but if positive results can be drawn from the determination of the density, it can only be when tables of the respective solutions of each active principle have previously been formed, in each of which the densities correspond to determined proportions of the principle dissolved, and to the rotatory powers of solutions made according to the same proportions.

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[*] *Nouvelles Annales du Muséum d'Histoire Naturelle,* 3rd series, vol. ii. p. 341.

10. Without such researches, without accurate knowledge of the nature of the bodies which accompany one or several active principles, of their proportions relative to their solvents, and of their influence upon the density of the juice in which they are contained, how can a general rule be formed for the appreciation of the density proper to the active principle or principles of a vegetable juice, endowed with a property of causing a deviation in the plane of polarization, in order to deduce from it the nature and the proportions of the principle or principles? Now science is actually deficient in all the investigations necessary to surmount this difficulty.

ARTICLE II.—*On the objections which may be urged against the importance of the optical character in the definition of chemical species.*

11. Though there is not for the classification and definition of chemical species, a subordination of characters comparable to that observed in natural classifications, among which those endowed with life, such as the zoological and botanical species, are ordinated into genera, families, orders, and superior divisions; yet there are properties in chemistry, the importance of which rest upon distinctions essential to that science, which furnish characters more or less rational, for grouping the species together and distinguishing them from each other. The differences to be remarked between the classification of chemical species and that of zoological and botanical species arise principally, as I have elsewhere shown, from the small number of general properties which are adapted to serve as general characters of chemical species, from the correlative relation of these properties, and from the circumstance that the special object of chemistry is the circumscription of the species. We will now enter upon a few details relative to the properties capable of serving as characters of chemical species, whether for grouping them or for distinguishing each of them in particular, and supplying means by which they may be known.

12. The combustible and comburent properties of simple bodies, and the acid and alkaline properties of compound bodies, are general properties of such a nature, that if in consequence of their correlation, they will not serve for the formation of groups perfectly circumscribed, yet they serve to give a precise idea of a body which possesses one of these properties in a certain degree of energy. We will take for example the acidity of an oxygenated body when it is sufficiently energetic to remove the alkali from the red substance of litmus.

Since it possesses this property which gives it the function of an acid, it may be concluded:

1st. That it will enter into combination with all, or at least with the greater part of the compounds of an alkaline quality.

2nd. That the proportion of potash or of any other oxybase necessary to neutralize this acid being once known, it will not be necessary to have recourse to experiments to ascertain the quantity of the other oxybases which are capable of neutralizing the same acid.

3rd. That the action of the acid upon organic colouring principles may be predicted with a great degree of probability.

13. There are properties which, without having the importance of those which have been noticed, are interesting, inasmuch as they furnish useful indications. For instance, if a substance precipitates without alteration the animal matter of the water in which they are held in solution, it may be inferred with great probability that it will preserve animal matter, as is the case with the tannins; and I shall show that nearly all the substances of which this is predicable, though they may differ widely in their elements, have notwithstanding many analogous properties; among others a flavour more or less astringent.

14. The properties which have been considered may be remarked (12 and 13), though under different relations, when a community of characters is endeavoured to be established between a greater or less number of chemical species, differing greatly with regard to their elementary composition. Let us now examine the properties which are the best adapted for the definition of particular species.

15. The properties the most suitable for this purpose are certainly those which are manifested for our observation with equal intensity in the different conditions in which the specimens of the species possessing them may be placed. For example, acidity, which we have considered as one of the most general properties of compound bodies, may become a specific character of precision when considered in an acid body in particular, with regard to the proportion in weight of the potash or any other alkali, that a given quantity of this acid requires in order to its neutralization. In fact this proportion will be constant as long as the specific nature of the acid lasts.

16. Properties which are manifested by obvious phænomena of easy production are also adapted to become specific characters; but I shall show that these characters are specific in proportion to the small amount of the alteration sustained by the elementary composition of the species. With this circumstance in view, I have formed three groups of chemical properties from these three species: first, those which do not undergo any sensible alteration in their composition; secondly, those which undergo an alteration which does not prevent them from resuming their first composition; and lastly, those in which the alteration is sufficiently profound to prevent the resumption of the original composition*.

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* *Considérations générales sur l'Analyse organique.* Levrault, Paris, 1824. p. 34 to 42.

This distinction is very important when in our researches into the immediate principles of organized beings we are desirous of ascertaining the value of indications furnished by what in chemistry are called *reagents**.

17. There are physical properties which furnish characters for distinguishing bodies in analytical researches, which are valuable in proportion to the limitation of the number of species possessing them, and the facility with which these species may be distinguished among themselves by other characters. Such is the property of producing a violet vapour, which belongs only to iodine and indigo, bodies very distinct, since the vapour of the first does not undergo any alteration, even at the most elevated temperatures, while the vapour of the second is completely altered even below 560d.

18. Definitively, the properties which furnish the chemist with characters the best adapted for the classification, definition, and recognition of chemical species in analyses, are

a. Those which are the most constantly found in a certain species, whatever be the diversity of circumstances in which it may be placed;

b. Those the existence of which necessarily involves that of others;

c. Those which are in general concomitants;

d. Those, easily verified, which belonging only to a very small number of species, differing widely in other respects, are valuable for analytical researches, or to concur with other properties in characterizing these species, but whose existence does not lead to any conjecture relative to an analogy of properties between the bodies to which they belong.

We shall now examine, according to the views that I have just explained, the optical character proposed by M. Biot.

19. Grape sugar which has not been solidified directs the plane of polarization to the left; and as, according to M. Biot, its chemical nature is unaltered when it becomes crystallized in grape juice, and as it then directs the plane of polarization to the right, it follows *that this property is not fundamental, since it is found in the same species with two different signs;* it does not therefore fulfill the condition 18 *a*.

20. Cane sugar has certainly less analogy with sugar of starch of the first formation than the latter has with sugar of starch of the second formation; the action however of the two first is equal or nearly equal, while the action of sugar of starch of the second formation is much feebler than that of sugar of starch of the first formation. *From this it is evident that the optical character proposed by M. Biot does not apply to one of those properties the existence of which necessarily involves that*

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* *Rapport de M. Chevreul sur un Mémoire de M. Donnée. Annales de Chimie et de Physique*, vol. xxxviii. p. 89.

of others or leads to their prediction, since it has a tendency to confound two very different bodies, and since on the other hand it establishes a difference between two bodies which in other respects have the greatest analogy of properties and composition.

It does not therefore fulfill the conditions 18 *b c d.*

21. From the manner in which M. Biot has related his observations, it appears to me that, in the actual state of things, *the property of causing a deviation in the plane of polarized light is in its variations connected rather with the various arrangements that the particles of a species may take without their nature being altered, than it is with the various arrangements which constitute different species; so that there is not now more reason to establish a mutual relation between species which act in the same direction and with the same energy, than there is to presume that a decided opposition exists between the properties of two species which act differently upon the plane of polarization.*

§ 2.

On the probable Utility of the Optical Character.

22. I have stated the objections which may be urged against the use of the optical character as it has been presented by its author; I shall now consider in what it is likely to be useful. By this mode of examining the physical character, the application of which to organic chemistry has been proposed, I hope to render apparent the object which I have really in view, which is to restrain within its true limits that which has been made to exceed them by ascribing to it a generality which it does not possess, and a degree of precision which it can only attain by ulterior experiments, and which even then will be confined to the limits which I attribute to it.

ARTICLE I.—*Utility of the Optical Character for the various Arrangements of the Atoms or Particles of a Species.*

23. If it be true, as M. Biot thinks, that a body, as grape sugar, though dissolved in water, affects the molecular state in such a manner as to cause deviation to the right or left of the plane of polarization, accordingly as the solution has been made with crystallized sugar, or is such as nature presents us with in the juice which has just been extracted from the grape, it is undoubtedly interesting to inquire whether other species of immediate principles present an analogous phænomenon, in order to judge whether any consequence may be deduced, relative either to the various arrangements of which the atoms or particles of these species taken separately may be susceptible, or to the cause which produces the variation of the phænomenon.

24. When the object of study is a species of body brought to its

greatest degree of purity, it is incontestably important to investigate the action which it is capable of exercising upon the plane of polarization, when it is dissolved in any liquid whatever, compared with the action which it is capable of exercising after having been exposed to the influence of some agent, such as light, heat, or electricity.

25. I shall show that the importance of these researches to the chemist does not arise from their affording proof that an observable alteration has taken place in the rotatory power of a substance submitted to the action of a certain agent, when the nature of this substance has been evidently changed; that is to say, when it has been converted into a substance absolutely distinct from what it was before the experiment; but from their enabling him to ascertain whether an alteration in the arrangements of the particles has really occurred, in cases in which the substances submitted to experiment appear at first sight not to have undergone any such alteration, and in which, without the test of circular polarization, we should be led to conclude that they had absolutely not experienced any.

26. The following example will illustrate my proposition:

A solution of starch in boiling water is converted into sugar by sulphuric acid. The starch dissolved in water being insoluble in alcohol, whilst the sugar into which it becomes converted is soluble in that substance, we have a means of distinguishing in the action of sulphuric acid upon starch the moment when the conversion of this principle into saccharine matter commences, and the moment when it is completed. If it be now discovered that the solution of starch is possessed of a property of causing the plane of polarized light to deviate to the right in a much greater degree than is effected by its sugar, is it not true that the observation of the diminution of the rotatory power of the solution of starch submitted to the action of sulphuric acid teaches nothing more than the preceding facts relative to the alteration effected in the properties of the starch? And the conversion of a substance essentially insipid and incapable of producing alcohol into a fermentable saccharine substance, gives a much more exact idea of the change effected in its composition than that derived from the variation of its rotatory power. In cases in which a substance submitted to an agent has sustained an alteration in its rotatory power, which, far from being, as in the example of the starch, the result of the conversion of one substance into another perfectly distinct from the first, has on the contrary sustained so slight an alteration in the distribution of its particles that without having verified it we should conclude that the substance had not undergone any alteration whatever in its properties; it is then, I repeat, that the observation of the optical character becomes interesting, as leading to researches which may render other alterations discernible which without them might escape the notice of the observer.

Article II.—*Utility of the Optical Character for estimating the Changes which may occur in determinate Species mingled together.*

27. It would be important to ascertain the influence that chemical species, the relations of which to the property of which I am treating have been previously perfectly determined, would exercise by their mutual contact, whether in the destruction or the neutralization of this property, in its development, augmentation, or diminution. I conceive that there are mutual actions of certain bodies in solution of which we are at present ignorant, in consequence of not possessing means of observing some phænomenon which is only manifested when they are mutually present. It is, I apprehend, particularly in relation to the organoleptic properties of bodies that it would be useful to attempt researches of this nature. I cannot here enter upon the subject more in detail; but I reserve the particulars for a work upon the *neutrality* of bodies, considered in the most general manner.

Article III.—*Utility of the Optical Character as a Reagent in the Determination of Chemical Species of Organic Origin.*

28. A certain vegetable fluid, or liquid of animal origin, or in a word any solution the nature of which is investigated by analysis, when submitted to the action of polarized light, gives a determinate result. Well! I apprehend that the observation of the optical property may furnish useful indications in the following cases:

First case:

29. In which the properties of the substance analysed are found in the separate principles. The optical property of these principles explains perfectly that of the substance of which they are the constituents; consequently this result concurs with other observations to prove that it has not sustained alteration in the analysis.

Second case:

30. In which all the properties of the substance analysed are not found in the separate principles. The observation of the optical character may here assist in the solution of the question, Has there been any alteration of the separated principles? or do not the changes observed arise from the destruction either of a combination or a mutual influence of principles, while no alteration has occurred in the elementary composition of these principles? We then enter upon the question which I have considered in my "*Considérations générales sur l'Analyse organique*." p. 116.

31. I think that sufficient attention has not been given to ascertain whether there be not in grape juice and sugar of starch of the first formation some body foreign to grape sugar and sugar of starch,

which exercises some influence upon the results as they have been described by M. Biot.

CONCLUSION.

If we admit with M. Biot:

1st. That a substance such as grape sugar can cause a deviation in the plane of polarization in one case to the right, and in another to the left;—

2nd. That two substances perfectly distinct, such as cane sugar and the sugar of starch of the first formation, have sensibly the same rotatory power;—

3rd. That two substances so approximated as are the sugars of starch of the first and second formations, have perfectly distinct rotatory powers; we must conclude that there is no consequence deducible from the optical character relative to the connection of one species with another; and that its indications relate only to differences of molecular arrangements, which have but an inconsiderable influence upon the characteristic properties of the species.

On the Application of the Laws of Circular Polarization to the Researches of Chemistry; by M. BIOT.

From the *Nouvelles Annales du Muséum d'Histoire Naturelle,* vol. iii. p. 502, *et seq.*

WHEN a new process of observation is introduced into the sciences, it is well that it should be contested and criticized by persons of reputation, for if the discussion be conducted by each party with freedom and sincerity, nothing can be more advantageous to the new invention, provided it be sufficiently well established to sustain it. With a conviction of this truth, I have read the dissertation inserted by M. Chevreul in the last number of the *Annales d'Histoire Naturelle,* upon the phænomena of circular polarization which I discovered in a great number of solid, liquid, and even gaseous substances; and upon the employment which I proposed to make of it in the most delicate researches of organic chemistry, in which these substances almost exclusively occur.

M. Chevreul divides his dissertation into two parts.

In the first (I quote his own words) he examines the objections which may be urged against the optical character which I have discovered.

In the second he specifies the use to which he thinks it may be applied.

I also shall follow this division of ideas. But in employing this arrangement, it is necessary here to recall with precision the nature of the character under consideration, as it was conceived and explained by me when either experimentally determining or applying its physical laws: for (a circumstance resulting probably from its novelty, and from the scarcity of the apparatus hitherto constructed for its application,) I differ almost as much from M. Chevreul with regard to the appreciation of the advantages which he attributes to it, as I do with regard to the limitations to which he supposes it liable. Nor will a clear and precise explanation of this new method of studying bodies be misplaced in the annals of natural history, in which I have several times described the results which I have deduced from it relative to various particulars of vegetation.

When a ray of homogeneous light is polarized by reflection in a certain plane, which I shall suppose to be vertical, both sides of this plane manifest symmetrical properties, when it is analysed immediately with a doubly refractive achromatic prism. This symmetry is still preserved when the ray thus prepared traverses certain transparent liquids, water, alcohol, and the fat oils, for example, before it arrives at the prism; at least such is the case within the limits of the thickness in which I have had opportunity of testing them. Other liquids on the contrary, such as solutions of sugar, camphor, and gum, and many of the essential oils, destroy this primitive symmetry even when the surfaces of entrance and of emergence are perpendicular to the direction of the transmitted ray. If this ray be analysed after its emergence, it is again found polarized in one direction, but that different to its primitive direction, with an angular deviation towards the right or the left of the observer, according to the quality of the substance interposed. The angle of deviation for each substance when in a similar state is exactly in proportion to the thickness that the simple ray has traversed, which assimilates the observable effect to a continuous and uniform rotation of the plane of polarization. But the arc of rotation described in each substance of equal thickness differs for the different simple rays, according to the fixed laws which I have experimentally determined, and which up to the present time are sensibly identical for all substances, with the exception of tartaric acid, which alone offers an anomaly in this respect, whence it may, not without probability, be inferred that it is a combination of two atomic groups of contrary rotations having unequal dispersive powers. Whatever may be the fact, the general law of the deviation of different rays in all other cases, enables us to predict numerically the composition and the succession of the coloured images that the crystallized prism presents when the light transmitted is white; whence results the facility of making observations with a light of this nature as rigorously as with simple light, and in a manner infinitely more convenient in practice, and more delicate with respect to the appreciations which can be made by its means.

The single fact that the rotations are proportional to the degree of thickness in liquid mediums under normal incidences, proves to every natural philosopher familiar with the general laws of mechanics that the action thus exerted is molecular; that is, that the total deviation observed through a limited thickness is the sum of the infinitely small angular deviations successively produced by the groups of atoms which compose each infinitely thin layer of the simple or compound substance exercising a power of this nature. This molecular power is of such great importance, and is so evidently the principle of all the chemical applications of the phænomenon which are possible, that I have employed the most minute attention and the most diversified tests in order indubitably to establish its truth.

I endeavoured in the first instance to remove all idea that the effect could arise from a certain actual relation of position existing among the molecular groups of the active medium. For this purpose I agitated the particles by moving the medium during the transmission of the ray, and modified their intervals by the application of heat, without however carrying it so far as chemically to alter the atomic groups; the total deviation remained the same, as the mechanical laws had previously indicated. I again removed these groups much further, and, so to speak, indefinitely, by mixing active with inactive liquids, or with those having an action upon each other either in a similar or in a contrary direction: the total deviation produced by the mixed system was always rigorously the sum of the partial deviations that the luminous ray would have undergone in traversing the same sum of active and inactive groups placed in succession in separate tubes. These experiments, which M. Chevreul says require *now* to be made*, I performed, and published in the memoirs of the Academy *sixteen years ago;* and I almost lost my life at that time in performing an experiment by which I proved that the essence of turpentine preserves its rotatory property in the state of vapour in motion.

I established all these laws in 1818†, and no addition had been made to them until a more profound examination of these laws, and an apparatus of much greater sensibility, furnished me with indications of the rotatory property infinitely more delicate than those which I had previously employed; and I succeeded by these means, two years ago, in discovering this property in a great number of substances of organic

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* *Rapport sur l'Amidon*, chap. v., § 72. *Annales*, p. 266.

† Vide the *Mémoires de l'Académie des Sciences* for the year 1817.

origin in which I had not previously suspected its existence. I then resumed with renewed attention all the experiments which could serve as bases for establishing the details; and these results were accompanied by the formulæ necessary to deduce the comparable consequences; establishing for each substance, whether simple or compound, what I call its power of actual molecular rotation, which is the angular deviation which it exercises upon the plane of polarization of a certain simple ray, with a thickness of one millemetre, and a hypothetical density equal to unity. Though the volume of the Academy in which these researches are inserted has not yet appeared before the public, I have sent within about the last year printed copies of them to several chemists both Frenchmen and foreigners, and they have served as the foundation of all my subsequent researches. For I have since found it sufficient to apply the same methods and the same formulæ in the various experiments which I have undertaken, simply extending or corroborating them by the additional processes which the development of my researches required or suggested; so that to dissipate the different objections that M. Chevreul has raised, at least those which I have well understood, I shall merely have to quote the corresponding results which are already published in these Annals.

But first I shall greatly simplify this discussion by declaring that I have not any intention of following M. Chevreul in the most extended article of his dissertation, in which he examines "*the objections which may be urged against the importance of the optical character in the definition of chemical species.*" Having never proposed its application to such a use, I have not to defend it upon this point; more especially as in my own opinion no character taken separately is sufficient to define, I will not say a chemical species in general merely, but even a substance individually unique. Such definitions are and can be merely the expression of our ignorance; or in other terms, of our actual knowledge. An attempt was made to class natural solid bodies according to their crystallization; but among them were found some rigorously isomorphous; for instance, those which crystallize in cubes or in regular octahedrons proved to be such by the complete symmetry of their derived forms. A second attempt was made to class them according to chemical composition: this was defeated by the discovery of bodies exactly isomeric. These two examples may suffice to convince us that the definition of bodies should be established upon the union of the observable characters that each of them possesses; and that this definition must always be merely provisional, as another system of material particles may be discovered tomorrow, possessing in common the whole of this first collection of properties. The character derived from circular polarization is therefore, and can be, nothing more than an additional element, a new condition of the actual molecular state of the material systems, whether simple or compound, in which it exists; and I have positively and repeatedly said in the *Annales d'Histoire Naturelle*, that it was in this light that I regarded it*. In agreement with this view, in my applications of this character I have naturally had recourse to all auxiliary means suitable to be employed in conjunction with it. M. Chevreul had no occasion to say, as he has done p. 592, that, "*as M. Biot acknowledges*," when cane sugar and sugar of starch are mixed together in one solution, it is necessary in order to distinguish them to have recourse to alcoholic fermentation, or to the action of acids suitably regulated, in order to change the sum of the two rotations into a difference. He ought to have said that the employment of these auxiliary processes was my constant practice, and one of my principles formally expressed.

The metaphysical question relative to the species being disposed of, I proceed to M. Chevreul's other objections. The first three, which he calls *a*, *b*, *c*, consist of inquiries how, when a deviation to the left is observed, it is to be immediately ascertained whether it belongs to gum or to grape sugar not solidified, or to a mixture of the two substances, it being common to them both; and when a deviation to the right is observed, how it is to be immediately distinguished whether it be produced by dextrine or sugar of amidon. Considering these questions in the positive sense of their experimental application, it is not now necessary in the actual state of optical chemistry to reply to them; for not only the particular conditions here suggested, but a great number of others analogous and more difficult, were long ago determined in my researches upon vegetation, in which the specialty of function of various organs, incessantly modified by the progress of life, effectuated mixtures very differently complicated than those suggested by M. Chevreul. As I cannot suppose that he is ignorant of these results, which were published in the Annals, and still less that he wilfully suppressed them, I must of necessity discover some abstract sense in the difficulties he has raised, independent of the real applications which I have made; and a word that I have just written, the word *immediately*, excites a suspicion in my mind upon the subject. In the title of my first memoir upon liquid grape sugar, which has since been followed by many other more extended applications of my methods, I have said that by means of the optical character derived from circular polarization, the juices of fruits capable of producing sugar analogous to cane sugar, and those from which only grape sugar might be expected, may be *immediately* distinguished. In fact all the juices of our cli-

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* Vide the memoir upon the slow or sudden variations which occur in several organic combinations, *Nouvelles Annales du Muséum d'Histoire Naturelle*, vol. ii. p. 335. Ibid., vol. iii. p. 48, upon the application of circular polarization to the analysis of the vegetation of the Gramineæ. [See p. 581.]

mates from which cane sugar has hitherto been extracted, those of beet-root, parsneps, carrots, and marsh-mallows, presented a rotation to the right, whilst all those which yield only grape sugar invariably presented a rotation to the left; thus by the word *immediately* I meant *instantly*, at *the very moment;* and indeed in my first observations, I did not seek for other means of distinguishing the two kinds of sugar in question, not having at that period met with them naturally mixed sufficiently to conceal or intervert their proper rotation. Now if it be the word *immediately* which has shocked M. Chevreul, as expressing the pretence on my part of employing solely the optical character, to the exclusion of all other, and particularly of chemical means, I would beg him to observe that I have never acted in a manner to justify this interpretation. For even in my first fundamental memoir read before the Academy on the 5th of October, 1832, I determined the opposite rotations of the two principles of honey, the crystallizable and the uncrystallizable, after having separated them by means of alcohol; and I have never since neglected to seek all the assistance that chemistry is capable of affording. It is however, I repeat, with much hesitation that I attribute to M. Chevreul the suggestion of a difficulty which appears to me to be purely grammatical; for if such were his thought, he could not, without a degree of injustice of which I believe him incapable, cite my original expressions as he has done, without adding that all my researches subsequently published contradict the idea of exclusion which this interpretation attributes to me; and that I even formally expressed the contrary principle at the commencement of my memoir on the analysis of vegetation in the Gramineæ, as may easily be seen. As to the rest, it will at least be evident from this discussion, that neither am I who have invented and applied the optical character, nor is M. Chevreul who examines it, of opinion that it should be separated from the chemical characters which may aid in its applications; and this I apprehend is the only scientific point of interest at present.

I now arrive at the last of M. Chevreul's objections, objection *d*, which is expressed in these terms: "*Difficulty of estimating the quantity of an active principle from the density of the liquid by which it is held in solution.*" I cannot possibly understand how, or in what respect this objection can be applied to my formulæ, or to the results which I have deduced from them. And certainly it is the intention of the writer so to apply it; for in his development of it, mentioning the necessity for distinguishing the proportion of the active substance in the solvent, in order to decide upon its specific nature, M. Chevreul inquires (p. 593.) how this proportion is to be ascertained; and he adds "it is, *according to M. Biot, by taking the densities of the liquids*," a method which appeared to him, and with truth, to be of difficult employment, and he might have added, very inexact. But there is here on the part of M. Chevreul some error, though undoubtedly an involuntary one; for I have never proposed or employed such a method, which may be proved by consulting my formulæ. It is true that they contain the density of the solutions observed, as they also enter into the determination of numerous other physical results; as, for example, into the calculation of refractive power, and of capillary forces; though certainly it has never been said that these phænomena are estimated or measured by the density. So in the phænomena of circular polarization there exists for each active substance a necessary mathematical relation between its power of molecular rotation; the thickness through which it is observed, whether insulated or in solution; the angular deviation which it produces of the plane of polarization of a simple ray of a given nature; and lastly, the actual density of the solution in which the substance exists, as well as its ponderable proportion in that solution*. Of these five elements, four being given, the fifth is deduced by necessity from the mathematical relation; and if this unknown fifth be, for example, the ponderable proportion of the active substance, it may indeed be obtained by calculation, in which the density will enter as one of the elements. But it will not be from this density, at least not from it alone, that the proportion will be estimated. It is even evident from the formulæ that in aqueous solutions greatly diluted, the density of which consequently scarcely differs from unity, this element preserves scarcely any influence upon the ponderable proportion, because it only affects the decimals of a very distant order. For example, when I say, as I can say, that by means of the apparatus which I now employ the presence of two thousandths in weight of cane sugar, or one of dextrine, in an aqueous solution may *immediately* be rendered sensible and appreciable, it is not certainly from the density that such results are obtained; for at such degrees of dilution the densities of the solutions differ so little from unity, that the observation of the density might be entirely dispensed with, and unity be substituted in its place, without the ponderable proportions of the substances being affected by it in an observable degree. The estimation of the ponderable proportion by the density must not therefore be attributed to me, for it does not in any degree belong to me, and the supposition that it does would lead to a very false idea of my processes.

After having thus considered the objections which M. Chevreul

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* Not only is the mathematical relation of which I am treating established in my memoir of the 5th of October, 1832, printed among those of the Academy; it is also mentioned in the memoir upon grape sugar which has served specially as a text to M. Chevreul's dissertation. Vide the *Nouvelles Annales du Muséum d'Histoire Naturelle*, vol. ii. p. 97, in a note. The numerical table in the following page is mathematically deduced from that relation.

thinks may be *urged against the optical character derived from circular polarization,* I intended to follow him in his consideration of its utility; but this relating to our own views without affecting those of others, the consideration of it would not be profitable to science. Those who labour at the present day to connect by rational relations the innumerable transformations to which organic chemistry gives birth, will easily feel that the specially molecular character of the power of optical rotation assigns new conditions which must necessarily be satisfied, in selecting the groups of atomic combinations which represent the compound products. I thought that I should be rendering a service to science by here giving a precise explanation of this character, of which M. Chevreul's dissertation appeared to me to present involuntarily a very inexact idea, which might retard its application. This duty accomplished, I leave it to the judgement of experimentalists.

Paris, Dec. 14*th*, 1834.

ARTICLE XXX.

On the Laws according to which the Magnet acts upon a Spiral when it is suddenly approached to or removed from it; and on the most advantageous mode of constructing Spirals for Magneto-electrical purposes; by E. LENZ.

From the *Mémoires de l'Académie Impériale des Sciences de St. Petersbourg*, vol. ii., 1833, p. 427. Read on the 7th of November, 1832*.

FROM the great interest which the late discoveries of Faraday in the field of electro-magnetism must awaken in all the natural philosophers of Europe, it is to be expected that we shall soon receive many and various explanations of the momentary action of an electric current on an electrical conductor; and as it is allowed according to Ampère to reduce the action of a magnet entirely to that of circular electric currents, the same may be expected with respect to the action of the magnet upon such a conductor. Up to the present moment we here in the north are only acquainted with the papers of Becquerel, Ampère, Nobili, and Antinori and Pohl; and as none of these authors have occupied themselves with that branch of the subject to which I have directed my particular attention, I hasten to make known as quickly as possible the following contribution to the science of magneto-electrism.

After having repeated Faraday's chief experiments†, I first proposed to myself to find out in what manner the phænomena of the magnetic action on a spiral suddenly approached or removed might be produced in the easiest and most powerful manner. For this purpose I had to determine what influence

1. The number of coils,
2. The breadth of the coils,
3. The thickness of the wire,
4. The substance of the coils,

of the electromotive spirals (i. e. of those which are acted upon by the magnet) had upon the phænomenon; and this determination, together with the necessary consequences following from it, are contained in this present memoir.

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* Translated from the German by Mr. W. Francis.

† In this repetition I obtained the spark beautifully by means of a spiral of a wire 70 feet in length and 0·014 inch thick. The apparatus was formed after the one described by Nobili, so that the horse-shoe magnet (of 22 lbs. lifting power) caused of itself the closing of the current.

The following was the apparatus I employed for my experiments. A multiplier (with a very sensible double needle of Nobili) of seventy-four coils of copper wire of 0·025 of an English inch in thickness* was placed in connection by means of conducting wires with the electromotive spirals, so that the horseshoe magnet which acted on the spirals was at a distance of nineteen feet from the multiplier, and had no immediate influence on its needles. I had assured myself of this by previous experiments. The horseshoe magnet consisted of five single bent steel bars, firmly connected with one another by screws; the middle one protruded at the ends about 0·7 of an inch; it might together with the armature weigh somewhat more than twenty-two pounds. The length of the bars was twenty-three inches, the breadth 0·8, and the thickness 0·22; the middle one projecting beyond the others was 0·4 in thickness; the distance of the arms was 1·64 inch. In order to be able to approach and remove the spirals, and at the same time to read off the deviation of the needle without any aid, I constructed my apparatus in the following way:—I did not cover the multiplier with its bell glass, but with a glass cylinder open at both ends, and closed these by means of a plate of mirror glass; I then placed over it a good mirror under an inclination of 45°, and from a point near the magnet I observed by means of a good Munich telescope the reflected image of the scale of the multiplier. The reading off was thus performed very precisely, and was more certain than with the naked eye close to the scale, because at this distance and with a fixed position of the eye the parallel axis of the index which stands at some distance from the graduated circle may be considered as evanescent. The method of exciting the electric current was the same as that given by Nobili: I wound the electromotive wire about a soft iron cylinder, which served as an armature and was filed smooth at those places where it was laid on the magnet, and laid it then on the magnet, or removed it suddenly from it, by which the magnetism arising at the moment, or vanishing again in the armature, thus produced the momentaneous electric current. But as the removal of the armature can be performed in a more certain, prompt, and uniform manner than the placing of it on, I have in all my following experiments only given the results which were caused by the taking off of the armature, or the sudden removal of the magnetism in the iron. I must here at the same time remark that in my experiments it made no difference whether the magnetism of the iron disappeared really and entirely all at once, or there still remained a part, provided only the remaining quantity of magnetism was the same after each removal. I frequently convinced

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* In this memoir the measures are always expressed in English inches, except when otherwise remarked.

myself of this by the identity of the results in several repetitions of the experiment. This also showed me that the electromotive power of the magnet, at least after having already undergone several removals, did not become weaker; proofs of this will also be furnished in some experiments hereafter to be mentioned. In the above-described arrangement of the apparatus, I could now with the right hand perform the removal of the armature from the magnet, which was fixed to a table, while at the same time my eye observed in the telescope the consequent deviation of the index of the multiplier. This index was a thin lath, which was fixed by means of some wax to the wire which served as a common axis for the two needles of the multiplier, and formed a diameter of the graduated circle. Being thus able to observe the deviation for every result which was to be deduced therefrom, first on the one and then on the other end of the index, I freed this result from the influence of the eccentricity of the axis of the needles, and turning first the end A and then the end B of the spirals towards the north arm of the magnet, and allowing the needles of the multiplier to deviate first on the one side then on the other, I made the result independent of a second error which arises if the cocoon threads to which the needles of the multiplier are suspended possess a rotatory motion. Further, I carefully avoided every disturbance of the multiplier during a series of combined experiments, because it is impossible that every coil of the multiplier could act in the same manner as another (this would presuppose that they were all in the same plane, and parallel to one another), and because even if this might be presupposed, the action would still vary according as the needle when stationary might be exactly parallel to the coils, or form a greater or less angle. The positions of the needles when at rest seldom differ more than 0°·3 from one another. According to the above statement, a complete experiment always demanded four observations, namely, two (at both ends of the hand) for the position where the end A of the spiral was turned to the north pole, and two where B was directed to the north pole. Besides this I have repeated almost every experiment twice over in order to convince myself that no accidental fault had crept into the reading off; if the two observations differed much from one another, I again repeated each of them. The first preparatory experiments were made on the influence of the combinations of the conducting wires with the electromotive spirals and with the wires of the multiplier, in order to see whether I should content myself with winding the ends of the wires, which had been freed from their silk and were clean, very closely round one another, or should be obliged to produce a closer connection, for instance by immersing them in quicksilver. I proceeded on the supposition that if the connection effected by winding them many times closely round one another was not sufficient, an increase of convolutions would necessarily increase the force of the electrical current, I therefore made the following experiment. I wound round the armature ten convolutions of copper wire bespun with silk, and the conducting wires were connected with the ends of this spiral only by a single twist of the wires; the result of the four readings off amounted to 36°·8; upon this the same connection was made by twisting the ends of the wires ten times round one another as tightly as possible; the deviation amounted again to 36°·8; I finally pressed the last connection as tightly as possible together with a pair of pinchers, so that they were very much flattened; the deviation was 36°·75. We may therefore consider the connection made by tightly twisting the wires ten times round one another as quite sufficient, and this was therefore made use of in all the subsequent experiments. The places where the connection was made were then wound round with silk stuff in order to secure them from reciprocal contact.

The second preparatory experiment I made in order to see whether, when I advanced the electromotive spiral on the armature more to the north limb or to the south limb of the magnet, it had any influence on the electric current. For this purpose I obtained with two convolutions the following results:

The convolutions advanced till in contact with the north limb of the magnet, gave a deviation . . . . = 5°·55

The convolutions advanced until in contact with the south limb of the magnet, gave a deviation . . . . = 5°·55

The convolutions advanced to the middle of both limbs gave a deviation . . . . = 5°·60

therefore this influence also of the different positions of the spirals on the armature is imperceptible: from this time I always placed them so that the spirals occupied the middle of the armature.

I thirdly determined, before I proceeded to the proposed experiments, the thickness of the copper wires employed; I weighed two feet of each having wound off the silk, by which I obtained the proportions of their diameter on which it principally depended; but in order to obtain also their absolute thickness, I measured the thickest by means of a micrometrical contrivance: I obtained the following results, in which I have designated the wires, beginning with the thinnest, Nos. 1, 2, 3, and 4.

	grains		inch.
2 feet of wire No. 1 weighed =	23·3;	absolute thickness =	0·023
2 ———— No. 2 ———— = (wire of the multiplier)	27·4;	———————— =	0·025
2 ———— No. 3 weighed =	83·9;	absolute thickness =	0·044
2 ———— No. 4 ———— =	166·1;	———————— =	0·061

All the four kinds of wires were well covered with silk, so that no metal could be perceived except at the ends which served for connecting them.

I now proceed to the experiments themselves.

I. *On the Influence of the number of Convolutions upon the Electromotive Power produced in them.*

In these experiments I connected the wire No. 3 with the multiplier so that the conducting wire and the electromotive spirals were formed of one and the same piece; the length of this wire was about fifty feet: here however this is of no consequence, as it remained the same in all the experiments. The experiments themselves are contained in the following table.

Number of the Convolutions.	INDIVIDUAL DEVIATIONS.				Mean deviation, or α.	α in minutes.	$\frac{1}{2}\alpha$.
	Side A of the Spiral to the north pole.		Side B of the Spiral to the north pole.				
	End *a* of Index.	End *b* of Index.	End *a* of Index.	End *b* of Index.			
2	5°·7	5°·8	5°·3	5°·8	5°·65	5° 39′	2° 49′
4	12·1	12·9	11·1	12·0	12·00	12 00	6 00
8	25·7	25·8	22·9	25·2	24·90	24 54	12 27
9	29·5	30·1	26·2	28·5	28·32	28 19	14 15
10	32·5	33·3	29·4	32·0	31·80	31 48	15 54
12	39·8	40·9	35·8	38·6	38·77	38 46	19 23
14	47·4	48·8	40·8	45·9	45·43	45 43	22 51
15	49·3	50·9	45·0	49·0	48·55	48 33	24 16
16	55·7	56·8	47·6	52·3	53·10	53 6	26 33
18	63·1	64·4	54·1	57·8	59·80	59 48	29 54
20	71·0	71·8	62·8	66·6	68·05	68 3	34 1

From this series of experiments we must now deduce the electromotive power of the spirals for each number of convolutions, for which purpose the following considerations will be of service.

The action of the electric current in the wire of the multiplier upon the magnet needle, is a momentary one, since the current itself exists only for a moment; we may therefore consider this action as an impulse given to the needle, and shall be able to measure its force by the velocity which it imparts. But the velocity of the needle at its exit is evidently as great as that which it acquires when it springs back to the point of exit; it may therefore be expressed (f being constant) by

$$A = f \sqrt{(\text{sin. vers. } \alpha)}$$

where A represents the sought for velocity of the exit; or according to what has been above stated, the magnitude of the current in the wire of the multiplier, and α the angle of deviation of the needle produced by this force. This expression changes however by the substitution of $2 \text{ sin.}^2 \frac{1}{2}\alpha$ instead of sin. vers. α into the following

$$A = p \cdot \text{sin. } \tfrac{1}{2}\alpha$$

if we put $p = f \sqrt{2}$.

In order now to find the resistance which the electric current suffers in its passage through the different wires, I first reduce their lengths all to one diagonal, and indeed to that of the wire of the multiplier, on the principle that two wires of the same metal offer then the same resistance to conduction when their lengths are in the same proportion as their diagonals (See *Ohm's Galvanic Chain*). In this case therefore the reduced lengths of the wires express their resistance to conduction: to have therefore a general idea of the problem, I suppose the multiplier, the conducting wires, and the electromotive spirals (with their free ends) to have the three reduced lengths, L, l, and λ, and the electromotive power produced in the spirals to be represented by x, then $\frac{x}{L + l + \lambda}$ will be in effect the current which takes place, and we therefore have

$$\frac{x}{L + l + \lambda} = p \sin. \tfrac{1}{2} \alpha$$

$$x = (L + l + \lambda) \cdot p \cdot \sin. \tfrac{1}{2} \alpha \quad . \quad . \quad . \quad . \quad . \quad . \quad (A.)$$

If we now consider the electromotive power in a convolution of the wire as unity, representing the unknown deviation produced by a convolution by ξ, and its reduced length by (λ); then granting the probable hypothesis, that at one and the same distance of the convolutions the electromotive force is directly as the number of convolutions, the following relation will take place for the number n, and for the reduced lengths λ_n belonging to it (this is not necessarily $n\,\lambda$, because the free ends of the spirals need not increase in the same ratio for every number of convolutions)

$$\frac{1}{n} = \frac{(L + l + (\lambda))\, p \cdot \sin. \frac{1}{2} \xi}{(L + l + \lambda_n)\, p \cdot \sin. \frac{1}{2} \alpha}$$

therefore

$$\sin. \tfrac{1}{2} \alpha = n \cdot \frac{L + l + (\lambda)}{L + l + \lambda_n} \cdot \sin. \tfrac{1}{2} \xi \quad . \quad . \quad . \quad (B.)$$

In the experiments just mentioned $l + \lambda$ continued of the same magnitude for every number of convolutions, as the conducting and spiral wire consisted of one piece, besides L remains the same, we therefore have $L + l + (\lambda) = L + l + \lambda_n$ and the equation B becomes changed into the following:

$$\sin. \tfrac{1}{2} \alpha = n \sin. \tfrac{1}{2} \xi \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (C.)$$

If we now put instead of $\frac{1}{2}\alpha$ the values contained in the last column of our table of experiments, we obtain eleven equations, from which after the method of the least square, we shall be able to determine ξ, and if we bring this value of ξ into the equation (C.), we shall find the deviations α belonging to the number n of convolutions, and the differences between this and the observed values will show whether the assumed hypothesis of the proportionality of the number of convolutions and of the electromotive power is confirmed in reality by the observation.—The known formula for sin. $\frac{1}{2}\xi$ is after the method of the least squares:

$$\sin.\tfrac{1}{2}\xi = \frac{\Sigma\,(n\cdot\sin.\tfrac{1}{2}\alpha)}{\Sigma\,(n^2)}$$

and after having performed the calculation, we have from the foregoing table

$$\xi = 3^\circ\ 9' \text{ or } \log.\sin.\tfrac{1}{2}\xi = 8{\cdot}43989.$$

This value of ξ gives for α the following values:

α		DIFFERENCES.		α		DIFFERENCES.	
Calculated.	Observed.	In Degrees and Minutes.	In Degs.	Calculated.	Observed.	In Degrees and Minutes.	In Degs.
6° 18′	5° 39′	+ 0° 39′	+ 0°·6	45° 22′	45° 26′	− 0° 4′	− 0°·1
12 38	12 00	+ 0 38	+ 0·6	48 48	48 32	+ 0 16	+ 0·3
25 36	24 54	+ 0 32	+ 0·5	52 16	53 6	− 0 50	− 0·8
28 42	28 19	+ 0 23	+ 0·4	59 26	59 48	− 0 22	− 0·4
31 58	31 48	+ 0 10	+ 0·2	66 50	68 1	− 1 11	− 1·2
38 36	38 46	− 0 10	+ 0·2				

the coincidence of the calculated with the observed deviations, confirming our presupposition that the electromotive power increases as the number of convolutions.

A second series of experiments on the same subject were made with the same wire, No. 3, except that the length of the wire through which the current had to pass, was no longer the same in each number of convolutions; we must therefore return to our general formula (B.). It was

$$\sin.\tfrac{1}{2}\alpha = n\cdot\frac{L + l + (\lambda)}{L + l + \lambda_n}\cdot\sin.\tfrac{1}{2}\xi.$$

The wire of the multiplier and of the conductors always remained the same, and was reduced to the diameter of the wire of the multiplier

$$L + l = 673{\cdot}25 \text{ inches}.$$

The lengths λ, $\lambda_{\prime}$, $\lambda_{\prime\prime}$, &c., were however changeable; I have therefore added these values, reduced also to the wire of the multiplier in the following table of the experiments.

$$L + l + (\lambda) \text{ is } = 681{\cdot}45$$

Number of the Convolutions.	DEVIATIONS.				Individual Means.	Complete Means or α.	λ	$L+l+\lambda$
	Side A of Spiral to the north pole.		Side B of Spiral to the north pole.					
	End *a* of Index.	End *b* of Index.	End *a* of Index.	End *b* of Index.				
5	18·5	18·5	19·8	20·5	19·33	19·40	17	690·25
	18·6	18·8	20·2	20·3	19·47			
10	37·3	37·6	39·6	39·3	38·45	38·41	28	701·25
	37·3	37·5	39·4	39·3	38·37			
15	57·8	58·7	58·6	58·2	58·32	58·18	39	712·25
	57·4	58·2	58·6	57·6	57·95			
20	81·4	82·3	80·7	79·8	81·05	80·91	50	723·25
	81·3	82·3	79·7	79·8	80·77			
25	111·0	112·7	103·1	101·9	106·67	106·67	61	734·25
	110·0	112·8	103·7	102·2	106·67			

If we now apply the method of the least squares to this table, as we did to the first, we obtain

$$\xi = 3^{\circ}\cdot97 \text{ and log. sin. } \tfrac{1}{2}\,\xi = 8\cdot53944$$

and with this value we obtain from formula (B.) the following deviations:

Number of Convolutions.	DEVIATIONS.		Difference.
	Calculated.	Observed.	
5	19·53	19·40	+ 0·13
10	39·00	38·41	+ 0·59
15	59·07	58·13	+ 0·94
20	80·67	80·91	− 0·24
25	105·67	106·67	− 1·00

In this place also the calculation coincides well with the observation; as I expected however to attain this coincidence still more completely, if I allowed the length of the conductors to remain the same for all the experiments, I made a second series of experiments similar to the above with another multiplier, where λ, λ', &c., remained always equal to one another; this series has also been performed with more care than the others above-mentioned, since each of the numbers contained in the following table is the mean deduced from three observations, in which mean however I retained only one decimal place. The columns designated by 1, 2, 3, 4 are intended for the same purpose as the four columns in the former tables.

Number of Convolutions.	Deviations.				Mean Deviations or α.
	1	2	3	4	
5	8·6	8·7	8·5	8·6	8·63
10	17·5	17·8	17·2	17·1	17·40
15	26·4	27·2	26·6	25·6	26·45
20	35·5	35·3	35·6	34·6	35·25
25	45·2	46·0	45·0	44·2	45·10
30	54·6	56·5	55·0	54·1	55·05

Hence may be calculated by means of the least squares

$$\xi = 1{\cdot}73 \text{ and } \log. \sin. \tfrac{1}{2}\xi = 8{\cdot}18478$$

therefore we have for the calculated values of α

Number of Convolutions.	α		Difference.
	Calculated.	Observed.	
5	8·77	8·60	+ 0·17
10	17·60	17·40	+ 0·20
15	26·53	16·45	+ 0·08
20	35·58	35·25	+ 0·33
25	45·00	45·10	— 0·10
30	54·67	55·05	— 0·38

Here then the coincidence for this kind of experiments is very great, so that we may regard the position as entirely confirmed, namely that

> "*the electromotive power which the magnet produces in a spiral, with convolutions of equal magnitude and with a wire of equal thickness and like substance, is directly in the same proportion as the number of the convolutions.*"

Moreover, we must not let it escape our attention, that in all the three series of observations the differences of the calculated and of the observed deviations are in the beginning positive, and then negative; which seems to show that the electromotive power increases in a somewhat quicker proportion than the number of the convolutions; but the differences are so small, and become, when the observations are made with great care (as the third series proves) smaller and smaller, I therefore ascribe this little irregularity to the influence of some peculiar circumstance which up to the present moment I have not succeeded in discovering.

II. *On the Influence of the Distance of the Convolutions of Spirals on the production of the Electromotive Power in them.*

In these experiments I employed at first the horseshoe magnet, but

I soon perceived that from this none but false results could be obtained. By considerably widening the circuit of the spiral, it advanced nearer and nearer to the upper bow of the magnet; so that by removing the armature, not only the sudden disappearance of the magnetism in it, but also the sudden removal which took place at the same time from that upper part of the magnet (the bay of the horseshoe) acts on the spirals, and indeed unequally with unequal diameters of the spiral; the electromotive power becomes thus greater in larger spirals than it would otherwise be. On this account I took two strong rectilinear magnetic systems, each of which consisted of ten single magnet bars; I laid them with their opposite poles against one another so that they lay in a straight direction, and brought the iron cylinder which had served me in the above-mentioned experiments as an armature to the horseshoe magnet, between their poles, while the spirals covered the cylinder; upon this I let the magnet be suddenly drawn by two assistants from each other in opposite directions.

I wound at first ten convolutions of wire No. 2 round the iron cylinder,

the diameter of the convolutions = 0·73 inch;

upon this I wound ten convolutions of wire No. 2 round a wooden disc,

the diameter of the convolutions = 6·57 inches;

the wooden disc was perforated in the centre, into which the iron cylinder was inserted. The observation gave

	Angle of Deviation.				Mean.
	1	2	3	4	
Narrow Conv.	24·6	27·1	26·4	26·5	26·15
Wider Convolutions.	22·8	22·7	22·0	22·5	22·50
	23·4	23·5	21·6	23·2	22·92
Narrow Conv.	24·8	27·7	26·3	26·6	26·35

I observed the deviation of the wider spirals between the narrower, in order that the fault which might have originated by diminishing the magnetic power of the magnet systems might be estimated: we therefore have

for the narrower spirals the angle of deviation $\alpha = 26{\cdot}25$

for the wider spirals the angle of deviation $\alpha' = 22{\cdot}71$

The length of the wire of the multiplier and of the conducting wires (reduced to the diameter of the first) was as in the former experiments, i. e. they amounted together to 673·25, or $L + l = 673{\cdot}25$, λ however is for the narrower convolutions = 28, and for the wider $\lambda' = 203$.

By means of formula (A.) we shall therefore obtain for the narrow spirals

$$x = (\mathrm{L} + l + \lambda)p \cdot \sin.\tfrac{1}{2}\,\alpha = 701{\cdot}25 \cdot p \cdot \sin.(13^\circ\ 7'),$$

for the wide spirals

$$x' = (\mathrm{L} + l + \lambda')p \cdot \sin.\tfrac{1}{2}\,\alpha' = 876{\cdot}25 \cdot p \cdot \sin.(11^\circ\ 21'),$$

therefore the relation of the electromotive powers, or

$$\frac{x'}{x} = \frac{876{\cdot}25 \cdot \sin.(11^\circ\ 21')}{701{\cdot}25 \cdot \sin.(13^\circ\ 7')} = 1{\cdot}0838,$$

therefore not deviating much from 1, that is, the electromotive power is in both spirals the same.

I endeavoured in a more striking manner to confirm this position by the following experiment: I wound the wire No. 2 in six convolutions round a great wooden wheel of 28 inches in diameter, and placed the wheel on the iron cylinder. After having completed, as in the former cases, the experiment, I wound also six convolutions of the same wire immediately round the same iron cylinder, where also, as above, the convolutions again were 0·73 inch in diameter. The experiment gave

	Angle of deviation.				Mean or α	λ	$\mathrm{L}+l+\lambda$
	1	2	3	4			
Narrower convolutions.	13·1	15·8	12·8	12·4	13·52	19·2	692·45
Wider convolutions.	7·1	8·7	7·1	8·7	7·90	549·2	1222·75

therefore $$\frac{x'}{x} = \frac{1222{\cdot}75 \cdot \sin.(3^\circ\ 52')}{692{\cdot}45 \cdot \sin.(6^\circ\ 45'{\cdot}5)} = 1{\cdot}0107.$$

Here the proportion of both electromotive powers approaches still more nearly to unity than in the former case, although the proportion of the diameter of the spirals is = 1 : 38·3. We may therefore regard as a thing proved by experiment, the position, that

> "*the electromotive power which the magnetism produces in the surrounding spirals is the same for every magnitude of the convolutions.*"

Since however a spiral wire inclosing the armature presents to the action of the magnetism in the armature a length greater in proportion as its diameter or its distance from the armature is greater, it follows from the law just discovered that the electromotive action of the magnet upon one and the same particle of the wire decreases in the simple ratio of the distance. This is as it were the reversal of the law demonstrated by Biot in the field of electro-magnetism, which, as is known, states that the action of an electric closing wire upon a magnetic needle decreases in the simple ratio of the distance; and it follows from our experiments as from those of Biot, that the action of a particle of the electric currents which encircle the magnet upon every particle of the spiral, is in the inverse ratio of the squares of the distance.

It also immediately follows from the law just demonstrated that the electric current produced in the various wire rings which inclose the armature, by its removal from the magnet, is in the inverse ratio of the diameter of the rings; for the electromotive power is the same in every ring, but the resistance it suffers in being conducted increases as the diameter of the rings; therefore the electric current, the quotient of the electromotive power, by the resistance it suffers, decreases as the diameter of the rings increases.

III. *Influence of the Thickness of the Wire of the Electromotive Spirals on the Electromotive Power produced in them.*

I have also again made these experiments with the horseshoe magnet, since in this case the convolutions of the wires had always the same magnitude. I here employed ten convolutions, which I formed from the wires No. 1, No. 3, and No. 4, and in which the diagonals were in the same proportion as the numbers 233 : 839 : 1661. The entire length of the convolutions in each sort was 33 inches. The deviations are contained in the following table.

	Angle of deviation.				Mean.	
	1	2	3	4		
Spirals from No. 1.	39·3	40·4	35·1	37·8	38·15	38·19
	39·3	40·4	35·2	38·8	38·22	
Spirals from No. 3.	36·8	39·6	40·2	42·0	39·65	39·60
	36·4	39·4	40·4	42·0	39·55	
Spirals from No. 4.	40·5	42·4	37·5	39·3	39·92	39·74
	40·3	40·4	37·5	40·1	39·57	
Spirals from No. 1.	38·6	40·6	35·7	37·8	38·17	38·00
	38·7	40·0	35·2	37·4	37·82	

If we now combine the observations No. 1, at the beginning and end of the series of experiments, and take their mean, we have the following deviations:

For No. 1 the deviation or $\alpha = 38·1$,
— No. 3 ———— or $\alpha' = 39·6$,
— No. 4 ———— or $\alpha'' = 39·7$.

From the proportion of the diagonals in which that of the wire of the multiplier is expressed by 274, we find the following reduced lengths (referred to the wire of the multiplier or No. 2) of our three spirals,

$\lambda \ = 38{\cdot}81$ therefore $\mathrm{L} + l + \lambda \ = 712{\cdot}06$,
$\lambda' = 10{\cdot}78$ ——— $\mathrm{L} + l + \lambda' = 684{\cdot}03$,
$\lambda'' = \ 5{\cdot}44$ ——— $\mathrm{L} + l + \lambda'' = 678{\cdot}69$,

the equation (A.) gives therefore

$$\begin{aligned} x \ &= 712{\cdot}06 \cdot p \cdot \sin.\ (19^\circ\ 3'), \\ x' &= 684{\cdot}03 \cdot p \cdot \sin.\ (19\ \ 48\ ), \\ x'' &= 678{\cdot}69 \cdot p \cdot \sin.\ (19\ \ 51\ ), \end{aligned}$$

or, if the two last electromotive powers be compared with the first, the proportions

$$\frac{x}{x'} = \frac{712{\cdot}06 \cdot \sin.\ (19^\circ\ 3')}{684{\cdot}03 \cdot \sin.\ (19^\circ\ 48')} = 1{\cdot}00305 \quad \text{and}$$

$$\frac{x}{x''} = \frac{712{\cdot}06 \cdot \sin.\ (19^\circ\ 3')}{678{\cdot}69 \cdot \sin.\ (19^\circ\ 51')} = 1{\cdot}0085.$$

Both propositions differ so little from unity that we are fully warranted in concluding that the electromotive power which the magnet produces in the wire No. 1 is quite as strong as those in the wires Nos. 3 and 4, although the latter possesses a diagonal almost four and seven times greater, and therefore that the electromotive power is independent of the thickness of the wires. A second confirmation of this position is found in the following experiment previously made:

	Angle of Deviation.				Mean.	
	1	2	3	4		
10 Conv. of wire No. 3	36·3	37·8	33·5	35·7	35·82	
——— No. 2	36·0	37·0	32·1	34·9	34·0	34·95
	35·4	36·8	32·6	35·0	35·9	
——— No. 3	33·6	35·5	35·7	37·3	35·52	

Consequently we have for

No. 2, $\alpha = 34{\cdot}95$, further $\lambda = 34{\cdot}00$, also $\mathrm{L} + l + \lambda = 707{\cdot}25$
No. 3, $\alpha' = 35{\cdot}67$, ——— $\lambda' = 11{\cdot}52$, and $\mathrm{L} + l + \lambda' = 684{\cdot}77$,

consequently

$$\frac{x}{x'} = \frac{707{\cdot}25 \cdot \sin.\ (17^\circ\ 29')}{684{\cdot}77 \cdot \sin.\ (17^\circ\ 50')} = 1{\cdot}013.$$

Here also the proportion is so near to unity that we may from this, combined with the above results, regard it as an established truth, that "*the electromotive power produced in the spirals by the magnet remains the same for every thickness of the wires, or is independent of it.*"

From this law again it immediately follows that in rings of wires of various thickness surrounding the armature of the magnet, the *electric* *current* produced by its removal is directly as the diagonals of the wires; for the electromotive power remains the same, but the resistance it experiences in being conducted decreases inversely as the diagonals; consequently the electric currents, or the quotients of the electromotive powers by the diagonals, increase as the diagonals.

IV. *On the Influence of the Substance of the Wires on the Electromotive Power produced in the Spirals.*

Nobili and Antinori have in their first paper on the electrical phænomena produced by the magnet (Poggendorff's *Annalen*, 1832, No. 3) already determined the order in which four different metals are adapted to produce the electric current. They arrange them in the following order,—copper, iron, antimony, and bismuth.

It is particularly striking that this order is the same as that which the above metals occupy also in reference to their capacity of conducting electricity; and the idea suddenly struck me whether the electromotive power of the spirals did not remain the same in all metals, and whether the stronger current in the one metal did not arise from its being a better conductor of electricity than the other. With this view, therefore, I examined four metals, namely, copper, iron, platina, and brass, and pursued the following course: In order to avoid entirely the influence of different conduction, I brought at the same time into the metallic conducting circle through which the electric current had to pass, two spirals, equal in all respects excepting that they were of different metals, binding the one end of the first with the one conducting wire, the one end of the second with the other conducting wire, and connected the two ends of the spirals which had remained free with a distinct copper connecting wire. I now brought first the one spiral upon the iron armature of the horseshoe magnet, and proceeded in the same way with it as in the former experiments, and then the other. In this manner the resistance which the electric current suffered in each process was naturally quite the same.—I must also remark that I carefully avoided all thermo-electric disturbing forces, as I surrounded the places of connection of the various wires with several layers of blotting-paper, and after having arranged the apparatus I always waited several hours in order to give the places of connection time to take the temperature of the room.

The experiments themselves are as follows:

		Angle of deviation.				Mean.
		1	2	3	4	
		°	°	°	°	°
Copper and iron spirals	Copper spir. on the armature	17·3	17·4	17·6	17·7	17·500
	Iron spirals	17·3	17·6	17·5	17·9	17·575
		17·3	17·4	17·6	18·1	17·600
	Copper spirals	17·4	17·4	17·8	18·2	17·700
Copper and platina spirals	Copper spir. on the armature	15·2	15·4	15·8	15·8	15·550
	Platina spirals	15·7	15·4	15·9	15·4	15·600
		15·7	15·4	15·8	15·4	15·575
	Copper spirals	15·4	15·8	15·3	15·9	15·600
Copper and brass spirals.	Brass spir. on the armature	18·4	18·3	18·4	18·2	18·350
	Copper spirals	18·5	18·2	18·1	18·3	18·275
		18·4	18·2	18·5	18·4	18·375
	Brass spirals	18·4	18·3	18·3	18·3	18·325

If we now combine the single means together and convert the decimals of the degrees into minutes, we obtain from this table the following results:

Copper spirals, deviation .... $\alpha = 17^\circ\ 36'{\cdot}0$
Iron spirals, ——— .... $\alpha' = 17\ \ 35\,{\cdot}2$

Copper spirals, deviation .... $\alpha = 15^\circ\ 34'{\cdot}5$
Platina spirals, ——— .... $\alpha' = 15\ \ 35\,{\cdot}2$

Copper spirals, deviation .... $\alpha = 18^\circ\ 19'{\cdot}2$
Brass spirals, ——— .... $\alpha' = 18\ \ 20\,{\cdot}2$

Since in this case the resistances remain the same for every pair of observations, our chief equation (A.) gives, when treated as before, the following proportions of the electromotive powers, if we designate them for copper, iron, platina, and brass with x, x', x'', x''':

$$\frac{x}{x'} = \frac{\sin.\ (8^\circ\ 43'{\cdot}0)}{\sin.\ (8^\circ\ 42'{\cdot}6)} = 1{\cdot}00033,$$

$$\frac{x}{x''} = \frac{\sin.\ (7^\circ\ 47'{\cdot}2)}{\sin.\ (7^\circ\ 47'{\cdot}6)} = 0{\cdot}99912,$$

$$\frac{x}{x'''} = \frac{\sin.\ (9^\circ\ \ 9'{\cdot}6)}{\sin.\ (9^\circ\ 10'{\cdot}2)} = 0{\cdot}99894.$$

These three proportions are all of them so near to unity that there will exist no doubt as to the fact, that wires of copper, iron, platina, and brass suffer one and the same electromotive action; and that I may be allowed to extend the same position by analogy, even to all other metals and substances in general, until direct experiments shall have left the matter beyond all doubt. We shall have therefore the law,

"*that the electromotive power which the magnet produces in spirals* *of wires of different substances, but in every other respect placed in exactly the same circumstances, is completely the same for all these substances*"*.

Hence again it immediately follows that in two perfectly equal wire rings of different substance, surrounding the magnetic armature, the electric currents which are produced by taking the armature off or placing it on the magnet, are in direct proportion as the capacities of the substances for conducting electricity. Silver and copper wires therefore are the most advantageous.

From the latter observations we shall easily be able to deduce the capacity of the four metals for conducting, if we make a second similar observation, in which instead of bringing into the circle of the electric current two spirals of different metals, we make use of two of the already used copper spirals, and then place either of them on the armature, and determine the angle of deviation. Let this angle be called α; and, for the other spirals, in the order in which they followed in the observation (therefore the copper spiral, with that of iron, platina, and brass), let these angles be designated by α', α'', and α'''. Further, let the combined lengths of the wire of the multiplier of the conducting wires and that of the connecting wire of both spirals (all reduced to the diameter of the wire of the multiplier) be called L; but the lengths, which are equal in all the spirals, reduced also to the same diameter, be λ; we will further designate by 1, m', m'', m''', the conductive power of the metals in the above order, where that of the copper is also expressed by 1.

If we take the general formula (A.), namely

$$x = (\mathrm{L} + l + \lambda) p \cdot \sin. \tfrac{1}{2} \alpha,$$

we must here, since the wires are no longer of the same kind, substitute for the resistance $(\mathrm{L} + l + \lambda)$, the resistances †

$$(\mathrm{L} + \lambda), \left(\mathrm{L} + \frac{\lambda}{m'}\right), \left(\mathrm{L} + \frac{\lambda}{m''}\right), \text{ and } \left(\mathrm{L} + \frac{\lambda}{m'''}\right),$$

since they stand in inverse proportion to the capacities of conduction; we have therefore four equations (in which, according to the law just found, $x' = x'' = x'''$ are $= x$),

$$x = (\mathrm{L} + \lambda) p \cdot \sin. \tfrac{1}{2} \alpha$$

$$x = \left(\mathrm{L} + \frac{\lambda'}{m'}\right) p \cdot \sin. \tfrac{1}{2} \alpha'$$

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* After I had made the above experiment I saw from No. 5 of Poggendorff's *Annalen*, which I had then just received, that this last law had already been demonstrated, although in a different way, by Faraday. My experiment may therefore serve to confirm it.

† In the following expressions I consider the resistance l jointly with that of L, since in the multiplier last employed the conducting wires consisted of one piece with the wire of the multiplier, therefore $\mathrm{L} + \lambda$ must always remain constant.

$$x = \left(\mathrm{L} + \frac{\lambda''}{m''}\right) p \cdot \sin. \tfrac{1}{2}\,\alpha''$$

$$x = \left(\mathrm{L} + \frac{\lambda'''}{m'''}\right) p \cdot \sin. \tfrac{1}{2}\,\alpha''',$$

consequently, by division,

$$1 = \frac{\mathrm{L} + \lambda}{\mathrm{L} + \dfrac{\lambda}{m'}} \cdot \frac{\sin. \frac{1}{2}\,\alpha}{\sin. \frac{1}{2}\,\alpha'} \text{ or } \mathrm{L} + \frac{\lambda}{m'} = (\mathrm{L} + \lambda)\frac{\sin. \frac{1}{2}\,\alpha}{\sin. \frac{1}{2}\,\alpha'}$$

$$1 = \frac{\mathrm{L} + \lambda}{\mathrm{L} + \dfrac{\lambda}{m''}} \cdot \frac{\sin. \frac{1}{2}\,\alpha}{\sin. \frac{1}{2}\,\alpha''} \text{ or } \mathrm{L} + \frac{\lambda}{m''} = (\mathrm{L} + \lambda)\frac{\sin. \frac{1}{2}\,\alpha}{\sin. \frac{1}{2}\,\alpha''}$$

$$1 = \frac{\mathrm{L} + \lambda}{\mathrm{L} + \dfrac{\lambda}{m'''}} \cdot \frac{\sin. \frac{1}{2}\,\alpha}{\sin. \frac{1}{2}\,\alpha'''} \text{ or } \mathrm{L} + \frac{\lambda}{m'''} = (\mathrm{L} + \lambda)\frac{\sin. \frac{1}{2}\,\alpha}{\sin. \frac{1}{2}\,\alpha'''},$$

from which equations m', m'', and m''' may be found.

For our case, L is = 849 inches, $\lambda = 84{\cdot}1$, $\alpha = 21^\circ\ 52'$, $\alpha' = 17^\circ\ 36'$, $\alpha'' = 15^\circ\ 34'$, $\alpha''' = 18^\circ\ 20'$, hence we have

Capacity of conduction of copper .... = 1·00000,
——————— iron or m'...... = 0·27321,
——————— platina or m'' = 0·18370,
——————— brass or m'''... = 0·32106.

We might still find these values more exactly if the lengths of the wires were greater; but this investigation did not properly come within the scope of this paper, I therefore defer it till another occasion.

Consequences of the Laws already established in respect to the Construction of Electromotive Spirals.

In the following experiments I suppose the magnet for the production of the electric current to be given here, therefore the question is to determine those spirals of a certain metal which act most advantageously with this magnet and its cylindrical armature, which is likewise given. Further, I suppose the spirals, together with their free, not wound ends, to consist of one and the same wire; moreover, it is self-evident that every other property of the ends of the wires not belonging to the electromotive spirals may be reduced to those above mentioned, if we know the length, the diagonal, and the conducting power of the pieces of wire brought into the circle.

It is easy to see that by increasing the convolutions *ad infinitum* we do not also increase the strengths of the current *ad infinitum*. In the first place,—the number of convolutions of a given wire is limited by the length of the cylindrical armature; therefore the further increase of the number of convolutions can only be made by several series of convolutions placed one above another. Let the electromotive power of a series of convolutions which the length of the armature occupies $= \phi$; the length of the wire of all these convolutions, or, in this case, on account of the diameter of the wire being equal throughout, the resistance it offers $= \alpha$; let the length of the necessarily free ends of the wires together $= \beta$, the power therefore of the current of this first series of convolutions is

$$\mu_1 = \frac{\phi}{\alpha + \beta};$$

let γ be the piece of the second series of convolutions by which its length, on account of its necessarily greater diameter, is greater than the length α of the first series, the power of the current from these two series is

$$\mu_2 = \frac{2\,\phi}{2\,\alpha + \gamma + \beta},$$

and in the same manner

$$\mu_3 = \frac{3\,\phi}{3\,\alpha + \gamma + \delta + \beta},$$

where δ designates the quantity by which the first series is surpassed in length by the third. If now the second series of convolutions does not add to the strength of the current, we put $\mu_1 = \mu_2$, therefore

$$\frac{\phi}{\alpha + \beta} = \frac{2\,\phi}{2\,\alpha + \beta + \gamma},$$

whence we have $\beta = \gamma$,

i. e. as soon as the length of the free ends is only equal to the difference between the lengths of the second series of convolutions and those of the first, the second series would then add nothing to the strength of the current. In order to see what three series would do in this case, let us put $\beta = \gamma$ in the expression for μ, and we obtain

$$\mu_3 = \frac{3\,\phi}{3\,\alpha + 2\,\beta + \delta};$$

δ however is now greater than γ or β, we therefore put $\delta = \beta + \mu$, where μ expresses a positive magnitude; we obtain by this

$$\mu_3 = \frac{3\,\phi}{3\alpha + 3\,\beta + \mu} = \frac{\phi}{(\alpha + \beta) + \frac{\mu}{3}}.$$

This last expression for μ_3 is evidently smaller than $\frac{\phi}{\alpha + \beta}$, consequently three series of convolutions would only weaken the action of one or two series (which actions have been here assumed as equal).

In the same manner we find, if three series have not a more powerful action than two

$$\delta = \tfrac{1}{2}(\gamma = \beta),$$

i. e. this happens when the length of the free ends is half as great as half the sum of the differences between the length of the first series and the lengths of the second and third series.

Having thus proved that by increasing the series of convolutions we never obtain a maximum of the electric current, and therefore that a greater increase would only do harm, we proceed to the general consideration of the subject.

We therefore suppose the convolutions of a series of the bespun metallic wire to lie thick on one another. Let the length of the space on which the convolutions may be wound up be $= a$, the thickness of the wire $= b$; let the thickness of the wire covered with silk surpass the thickness of the uncovered wire by the excess β, so that it be $= b + \beta$, the length of a convolution be $= c$, the lengths of the free ends of the wire $= m$; the number of convolutions then which can be wound in one series upon the armature is $= \frac{a}{b + \beta}$ and the length of the wire of these convolutions $= \frac{a}{b + \beta} . c$, and the whole length which the electricity has to run through for one series of convolutions

$$= \frac{a}{(b + \beta)} c + m.$$

If we assume the resistance offered by a wire of the same substance, whose length $= 1$, and whose thickness $= 1$, as unity, the resistance for one series of convolutions becomes

$$= \frac{\frac{a}{b + \beta} c + m}{b^2} = \frac{a c + (b + \beta) m}{b^2 (b + \beta)}.$$

Further, let the electromotive power produced in one convolution, which, according to the second and third of our laws above proved, remains the same for every magnitude of the convolutions and for every thickness of the wire, be called f; the electromotive power produced in a series of convolutions is therefore, according to the first of the above laws,

$$= \frac{a}{b + \beta} . f,$$

and consequently the power of the electric current for a series of convolutions, or

$$p_1 = \frac{a b^2 f}{a c + (b + \beta) m}.$$

We must now for our purpose express the length of a convolution or c

in terms of the diameter of the cylindrical armature, and the thickness of the wire and its silky envelope. We have however the semi-diameter of a convolution, if half the thickness of the iron cylinders is $= q$,

for the first series $= q + \frac{b + \beta}{2}$
—— second —— $= q + \frac{3}{2}(b + \beta)$
—— third —— $= q + \frac{5}{2}(b + \beta)$
—— nth —— $= q + \frac{2n - 1}{2}(b + \beta)$

whence we have the length of a convolution or

c for the first series $= \big(2q + (b + \beta)\big)\pi$
c —— second —— $= \big(2q + 3(b + \beta)\big)\pi$
c —— third —— $= \big(2q + 5(b + \beta)\big)\pi$
c —— nth —— $= \big(2q + (2n - 1)(b + \beta)\big)\pi.$

If we substitute the first value of c in the equation for m, we obtain

$$p_1 = \frac{a b^2 f}{a\pi\big(2q + (b + \beta)\big) + (b + \beta)m}.$$

The electromotive power is for two series of convolutions with regard to the above law, No. 2,

$$= 2\,\frac{a}{b + \beta}\cdot f;$$

but the resistance is equal to that of the two series of convolutions, together with the piece m, therefore

$$= \frac{\frac{a}{b + \beta}(2q + b + \beta)\pi + \frac{a}{b + \beta}\big(2q + 3(b + \beta)\big)\pi + m}{b^2}$$

$$= \frac{a\pi\big(4q + 4(b + \beta)\big) + m(b + \beta)}{b^2(b + \beta)},$$

consequently the power of the current for two series, or

$$p_2 = \frac{2\cdot\frac{a}{b + \beta}\cdot f}{\frac{a\pi\big(4q + 4(b + \beta)\big) + m(b + \beta)}{b^2(b + \beta)}}$$

$$= \frac{2\,a\,b^2 f}{a\pi\big(4q + 4(b + \beta)\big) + m(b + \beta)}.$$

In the same manner we find

$$p_3 = \frac{3\,a\,b^2 f}{a\pi\,(6\,q + 9\,(b + \beta)\,) + m\,(b + \beta)}$$

$$p_4 = \frac{4\,a\,b^2 f}{a\,\pi\,(8\,q + 16\,(b + \beta)\,) + m\,(b + \beta)}$$

$$p_n = \frac{n\cdot a\,b^2 f}{a\pi\,(2\,n\,q + n^2\,(b + \beta)\,) + m\,(b + \beta)} \quad . \; . \quad \text{(D.)}$$

If I differentiate this general expression of the power of the current for n series of convolutions in regard to n, I obtain

$$\frac{d\cdot p_n}{d\,n} = a\,b^2 f\,\frac{a\pi(2nq + n^2(b+\beta)) + m(b+\beta) - a\pi n(2q + 2n(b+\beta))}{[a\,\pi\,(2\,n\,q + n^2\,(b + \beta)\,) + m\,(b + \beta)]^2}$$

If I put this expression $= 0$, we have after some reductions

$$m - a\,\pi\,n^2 = 0,$$

consequently $$n = \sqrt{\left(\frac{m}{a\,\pi}\right)}.$$

I take here the positive sign of the root, because n according to its nature cannot be negative, and m, a, π, are all three positive.

If we further develope $\frac{d^2\,p_2}{d\,n^2}$ and put in the expression found this value of $n = \sqrt{\left(\frac{m}{a\,\pi}\right)}$ we obtain a negative magnitude; consequently this value of n represents a maximum of the current.

From the discovered value of n for the maximum of the current, we can infer

1. That the maximum of the action of the magnet on our spirals, for every thickness of the wire, is attained by the same number of series of convolutions; for n is independent of $b + \beta$.

2. That the longer the free ends of the spirals are, or the greater m is, the greater is the number of the series of convolutions required in order to attain the maximum of the action.

3. That the longer the space a is on which the convolutions may be wound round in one series, the less number of series of convolutions are necessary to produce the greatest current.

4. That the maximum is independent of q, i. e. that it is quite indifferent for the number of series of convolutions necessary to the attainment of the maximum, whether they are immediately wound round the cylinder of iron, or round another cylinder which is placed on the other one.

If we put the above found value $n = \sqrt{\left(\frac{m}{a\,\pi}\right)}$ in the general expression of the power which is contained in the equation (D.), we obtain after some reductions, as the expression for the maximum which is attained by the current,

$$p_{(\text{maximum})} = \frac{b^2 f}{2\left(\pi q + (b+\beta)\sqrt{\left(\frac{\pi m}{a}\right)}\right)} \quad . \; . \; (\text{E.})$$

This expression again shows:

1. That the maximum of the current stands in direct proportion to f, i. e., to the power of the magnet, or rather to the strength of the magnetism which is produced in the armature by the placing on of the magnet, and which again vanishes.

2. The maximum is more powerful for a thick wire than for a slender one, for we can bring its expression to the form

$$\frac{f}{\frac{A}{b^2} + \frac{B}{b}},$$

which shows that the whole expression increases with the increase of b.

3. The maximum decreases with q, i. e. it becomes so much the smaller according to the greatness of the cylinder on which the first series of convolutions is wound, it being assumed that the armature does not on that account become greater.

4. It becomes smaller with the increase of m, i. e. the greater the free connecting ends of the spirals are, the smaller is the ultimate attainable maximum of the current.

5. Finally, the maximum increases when a increases, i. e. when the space of the armature upon which a series of convolutions can be wound becomes greater.

We shall consider the power of the current of a single convolution wound round the armature to be the same as m, then as soon as we put in the general expression (D.) for the current $n = 1$, and $a = b + \beta$, we find

$$p_{(\text{a convolution})} = \frac{b^2 f}{\pi(2q + b + \beta) + m}.$$

If we divide the expression for the maximum of the current (E.) by this, we may designate the quotients as *the maximum of increase*, and find that

the maximum of the increase is $$\frac{2q + (b+\beta) + \frac{m}{\pi}}{2q + 2(b+\beta)\sqrt{\left(\frac{m}{a\pi}\right)}} \quad . \; . \; (\text{F.})$$

If I propose to find, for instance, with how many series of convolutions I attain the maximum of the current for my magnet and armature, when I take a length of 850 English inches for the wire of the multiplier and the connecting wires together, I have

$a = 1{\cdot}6$, $b + \beta = 0{\cdot}065$ (wire No. 4) $q = 0{\cdot}335$, $m = 850$.

The formula $n = \sqrt{\frac{m}{a\pi}}$ gives for $n = 13{\cdot}07$, and the formula (F.) gives the maximum of increase $= 114{\cdot}8$. We shall obtain therefore the maximum of the current at somewhat about thirteen series of convolutions, and the current then becomes about 115 times stronger than when produced by one convolution.

We will here separately consider the case in which $m = o$, i. e. where the spirals have no free ends, but close in themselves. If we put $m = o$ in the expression of the current for one convolution, for one series of convolutions, and for n series of convolutions, we shall then obtain

$$\text{for a single convolution} = \frac{b^2 f}{2q\pi + \pi(b + \beta)}$$

$$\text{for a series of convolutions} = \frac{b^2 f}{2q\pi + \pi(b + \beta)}$$

$$\text{for } n \text{ series of convolutions} = \frac{b^2 f}{2q\pi + n\pi(b + \beta)}$$

whence it follows that here the current in one convolution is just as strong as in a series consisting of any number of convolutions; and that in both these cases it is stronger than when several series of convolutions cross one another (for n is quite a positive number). The expression of the current for a convolution may moreover be exhibited thus

$$\frac{f}{\frac{(2q + b + \beta)\pi}{b^2}}$$

i. e. it is equal to the electromotive power, divided by the resistance offered by a convolution; and in effect it is evident that in this case of $m = o$ a series of convolutions must act just in the same manner as a single convolution; for with the increase of the number of convolutions the electromotive power and the resistance become increased in the same proportion, consequently the quotient of the one by the other, or the electric current remains unchanged. It is also now evident that in effect a second series of convolutions can only weaken the current, since in the second series the electromotive power increases as in the first, with the increase of the number of convolutions; while, on the contrary, the resistance is greater in the two series than double the same in one series, on account of the enlarged diameter.

But there is one phænomenon of electro-magnetism to which all the above positions however cannot yet be applied, namely, to the production of the spark. This occurs then only, when the metallic conductor of the current is disturbed at some place; there enters therefore into the circular passage of the current an intermediate conductor, whose length is almost indefinitely small, but whose resistance is almost indefinitely great. We must therefore, in order to apply the above-developed formulæ, first be in a condition to reduce this intermediate conductor to a certain length of wire, with the diameter of the wire given, and thus to determine m;—but for this reduction we are yet in want of the data.

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Vols I.–VI.
Dulau 16.
March 1880.

PRINTED BY RICHARD AND JOHN E. TAYLOR, RED LION COURT, FLEET STREET.

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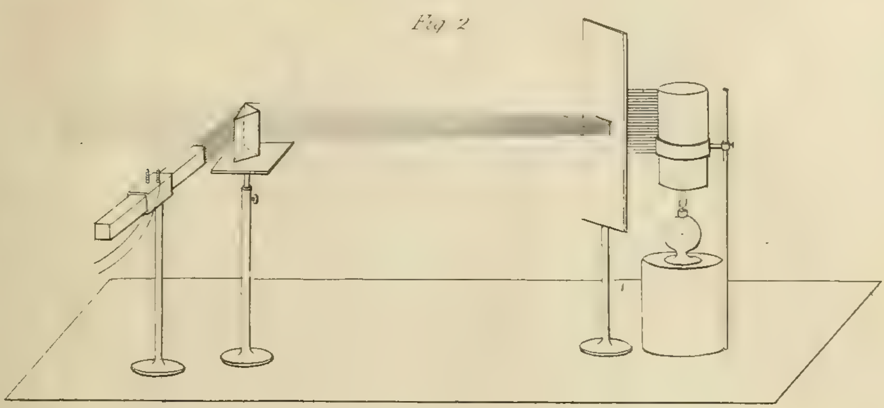

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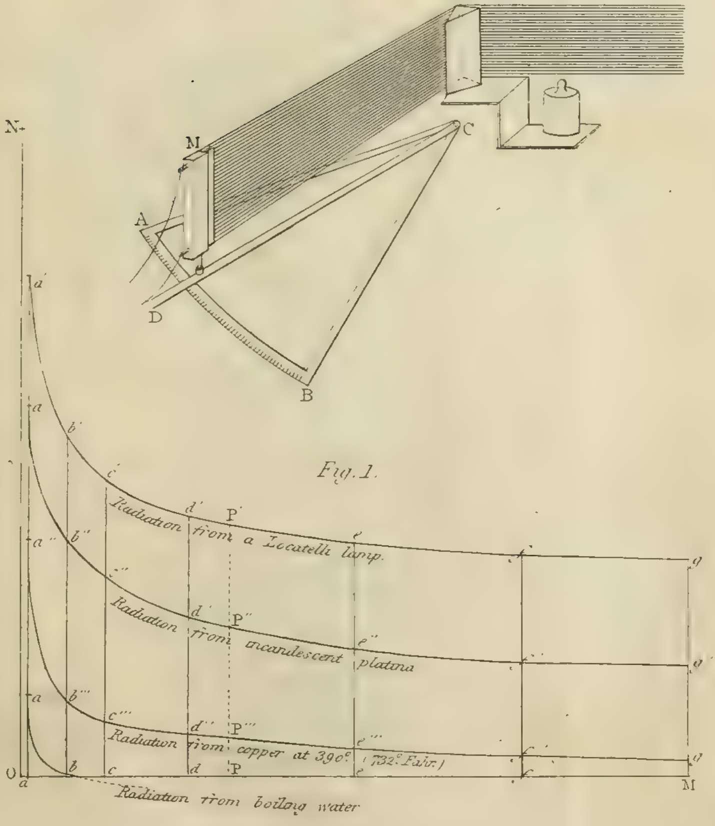

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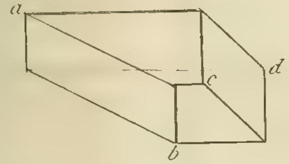

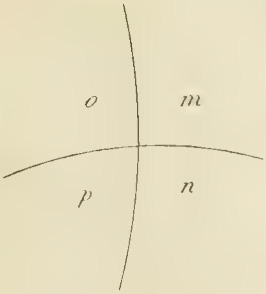

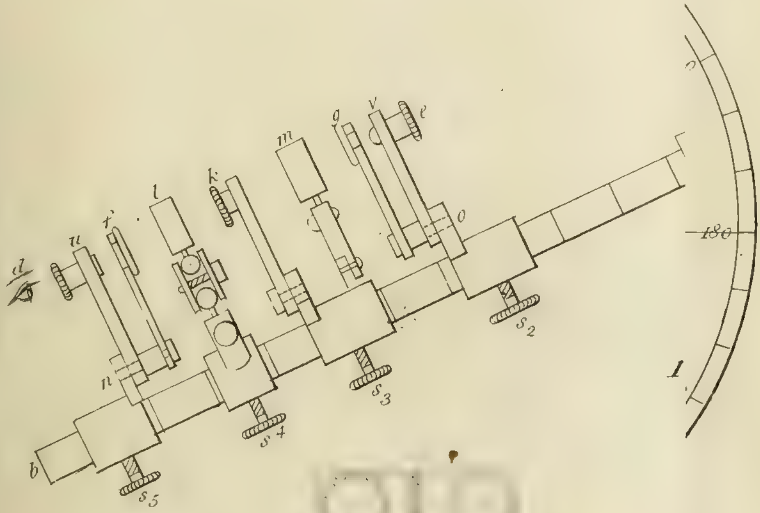

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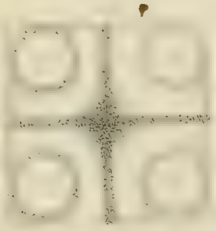

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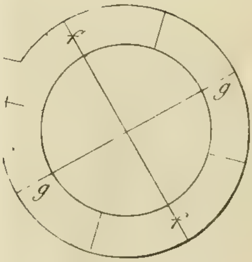

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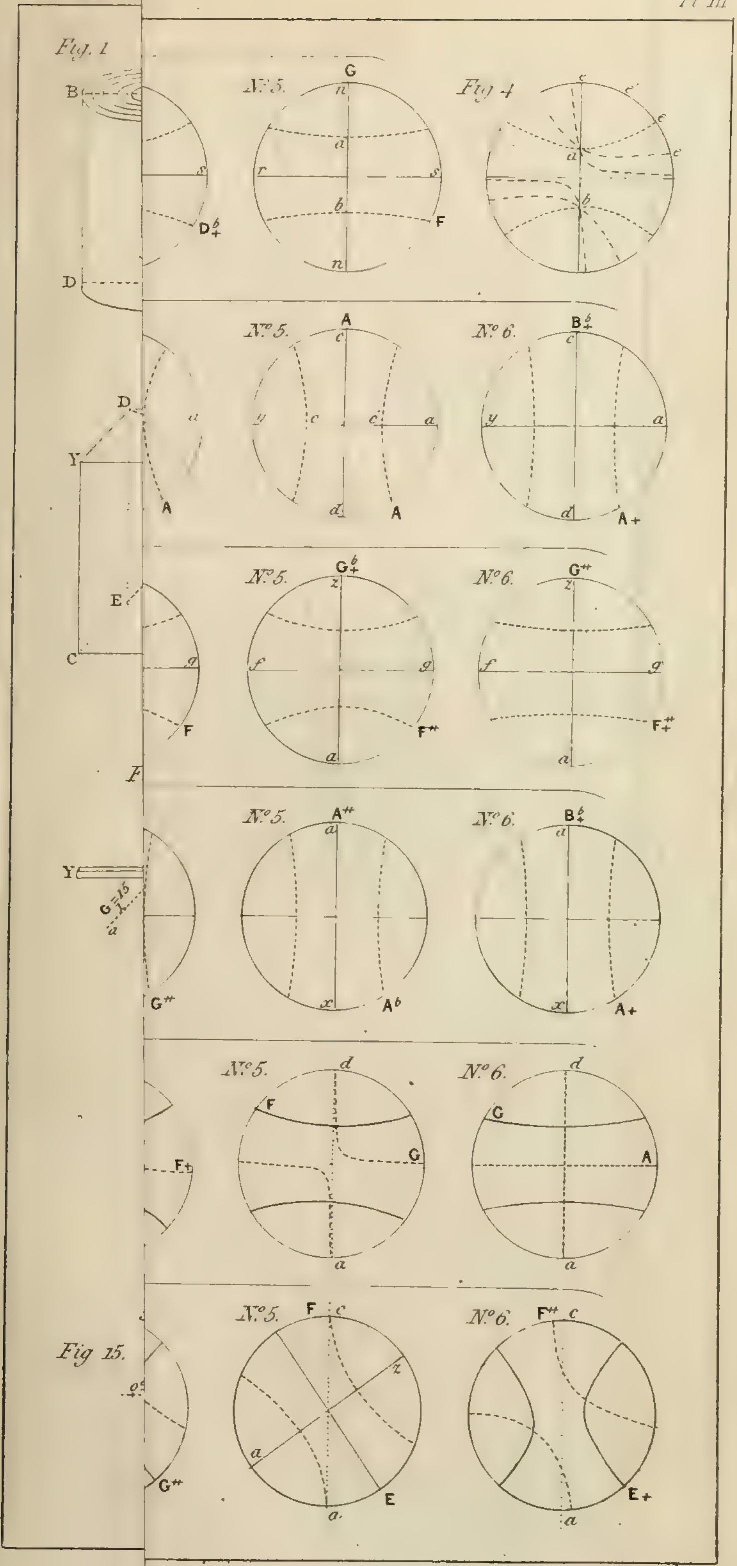

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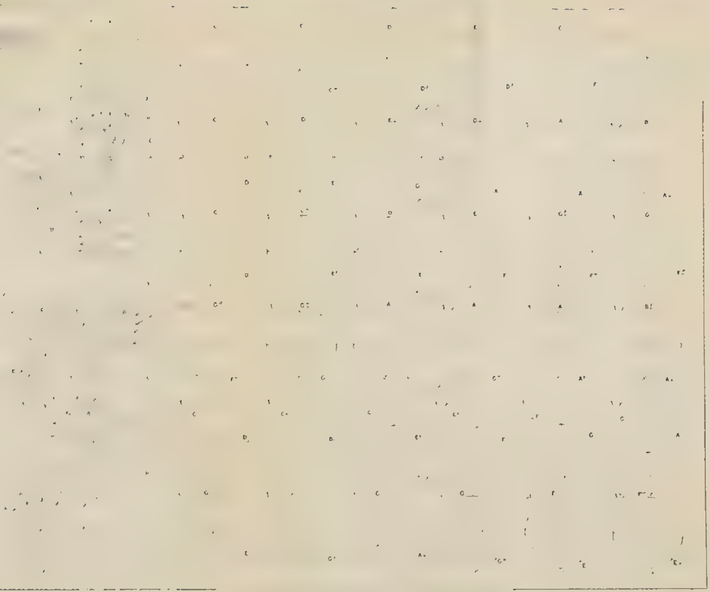

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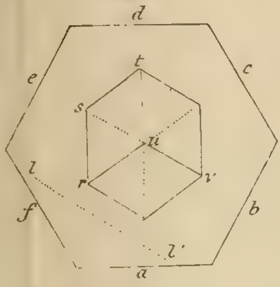

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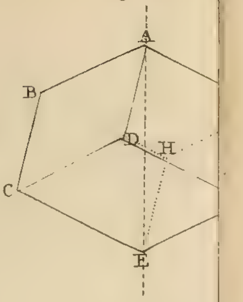

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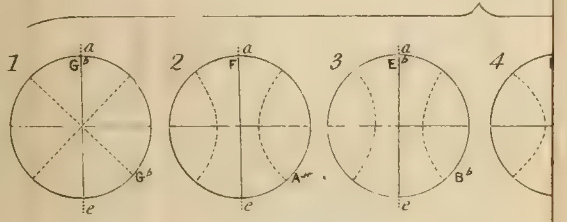

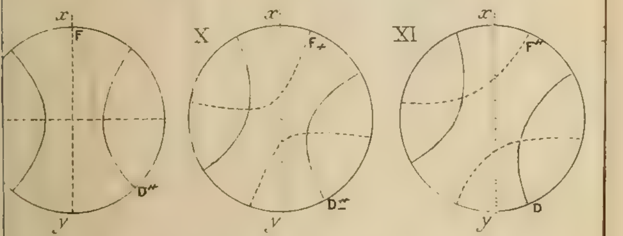

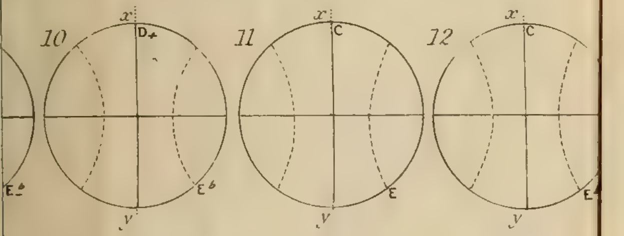

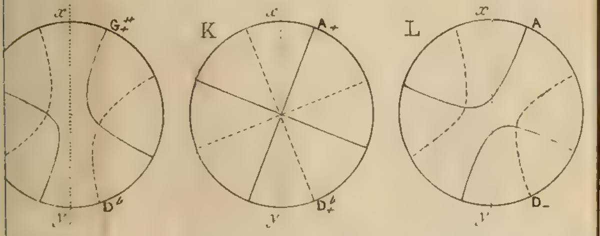

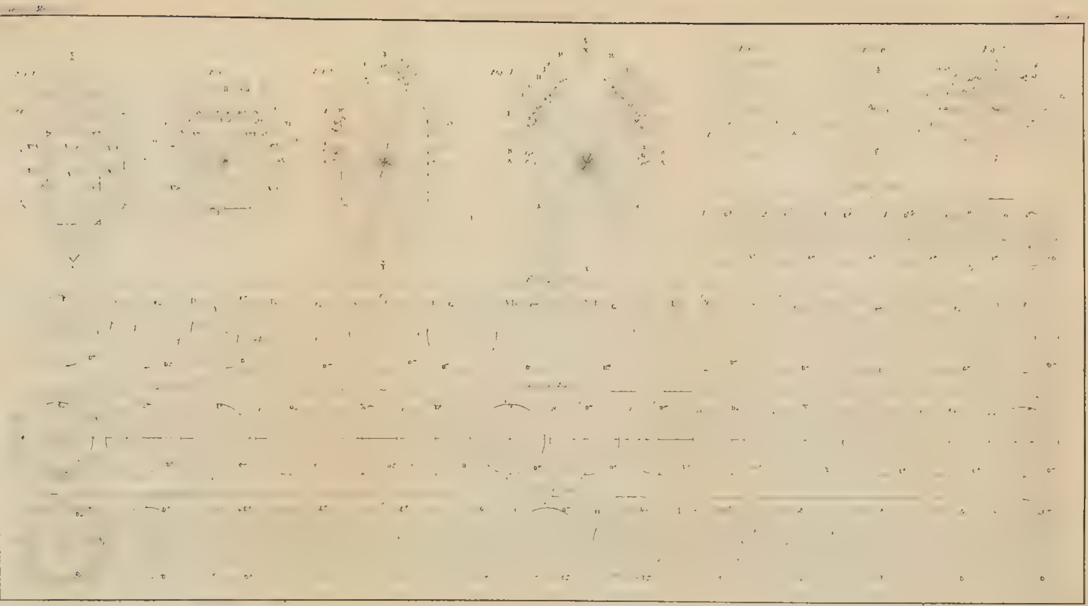

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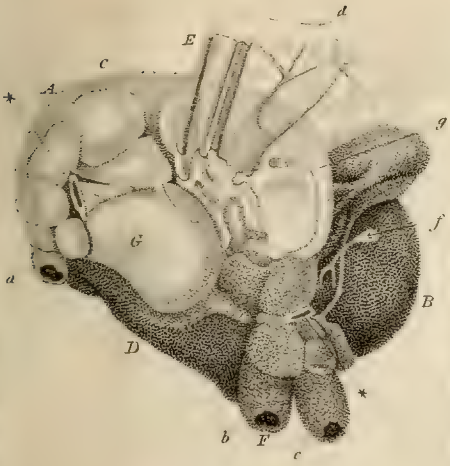

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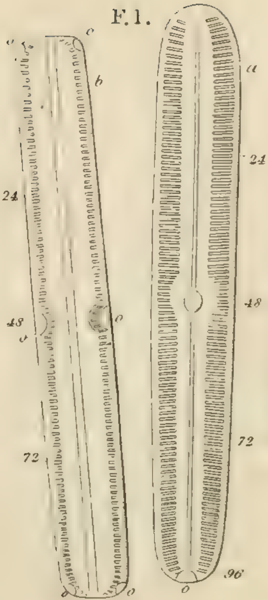

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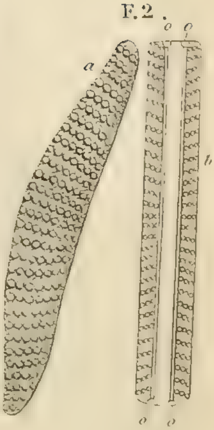

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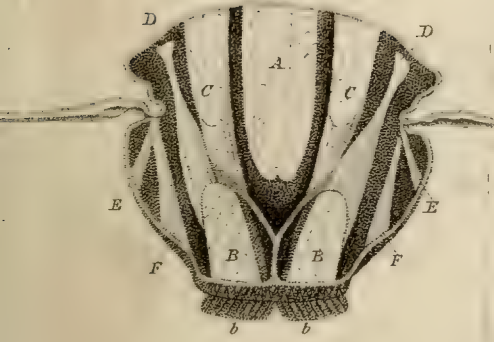

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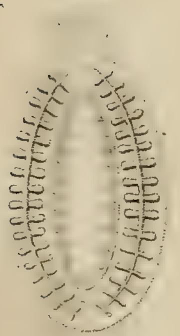

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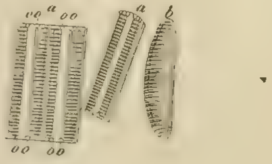

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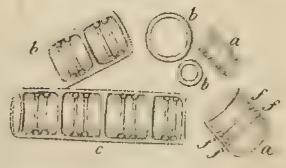

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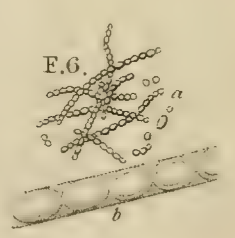

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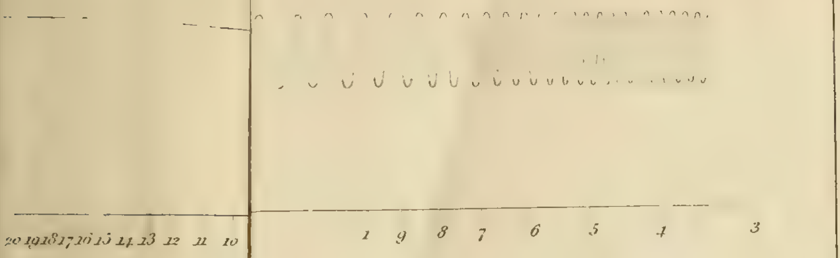

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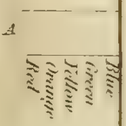

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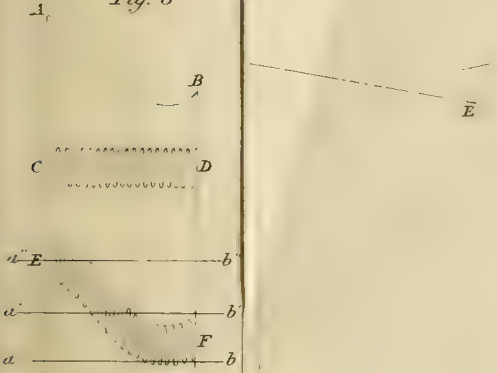

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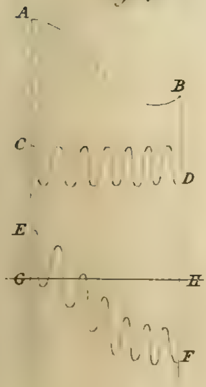

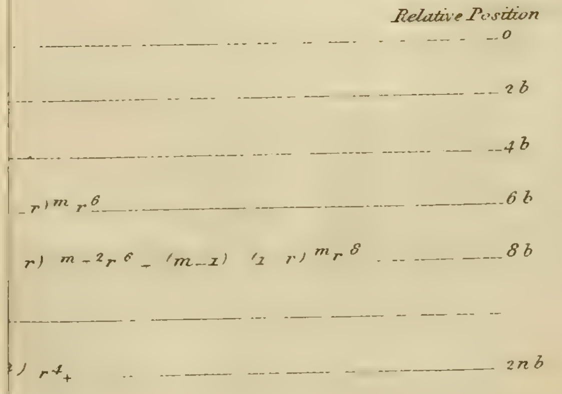

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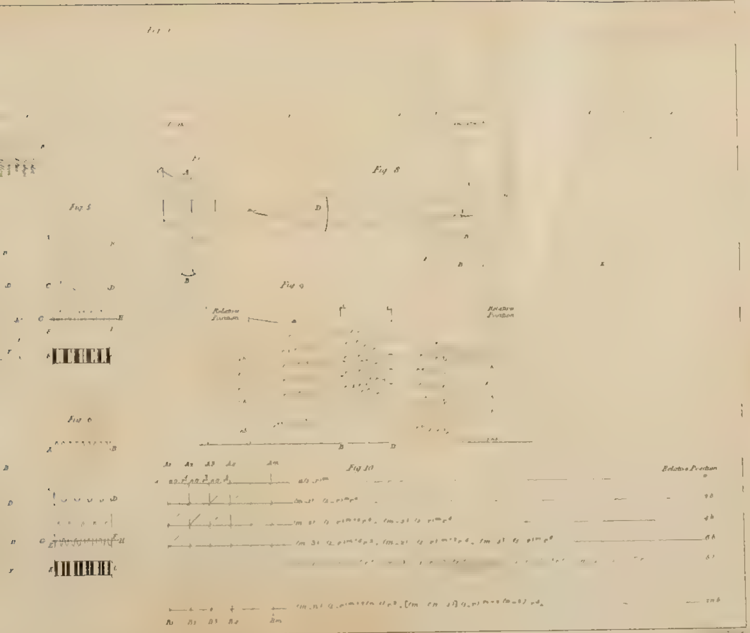

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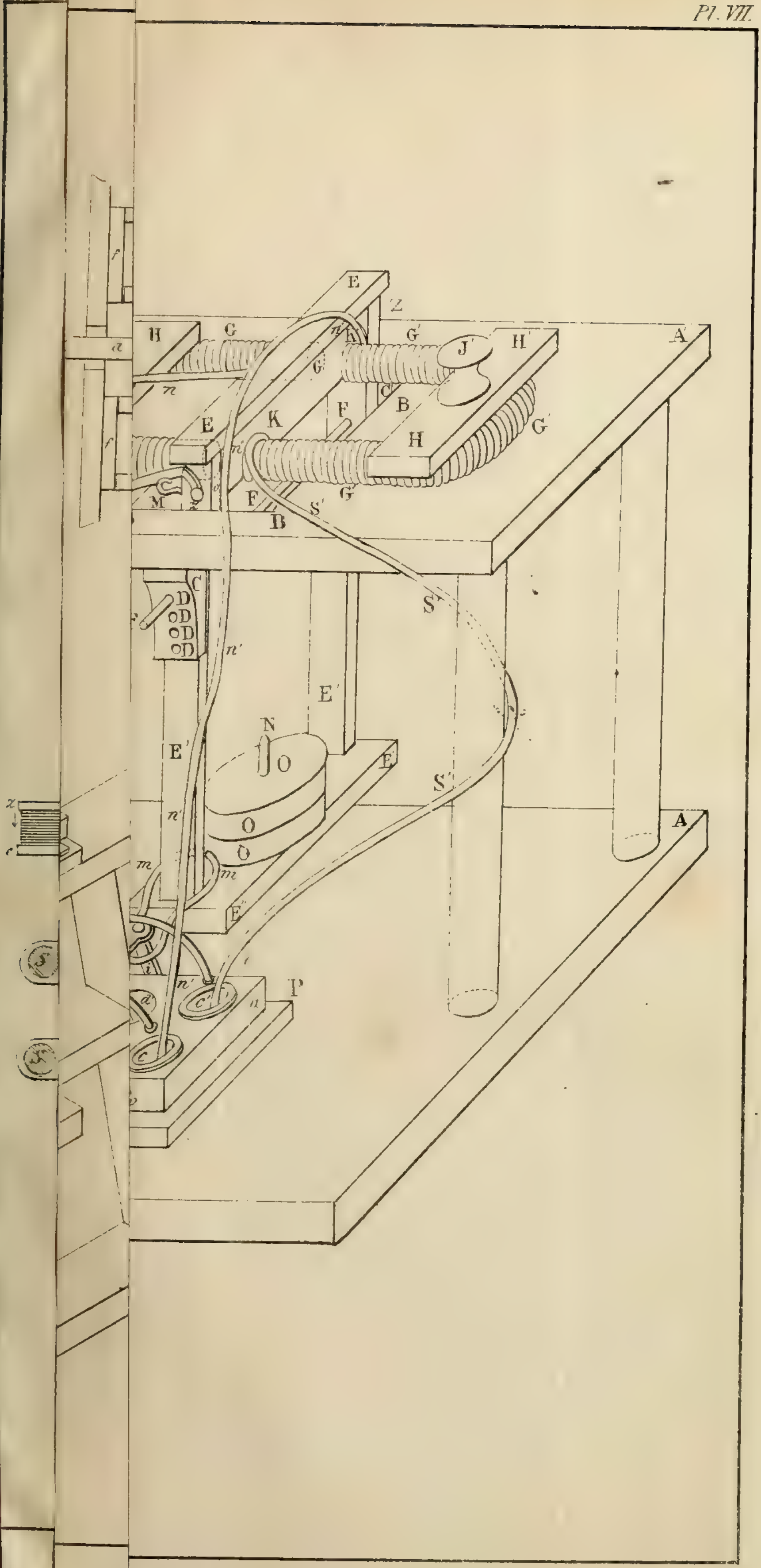

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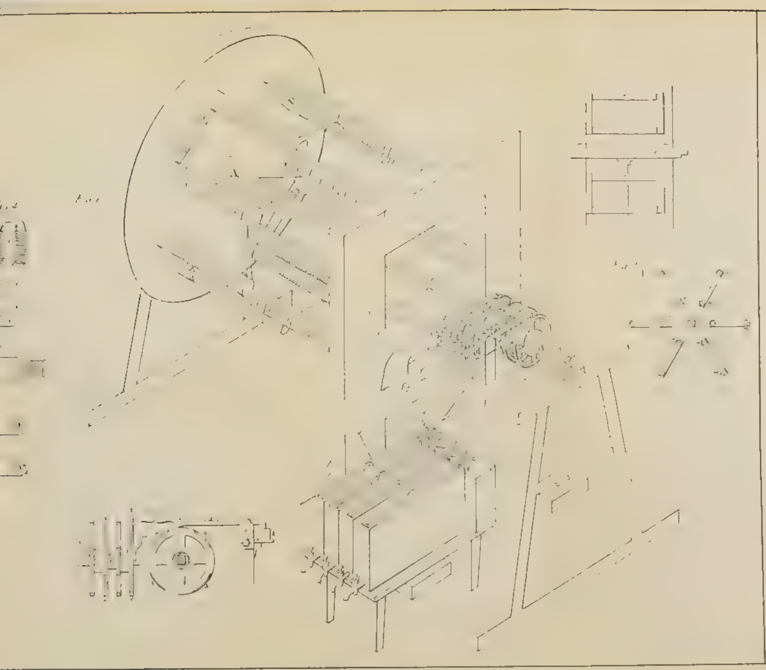

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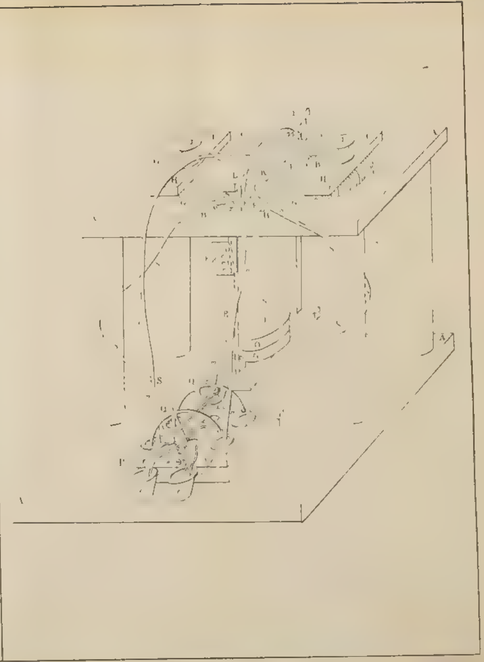

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