REPORT

OF THE

THIRD MEETING

OF THE

BRITISH ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE;

HELD AT CAMBRIDGE IN 1833.

LONDON:

JOHN MURRAY, ALBEMARLE STREET.

1834.

ALERE FLAMMAM

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PREFACE.

THE Transactions of the British Association consist of three parts; first, of Reports on the State of Science drawn up at the instance of the Association; secondly, of Miscellaneous Communications to the Meetings; and thirdly, of Recommendations by the Committees, having for their objects to mark out certain points for scientific inquiry.

It is proper to remark, that some of the Reports here printed are to be considered in the light of *first parts* of the intended survey of the sciences reviewed in them, the continuation being postponed to a future Meeting. Thus, the Report on *Hydraulics*, by Mr. G. RENNIE, will be completed in a second part, to be presented to the Meeting at Edinburgh; the Report on the mathematical theory of the same science, by the Rev. Mr. CHALLIS, which is here restricted to problems on the common theory of Fluids, will be further extended to the theories which have recently been advanced respecting the internal constitution of Fluids and the state of their *caloric*, to account for certain phænomena of their equilibrium and motion; and the Report on *Analytical Science*, by the Rev. Mr. PEACOCK, which in the present volume includes Algebra, and the application of Algebra to Geometry, is intended to be hereafter concluded by a review of the Differential and Integral Calculus and the theory of Series. In like manner, to the Report on *Botany*, by Dr. LINDLEY, which embraces only the physiological part of the science, that which Mr. BENTHAM has undertaken on the State and Progress of *Systematic Botany* will be supplemental; and to the present Report, by Dr. CHARLES HENRY, on one branch of *Animal Physiology*, a more general review of the progress of that science will be added by the Rev. Dr. CLARK.

With respect to the next part of the Transactions, which includes the communications made to the Sections, two rules have been adopted; the first is, to print no *oral* communications unless furnished or revised by the Author himself. In the former volume this rule was slightly deviated from, for the purpose of showing in what manner the Meetings were conducted. But however valuable a part of the proceedings of the Meetings the verbal communications and discussions may be, it is evidently impossible to publish a safe and satisfactory report of them from any *minutes* which can be taken. The second rule is, not to print any of the miscellaneous communications *at length*; but either *abstracts* of them, or *notices** only, the object of the rule being to keep the Transactions within the bounds which the Association has prescribed to itself, and to prevent any interference with the publications of other societies. In the present volume, there is one paper printed at length†, which contains the results of certain experiments *instituted expressly at the request of the Association.*

The Recommendations of various subjects for scientific inquiry agreed upon at Cambridge have been here incorporated with those adopted at former Meetings, and the Suggestions which are contained in the Reports on the state of science, published in the present and preceding volume, have likewise been added; so as to present a general view of the *desiderata* in science to which attention has been invited. To this part of the volume are also appended those directions for the use of observers which have proceeded from Committees appointed to promote particular investigations.

To the Transactions is prefixed a brief outline of the General Proceedings of the Cambridge Meeting, a fuller Report of them having been rendered unnecessary by the account which has already issued from the University press. The observations, however, delivered by the Rev. Mr. WHEWELL on the state of science as it is exhibited in the first volume of the Reports of the Association, not having been before published, are printed at length.

* The *notices* of Communications will be found in the general account of the Proceedings of the Sections, p. 353.

† "Experiments on the Quantity of Rain which falls at different Heights in the Atmosphere."

CONTENTS

TREASURER'S ACCOUNT.

Dr.

1832.	£	s.	d.
Balance from 1831	192	10	11
Compositions, 1832	605	0	0
Subscriptions	461	0	0
Arrears of Subscriptions, 1831	9	0	0
Compositions, 1833	80	0	0
Subscriptions	33	0	0
Received on Account of Reports	35	2	8
Dividend on Stock, six months	15	0	0
	£1430	13	7

We, the undersigned, having examined the Treasurer's Account of the British Association for the Advancement of Science, do find a Balance in the hands of the Treasurer of 8*l.* 10*s.*; in the hands of the Bankers, 319*l.* 2*s.* 11*d.*; due from Dr. Daubeny, 55*l.* 15*s.*; from the Rev. T. Luby, 24*l.* 16*s.* 4*d.*; amounting altogether to 408*l.* 4*s.* 3*d.*; and also 1000*l.* 3 per cent. Consols.

G. B. GREENOUGH,
WILLIAM SCORESBY, } *Auditors.*

Cambridge, 22*nd June*, 1833.

Cr.

1832.	£	s.	d.	£	s.	d.
Sundry Expenses				136	4	4
Purchase of 1000*l.* in 3 per cent. Consols				836	5	0
Secretary's Salary, six months				50	0	0
Balance in the hands of Bankers, Messrs. Barnetts and Co.	319	2	11			
Ditto Treasurer	8	10	0			
Ditto Dr. Daubeny, Oxford	55	15	0			
Ditto Rev. T. Luby, Dublin	24	16	4			
				408	4	3
				£1430	13	7

JOHN TAYLOR, *Treasurer.*

Cambridge, 22*nd June*, 1833.

THIRD REPORT.

PROCEEDINGS OF THE MEETING.

1833.

THE third Meeting of the British Association commenced its sittings at Cambridge on Monday, the 24th of June, 1833. It was attended by more than nine hundred Members, and was honoured with the presence of several foreign philosophers. The extent of accommodation provided by the University, and by the societies of which it consists, corresponded with the magnitude of the Meeting. The public schools, with two adjoining halls, were allotted to the use of the Sections and Committees, and the Senate-house was appropriated to the reception of the general assemblies; a large proportion of the visitors were lodged within the walls of the Colleges, and the great halls of the two principal foundations were opened in hospitality to a concourse of guests collected from all parts by a common interest in scientific pursuits.

GENERAL MEETING.

On Monday evening, at eight o'clock, the Members assembled in the Senate-house: and a public discussion took place on the phenomena and theory of the Aurora Borealis.

On Tuesday, at 1 P. M. a General Meeting was held in the Senate-house; the President of the preceding year, (the Rev. Dr. Buckland,) resigned his office. In the course of his speech*, he congratulated the Meeting on the proof afforded by the Report recently published, that the Association was pursuing a course of peculiar utility to science, whilst at the

* A fuller account of the speeches delivered at the Meeting will be found annexed to the *lithographed signatures*, &c., published at Cambridge.

same time it had fully redeemed its pledge of not interfering with the province of other Scientific Societies.

The President (the Rev. Professor Sedgwick,) stated, in his opening speech, that it was the desire of the Vice-Chancellor and the Heads of Colleges that everything should be done on the present occasion to emulate, as far as circumstances permitted, the splendid reception which had been given to the Association by the sister University of Oxford. He dwelt on the advantages which such a Meeting brought with it to the places in which it was held, by inducing scientific foreigners to visit them, and expressed the delight with which he hailed such visits, as an omen that the great barriers which for a length of time had served man for man, had now been broken down. He described the character of the Reports which the Association has published; and added that he attached so much value to these expositions of the state of science, that he had requested one of the Secretaries, (the Rev. William Whewell,) to present to the Meeting a fuller analysis of their contents. The President concluded his speech with the following gratifying announcement: "There is a philosopher," he said, "sitting among us whose hair is blanched by time, but possessing an intellect still in its healthiest vigour,—a man whose whole life has been devoted to the cause of truth,—my venerable friend Dr. Dalton. Without any powerful apparatus for making philosophical experiments, with an apparatus, indeed, which many might think almost contemptible, and with very limited external means for employing his great natural powers, he has gone straight forward in his distinguished course, and obtained for himself in those branches of knowledge which he has cultivated, a name not perhaps equalled by that of any other living philosopher in the world. From the hour he came from his mother's womb the God of nature laid his hand upon him, and ordained him for the ministration of high philosophy. But his natural talents, great as they are, and his almost intuitive skill in tracing the relations of material phænomena, would have been of comparatively little value to himself and to society, had there not been superadded to them a beautiful moral simplicity and singleness of heart, which made him go on steadily in the way he saw before him, without turning to the right hand or to the left, and taught him to do homage to no authority before that of truth. Fixing his eye on the most extensive views of science, he has been not only a successful experimenter, but a philosopher of the highest order; his experiments have never had an insulated character, but have been always made as contributions towards some important end, as among the steps towards some lofty generalization. And with a most happy prescience of the points to which the rays of scattered observations were converging, he has more than once seen light while to other eyes all was yet in darkness; out of seeming confusion has elicited order; and has thus reached the high distinction of being one of the greatest legislators of chemical science.

"It is my delightful privilege this day to announce (on the authority of a Minister of the Crown who sits near me,*) that His Majesty, King William the Fourth, wishing to manifest his attachment to science, and his regard for a character like that of Dr. Dalton, has graciously conferred on him, out of the funds of the Civil List, a substantial mark of his royal favour."

The Rev. WILLIAM WHEWELL, being called upon by the President, delivered the following address:—

"The British Association for the Advancement of Science meets at present under different circumstances from those which accompanied its former Meetings. The publication of the volume containing the Reports applied for by the Meeting at York, in 1831, and read before the Meeting at Oxford last year, must affect its proceedings during our sittings on the present occasion; and thus we are now to look for the operation of one part of the machinery which its founders have endeavoured to put in action. Entertaining the views which suggested to them the scheme and plan of the Association, they must needs hope that such an event as this publication will exercise a beneficial influence upon its future career.

"This hope is derived, they trust, from no visionary or presumptuous notions of what institutions and associations can effect. Let none suppose that we ascribe to assembled numbers and conjoined labours extravagant powers and privileges in the promotion of science;—that we believe in the omnipotence of a parliament of the scientific world. We know that the progress of discovery can no more be suddenly accelerated by a word of command uttered by a multitude, than by a single voice. There is, as was long ago said, no royal road to knowledge—no possibility of shortening the way, because he who wishes to travel along it is the most powerful *one*; and just as little is there any mode of making it shorter, because they who press forward are *many*. We must all start from our actual position, and we cannot accelerate our advance by

* The Right Honourable T. Spring Rice.

any method of giving to each man his mile of the march. Yet something we may do: we may take care that those who come ready and willing for the road, shall start from the proper point and in the proper direction;—shall not scramble over broken ground, when there is a causeway parallel to their path, nor set off confidently from an advanced point when the first steps of the road are still doubtful;—shall not waste their powers in struggling forwards where movement is not progress, and shall have pointed out to them all glimmerings of light, through the dense and deep screen which divides us from the next bright region of philosophical truth. We cannot create, we cannot even direct, the powers of discovery; but we may perhaps aid them to direct themselves; we may perhaps enable them to feel how many of us are ready to admire their success, and willing, so far as it is possible for intellects of a common pitch, to minister to their exertions.

"It was conceived that an exposition of the recent progress, the present condition, the most pressing requirements of the principal branches of science at the present moment, might answer some of the purposes I have attempted to describe. Several such expositions have accordingly been presented to the Association by persons selected for the task, most of them eminent for their own contributions to the department which they had to review; and these are now accessible to Members of the Association and to the public. It appears to be suitable to the design of this body, and likely to further its aims, that some one should endeavour to point out the bearing which the statements thus brought before it may and ought to have upon its future proceedings, and especially upon the labours of the Meeting now begun. I am well persuaded that if the President had taken this office upon himself, the striking and important views which it may naturally suggest would have been presented in a manner worthy of the occasion: he has been influenced by various causes to wish to devolve it upon me, and I have considered that I should show my respect for the Association better by attempting the task, however imperfectly, than by pleading my inferior fitness for it.

"The particular questions which require consideration, and the researches which most require prosecution, in the sciences to which the Reports now before you refer, will be offered to the notice of the Sections of the Association which the subjects respectively concern, at their separate sittings. It is conceived that the most obvious and promising chance of removing deficiencies and solving difficulties in each subject, is to be found in drawing to them the notice of persons who have paid a continued and especial attention to the subject. The consideration of these points will therefore properly form a part of the business of the Sectional Meetings; and all Members of the Association, according to their own peculiar pursuits and means, will thus have the opportunity of supplying any wanting knowledge, and of throwing light upon any existing perplexity.

"But besides this special examination of the suggestions which your Reports contain, there are some more general reflexions to which they naturally give rise, which may perhaps be properly brought forward upon this first General Assembly of the present Meeting; and which, if they are well founded, may preside over and influence the aims and exertions of many of us, both during our present discussions and in our future attempts to further the ends of science.

"There is here neither time nor occasion for any but the most rapid survey of the subjects to which your Reports refer, in the point of view in which the Reports place them before you. Astronomy, which stands first on the list, is not only the queen of sciences, but, in a stricter sense of the term, the only perfect science;—the only branch of human knowledge in which particulars are completely subjugated to generals, effects to causes;—in which the long observation of the past has been, by human reason, twined into a chain which binds in its links the remotest events of the future;—in which we are able fully and clearly to interpret Nature's oracles, so that by that which we have tried we receive a prophecy of that which is untried. The rules of all our leading facts have been made out by observations of which the science began with the earliest dawn of history; the grand law of causation by which they are all bound together has been enunciated for 150 years; and we have in this case an example of a science in that elevated state of flourishing maturity, in which all that remains is to determine with the extreme of accuracy the consequences of its rules by the profoundest combinations of mathematics, the magnitude of its data by the minutest scrupulousness of observation; in which, further, its claims are so fully acknowledged, that the public wealth of every nation pretending to civilization, the most consummate productions of labour and skill, and the loftiest and most powerful intellects which appear among men, are gladly and emulously assigned to the task of adding to its completeness. In this condition of the science, it will readily be understood that Professor Airy, your Reporter upon it, has had to mark his desiderata, in no cases but those where some further developement of calculation, some further delicacy of observation, some further accumulation of exact facts, are requisite; though in every branch of the subject the labour of calculation, the delicacy of observation, and the accumulation of exact facts, have already gone so far that the mere statement of what has been done can hardly be made credible or conceivable to a person unfamiliar with the study.

"One article, indeed, in his list of recommendations to future labourers, read at the last Meeting of the Association, may appear capable of being accomplished by more limited labour than the rest,—the determination of the mass of Jupiter by observations of the elongations of his satellites. And undoubtedly, many persons were surprised when they found that on this, so obvious a subject of interest, no measures had been obtained since those which Pound took at the request of Newton. Yet in this case, if an accuracy and certainty worthy of the present condition of Astronomy were to be aimed at, the requisite observations could not be few nor the calculation easy, when it is considered in how complex a manner the satellites disturb each other's motions. But the Meeting will learn with pleasure that the task which he thus pointed out to others, he has himself in the intervening time executed in the most complete manner. He has weighed the mass of Jupiter in the way he thus recommended; and it may show the wonderful perfection of such astronomical measures to state, that he has proved with certainty, that this mass is more than 322 and less than 323 times the mass of the terrestrial globe on which we stand.

"Such is Astronomy: but in proceeding to other sciences, our condition and our task are of a far different kind. Instead of developing our theories, we have to establish them; instead of determining our data and rules with the last accuracy, we have to obtain first approximations to them. This, indeed, may be asserted of the next subject on the list, though that is, in its principles, a branch of Physical Astronomy; for that alone of all the branches of Physical Astronomy had been almost or altogether neglected by men of science. I speak of the science of the Tides. Mr. Lubbock terminated his Report on this subject, by lamenting in Laplace's words this unmerited neglect. He himself in England, and Laplace in France, were indeed the only mathematicians who had applied themselves to do some portion of what was to be done with respect to this subject. Since our Meeting last year, Mr. Dessiou has, under Mr. Lubbock's direction, compared the tides of London, Sheerness, Portsmouth, Plymouth, Brest, and St. Helena; and the comparison has brought to light very remarkable agreements.

in the law which regulates the time of high water, agreements both with each other and with theory; and has at the same time brought into view some anomalies which will give a strong impulse to the curiosity with which we shall examine the records of future observations at some of these places and at many others. I may perhaps here take the liberty of mentioning my own attempts since our last Meeting, to contribute something bearing on this department. It appeared to me that our knowledge of one particular branch of this subject, the motion of the tide-wave in all parts of the ocean, was in such a condition, that by collecting and arranging our existing materials, we should probably be enabled to procure abundant and valuable additions to them. This, therefore, I attempted to do; and I have embodied the result of this attempt in an 'Essay towards a First Approximation to a Map of Cotidal Lines,' which is now just printed in the *Philosophical Transactions of the Royal Society*. If the time of the Meeting allows, I would willingly place before you the views at which we have now arrived, and the direction of our labours which these suggest.

"In the case of the science of Tides, we have no doubt about the general theory to which the phænomena are to be referred, the law of universal gravitation; though we still desiderate a clear application of the theory to the details. In another subject which comes under our review, the science of Light, the prominent point of interest is the selection of the general theory. Sir David Brewster, the author of our Report on this subject, has spoken of 'the two rival theories of light,' which are, as you are aware, that which makes light to consist in material particles emitted by a luminous body, and that which makes it to consist in undulations propagated through a stationary ether. The rivalry of these theories, so far as they can now be said to be rivals, has been by no means barren of interest and instruction during the year which is just elapsed. The discussions on the undulatory theory in our scientific journals have been animated, and cannot, I think, be considered as having left the subject where they found it. The claims of the undulatory theory, it will be recollected, do not depend only on its explaining the facts which it was originally intended to explain; but on this;—that the suppositions adopted in order to account for one set of facts, fall in most wonderfully with the suppositions requisite to explain a class of facts entirely different; in the same manner as in the doctrine of gravitation, the law of force which is derived from the revolutions of the planets in their orbits, accounts for the apparently remote facts of the precession of the equinoxes and the tides. To all this there is nothing corresponding in the history of the theory of emission; and no one, I think, well acquainted with the subject, would now assert, that if this latter theory had been as much cultivated as the other, it might have had a similar brilliant fortune in these respects.

"But if the undulatory theory be true, there must be solutions to all the apparent difficulties and contradictions which may occur in particular cases; and moreover the doctrine will probably gain general acceptance, in proportion as these solutions are propounded and understood, and as prophecies of untried results are delivered and fulfilled. In the way of such prophecies few things have been more remarkable than the prediction, that under particular circumstances a ray of light must be refracted into a conical pencil, deduced from the theory by Professor Hamilton of Dublin, and afterwards verified experimentally by Professor Lloyd. In the way of special difficulties, Mr. Potter proposed an ingenious experiment which appeared to him inconsistent with the theory. Professor Airy, from a mathematical examination of this case, asserted that the facts, which are indeed difficult to observe, must be somewhat different from what they appeared to Mr. Potter; and having myself been present at Professor Airy's experiments, I can venture to say, that the appearances agree exactly with the results which he has deduced from the theory. Another gentleman, Mr. Barton, proposed other difficulties founded upon the calculation of certain experiments of Biot and Newton; and Professor Powell of Oxford has pointed out that the data so referred to cannot safely be made the basis of such calculations, for mathematical reasons. There is indeed here, also, one question of fact concerning an experiment stated in Newton's Optics: In a part of the image of an aperture where Newton's statement places a dark line, in which Mr. Barton has followed him, Professors Airy, Powell, and others, have been able to see only a bright space, as the theory would require. Probably the experiments giving the two different results have not been made under precisely the same circumstances; and the admirers of Newton are the persons who will least of all consider his immoveable fame as exposed to any shock by these discussions.

"Perhaps, while the undulationist will conceive that his opinions have gained no small accession of evidence by this exemplification of what they will account for, those who think the advocates of the theory have advanced its claims too far, will be in some degree conciliated by having a distinct acknowledgement, as during these discussions they have had, of what it does not pretend to explain. The whole doctrine of the absorption of light is at present out of the pale of its calculations; and if the theory is ever extended to these phænomena, it must be by supplementary suppositions concerning the ether and its undulations, of which we have at present not the slightest conception.

"There are various of the Physical subjects to which your Reports refer, which it is less necessary to notice in a general sketch like the present. The recent discoveries in Thermo-electricity, of which Professor Cumming has presented you with a review, and the investigations concerning Radiant Heat which have been arranged and stated by Professor Powell, are subjects of great interest and promise; and they are gradually advancing, by the accumulation of facts bound together by subordinate rules, into that condition in which we may hope to see them subjugated to general and philosophical theories. But with regard to this prospect, the subjects I have mentioned are only the fragments of sciences, on which we cannot hope to theorize successfully except by considering them with reference to their whole;—Thermo-electricity with reference to the whole doctrine of electricity; Radiant Heat with reference to the whole doctrine of heat.

"If the subjects just mentioned be but parts of sciences, there is another on which you have a Report before you, which, though treated as one science, is in reality a collection of several sciences, each of great extent. I speak of Meteorology, which is reported on by Professor Forbes. There is perhaps no portion of human knowledge more capable of being advanced by our conjoined exertions than this: some of the requisite observations demand practice and skill; but others are easily made, when the observer is once imbued with sound elementary notions; and in all departments of the subject little can be done without a great accumulation of facts and a patient inquiry after their rules. Some such contributions we may look for at our present Meeting. Professor Forbes has spoken of the possibility of constructing maps of the sky by which we may trace the daily and hourly condition of the atmosphere over large tracts of the earth. If, indeed, we could make a stratigraphical analysis of the aerial shell of the earth, as the geologist has done of its solid crust, this would be a vast step for Meteorology. This, however, must needs be a difficult task: in addition to the complexity of these superincumbent masses, time enters here as a new element of variety: the strata of the geologist continue fixed and permanent: those of the meteorologist change from one moment to another. Another difficulty is this; that while we want to determine what takes place in the whole depth of the aerial ocean, our observations are necessarily made almost solely at its bottom. Our access to the heights of the atmosphere is more limited, in comparison with what we wish to observe, than our access to the depths of the earth.

"Geology, indeed, is a most signal and animating instance of what may be effected by continued labours governed by common views. Mr. Conybeare's Report upon this science gives you a view of what has been done in it during the last twenty years; and his 'Section of Europe from the North of Scotland to the Adriatic,' which is annexed to the Report, conveys the general views with regard to the structure of Central Europe, at which geologists have now arrived. To point out any more recent additions to its progress or its prospects is an undertaking more suitable to the geologists by profession, than to the present sketch. And all who take an interest in the subject will rejoice that the constitution and practice of the Geological Society very happily provide, by the annual addresses of its Presidents, against any arrear in the incorporation of fresh acquisitions with its accumulated treasures.

"The science of Mineralogy, on which I had the honour of offering a Report to the Association, was formerly looked upon as a subordinate portion of Geology. It may, however, now be most usefully considered as a science co-ordinate and closely allied with Chemistry, and the most important questions for examination in the one science belong almost equally to the other. Mr. Johnston, in his Report on Chemical Science, has, as the subject required, dwelt upon the questions of isomorphism and plesiomorphism, which I had noticed as of great importance to Mineralogy. Dr. Turner and Prof. Miller, who at the last Meeting undertook to inquire into this subject, have examined a number of cases, and obtained some valuable facts; but the progress of our knowledge here necessarily requires time, since the most delicate chemical analysis and the exact measurement of 30 or 40 crystals are wanted for the satisfactory establishment of the properties of each species*. In Che-

* Perhaps I shall not have a more favourable occasion than the present of correcting a statement in my Report, which is not perfectly accurate, on a point which has been a subject of controversy between Sir David Brewster and Mr. Brooke. I have noticed (p. 338.) the sulphato-tricarbonate of lead of Mr. Brooke, as a mineral which at first appeared to contradict Sir David Brewster's general law of the connexion of crystalline form with optical structure, in as much as it appeared to be of the rhombohedral system, and was found to have

mistry, besides the great subject of isomorphism to which I have referred, there are some other yet undecided questions, as for instance those concerning the existence and relations of the sulpho-salts and chloro-salts; and these are not small points, for they affect the whole aspect of chemical theory, and thus show us how erroneously we should judge, if we were to consider this science as otherwise than in its infancy.

"In every science, Notation and Nomenclature are questions subordinate to calculation and theory. The Notation of Crystallography is such as to answer the purposes of calculation, whether we take that of Mohs, Weiss, or Nauman. It appears very desirable that the Notation of *Chemistry* also should be so constructed as to answer the same purpose. Dr. Turner in the last edition of his Chemistry, and Mr. Johnston in his Report, have used a notation which has this advantage, which that commonly employed by the continental Chemists does not possess.

"I have elsewhere stated to the Association how little hope there appears at present to be of purifying and systematizing our mineralogical nomenclature. The changes of theory in Chemistry to which I have already referred, must necessarily superinduce a change of its nomenclature, in the same manner in which the existing nomenclature was introduced by the prevalent theory; and the new views have in fact been connected with such a change by those who have propounded them. It will be for the Chemical Section of the Association to consider how far these questions of Nomenclature and Notation can be discussed with advantage at the present Meeting.

"The Reports presented at the last Meeting had a reference, for the most part, to physical rather than physiological science. The latter department of human knowledge will be more prominently the subject of some of the Reports which are to come before us on the present occasion. There is, however, one of

two axes of double refraction; and which was afterwards found to confirm the law, the apparently rhombohedral forms being found by Mr. Haidinger to be not simple but compound. It seems, however, that the solution of the difficulty (for no one now will doubt that it has a solution,) is somewhat different. There appear to have been included under this name two different kinds of crystals belonging to different systems of crystallization. Some which Mr. Brooke found to be rhombohedral, Sir David Brewster found to have a single optical axis with no trace of composition; others were prismatic with two axes; and thus Mr. Brooke's original determinations were probably correct. The high reputation of the parties in this controversy does not need this explanation; but probably those who look with pleasure at the manner in which the apparent exceptions to laws of nature gradually disappear, may not think a moment or two lost in placing the matter on its proper footing.

last year's Reports which refers to one of the widest questions of Physiology; that of Dr. Prichard on the History of the Human Species, and its subdivision into races. The other lines of research which tend in the same direction will probably be brought before the Association in successive years, and thus give us a view of the extent of knowledge which is accessible to us on this subject.

"In addition to these particular notices of the aspect under which various sciences present themselves to us as resulting from the Reports of last years, there is a reflexion which may I think be collected from the general consideration of these sciences, and which is important to us, since it bears upon the manner in which science is to be promoted by combined labour such as that which it is a main object of this Association to stimulate and organize. The reflexion to which I refer is this;—that a combination of theory with facts, of general views with experimental industry, is requisite, even in subordinate contributors to science. It has of late been common to assert that *facts* alone are valuable in science; that theory, so far as it is valuable, is contained in the facts; and, so far as it is not contained in the facts, can merely mislead and preoccupy men. But this antithesis between theory and facts has probably in its turn contributed to delude and perplex; to make men's observations and speculations useless and fruitless. For it is only through some view or other of the *connexion* and *relation* of facts, that we know what circumstances we ought to notice and record; and every labourer in the field of science, however humble, must direct his labours by some theoretical views, original or adopted. Or if the word *theory* be unconquerably obnoxious, as to some it appears to be, it will probably still be conceded, that it is the rules of facts, as well as facts themselves, with which it is our business to acquaint ourselves. That the recollection of this may not be useless, we may collect from the contrast which Professor Airy in his Report has drawn between the astronomers of our own and of other countries. "In England," he says, (p. 184,) "an observer conceives that he has done everything when he has made an observation." "In foreign observatories," he adds, "the exhibition of results and the comparison of results with theory, are considered as deserving more of an astronomer's attention, and demanding greater exercise of his intellect, than the mere observation of a body on the wire of a telescope." We may, indeed, perceive in some measure the reason which has led to the neglect of theory with us. For a long period astronomical theory was greatly a-head of observation, and this deficiency was mainly supplied by the perseverance and accuracy of English observers. It was natural that the value and reputation which our observations thus acquired for the time, should lead us to think too disrespectfully, in comparison, of the other departments of the science. Nor is the lesson thus taught us confined to Astronomy; for, though we may not be able in other respects to compare our facts with the results of a vast and yet certain theory, we ought never to forget that facts can only become portions of knowledge as they become classed and connected; that they can only constitute truth when they are included in general propositions. Without some attention to this consideration, we may notice daily the changes of the winds and skies, and make a journal of the weather, which shall have no more value than a journal of our dreams would have; but if we can once obtain fixed measures of what we notice, and connect our measures by probable or certain rules, it is no longer a vacant employment to gaze at the clouds, or an unprofitable stringing together of expletives to remark on the weather; the caprices of the atmosphere become steady dispositions, and we are on the road to meteorological science.

"It may be added—as a further reason why no observer should be content without arranging his observations, in whatever part of Physics, and without *endeavouring* at least to classify and connect them—that when this is not done at first, it will most likely never be done. The circumstances of the observation can hardly ever be properly understood or interpreted by others; the suggestions which the observations themselves supply, for change of plan or details, cannot in any other way be properly appreciated and acted on. And even the mere multitude of unanalysed observations may drive future students of the subject into a despair of rendering them useful. Among the other desiderata in Astronomy which Professor Airy mentions, he observes, "Bradley's observations of stars," made in 1750, "were nearly useless till Bessel undertook to reduce them" in 1818. "In like manner Bradley's and Maskelyne's observations of the sun are still nearly useless," and they and many more must continue so till they are reduced. This could not have happened if they had been reduced and compared with theory at the time; and it cannot but grieve us to see so much skill, labour and zeal thus wasted. The perpetual reference or attempt to refer observations, however numerous, to the most probable known rules, can alone obviate similar evils.

"It may appear to many, that by thus recommending theory we incur the danger of encouraging theoretical speculations to the detriment of observation. To do this would be indeed to render an ill service to science: but we conceive that our purpose cannot so far be misunderstood. Without here attempting any nice or technical distinctions between theory and hypothesis, it may be sufficient to observe that *all deductions from theory for any other pupose than that of comparison with observation* are frivolous and useless exercises of ingenuity, so far as the interests of physical science are concerned. Speculators, if of active and inventive minds, *will* form theories whether we wish it or no. These theories may be useful or may be otherwise—we have examples of both results. If the theories merely stimulate the examination of facts, and are modified as and when the facts suggest modification, they may be erroneous, but they will still be beneficial;—they may die, but they will not have lived in vain. If, on the other hand, our theory be supposed to have a truth of a superior kind to the facts; to be certain independently of its exemplification in particular cases;—if, when exceptions to our propositions occur, instead of modifying the theory, we explain away the facts,—our theory then becomes our tyrant, and all who work under its bidding do the work of slaves, they themselves deriving no benefit from the result of their labours. For the sake of example we may point out the Geological Society as a body which, labouring in the former spirit, has ennobled and enriched itself by its exertions: if any body of men should employ themselves in the way last described, they must soon expend the small stock of *à priori* plausibility with which they must of course begin the world.

"To exemplify the distinction for a moment longer, let it be recollected that we have at the present time two rival theories of the history of the earth which prevail in the minds of geologists;—one, which asserts that the changes of which we trace the evidence in the earth's materials have been produced by causes such as are still acting at the surface; another, which considers that the elevation of mountain chains and the transition from the organized world of one formation to that of the next, have been produced by events which, compared with the present course of things, may be called catastrophes and convulsions. Who does not see that all that those theories have hitherto done, has been, to lead geologists to study more exactly the laws of permanence and of change in the existing organic and inorganic world, on the one hand; and on the other, the relations of mountain chains to each other, and to the phænomena which their strata present? And who doubts, that, as the amount of the full evidence may finally be, (which may, indeed, perhaps require many generations to accumulate,) geologists will give their assent to the one or the other of these views, or to some intermediate opinion to which both may gradually converge?

"On the other hand—to take an example from a science with which I have had a professional concern—the theory that crystalline bodies are composed of ultimate molecules which have a definite and constant geometrical form, may properly and philosophically be adopted, so far as we can, by means of it, reduce to rules the actually occurring secondary faces of such substances. But if we assume the doctrine of such an atomic composition, and then form imaginary arrangements of these atoms, and enunciate these as explanations of dimorphism, or plesiomorphism, or any other apparent exception to the general principle, we proceed, as appears to me, unphilosophically. Let us collect and classify the facts of dimorphism and plesiomorphism, and see what rules they follow, and we may then hope to discern whether our atomic theory of crystalline molecules is tenable, and what modifications of it these cases, uncontemplated in its original formation, now demand.

"I will not now attempt to draw forth other lessons which the Report of last year may supply for our future guidance; although such offer themselves, and will undoubtedly affect the spirit of our proceedings during this Meeting. But there is a reflexion belonging to what I may call the morals of science, which seems to me to lie on the face of this Report, and which I cannot prevail upon myself to pass over. In looking steadily at the past history and present state of physical knowledge, we cannot, I think, avoid being struck with this thought,—How little is done and how much remains to do;—and again, notwithstanding this, how much we owe to the great philosophers who have preceded us. It is sometimes advanced as a charge against the studies of modern science, that they give men an overweening opinion of their own acquirements, of the superiority of the present generation, and of the intellectual power and progress of man;—that they make men confident and contemptuous, vain and proud. That they *never* do this, would be much to say of these or of any other studies; but, assuredly, *those* must read the history of science with strange prepossessions who find in it an aliment for such feelings. What is the picture which we have had presented to us? Among all the attempts of man to systematize and complete his knowledge, there is one science, Astronomy, in which he may be considered to have been successful; he has there attained a general and certain theory: for this success, the labour of the most highly gifted portion of the species for 5000 years has been requisite. There is another science, Optics, in which we are, perhaps, in the act of obtaining the same success, with regard to a part of the phænomena. But all the rest of the prospect is comparatively darkness and chaos; limited rules, imperfectly known, imperfectly verified, connected by no known cause, are all that we can discern. Even in those sciences which are considered as having been most successful, as Chemistry, every few years changes the aspect under which the theory presents the facts to our minds, while no theory, as yet, has advanced beyond the mere horn-book of calculation. What is there here of which man can be proud, or from which he can find reason to be presumptuous? And even if the Discoverers to whom these sciences owe such progress as they have made—the great men of the present and the past—if THEY might be elate and confident in the exercises of their intellectual powers, who are *we*, that we should ape their mental attitudes?—we, who can but with pain and effort keep a firm hold of the views which they have disclosed? But it has not been so; they, the really great in the world of intellect, have never had their characters marked with admiration of themselves and contempt of others. Their genuine nobility has ever been superior to those ignoble and low-born tempers. Their views of their own powers and achievements have been sober and modest, because they have ever felt how near their predecessors had advanced to what they had done, and what patience and labour their own small progress had cost. Knowledge, like wealth, is not likely to make us proud or vain, except when it comes suddenly and unlearned; and in such a case, it is little to be hoped that we shall use well, or increase, our ill-understood possession.

"Perhaps some of the appearance of overweening estimation of ourselves and our generation which has been charged against science, has arisen from the natural exultation which men feel at witnessing the successes of art. I need not here dwell upon the distinction of science and art; of knowledge, and the application of knowledge to the uses of life; of theory and practice. In the success of the mechanical arts there is much that we look at with an admiration mingled with some feeling of triumph; and this feeling is here natural and blameless. For what is all such art but a struggle,—a perpetual conflict with the inertness of matter and its unfitness for our purposes? And when, in this conflict, we gain some point, it is impossible we should not feel some of the exultation of victory. In all stages of civilization this temper prevails: from the naked inhabitant of the islands of the ocean, who by means of a piece of board glides through the furious and apparently deadly line of breakers, to the traveller who starts along a rail-road with a rapidity that dazzles the eye, this triumphant joy in successful art is universally felt. But we shall have no difficulty in distinguishing this feeling from the calm pleasure which we receive from the contemplation of truth. And when we consider how small an advance of speculative science is implied in each successful step of art, we shall be in no danger of imbibing, from the mere high spirits produced by difficulty overcome, any extravagant estimate of what man has done or can do, any perverse conception of the true scale of his aims and hopes.

"Still, it would little become us here to be unjust to practical science. Practice has always been the origin and stimulus of theory: Art has ever been the mother of Science; the comely and busy mother of a daughter of a far loftier and serener beauty. And so it is likely still to be: there are no subjects in which we may look more hopefully to an advance in sound theoretical views, than those in which the demands of practice make men willing to experiment on an expensive scale, with keenness and perseverance; and reward every addition of our knowledge with an addition to our power. And even they—for undoubtedly there are many such—who require no such bribe as an inducement to their own exertions, may still be glad that such a fund should exist, as a means of engaging and recompensing subordinate labourers.

"I will not detain you longer by endeavouring to follow more into detail the application of these observations to the proceedings of the General and Sectional Meetings during the present week. But I may remark that some subjects, circumstanced exactly as I have described, will be brought under your notice by the Reports which we have reason to hope for on the present occasion. Thus, the state of our knowledge of the laws of the motion of fluids is universally important, since the motion of boats of all kinds, hydraulic machinery, the tides, the flowing of rivers, all depend upon it. Mr. Stevenson and Mr. Rennie have undertaken to give us an account of different branches of this subject as connected with practice; and Mr. Challis will report to us on the present state of the analytical theory. In like manner the subject of the strength of materials, which the multiplied uses of iron, stone and wood, make so interesting, will be brought before you by Mr. Barlow. These were two of the portions of mechanics the earliest speculated upon, and in them the latest speculators have as yet advanced little beyond the views of the earliest.

"I mention these as specimens only of the points to which we may more particularly direct our attention. I will only observe, in addition, that if some studies, as for instance those of Natural History and Physiology, appear hitherto to have occupied less space in our proceedings than their importance and interest might justly demand, this has occurred because the Reports on other subjects appeared more easy to obtain in the first instance; and the balance will I trust be restored at the present Meeting. I need not add anything further on this subject. Among an assembly of persons such as are now met in this place, there can be no doubt that the most important and profound questions of science in its existing state will be those which will most naturally occur in our assemblies and discussions. It merely remains for me to congratulate the Association upon the circumstances under which it is assembled; and to express my persuasion that all of us, acting under the elevating and yet sobering thought of being engaged in the great cause of the advancement of true science, and cherishing the views and feelings which such a situation inspires, shall derive satisfaction and benefit from the occasions of the present week."

Mr. Whewell having concluded his Address, the Meeting adjourned, after electing by a general vote the candidates who had been approved by the Council and by the General Committee.

At eight P.M., the Members having reassembled in the Senate-house, Mr. Taylor read a Report on the state of our knowledge respecting Mineral Veins, which was followed by a general discussion on the nature and origin of veins.

On Wednesday at one P.M., the Chairmen of the Sections having read the minutes of their proceedings to the Meeting, the Rev. G. Peacock delivered a brief abstract of his Report on the state of the Theory of Algebra. Professor Lindley read a Report on the state of Physiological Botany; and Mr. G. Rennie on the state of Practical Hydraulics. Auditors were appointed to examine the accounts.

On Thursday, at one P.M., the auditors reported the state of the accounts. The Chairmen of the Sections read the minutes of their proceedings. Professor Christie read a Report on the present state of our knowledge respecting the Magnetism of the Earth. A summary of the contents of a Report on the state of knowledge as to the Strength of Materials, by Professor Barlow, was given, in the absence of the Author, by the Rev. W. Whewell.

In the evening, Mr. Whewell delivered a Lecture in the Senate-house, on the manner in which observations of the Tide may be usefully made to serve as a groundwork for general views; either by observing the time of high water at different places on the same day, in order to determine the motion of the summit of the tide-wave; or by continuing the observations for a considerable time, and comparing them with the moon's transit to obtain the semi-menstrual inequality. He observed, that it appears from Mr. Lubbock's recent researches on the subject, that the tides of Portsmouth and Brest agree very closely in the law of this inequality, and that the tides of Plymouth and London also agree; but that there is an anomaly which cannot at present be explained in the comparison of Brest with Plymouth. Professor Farish explained to the Meeting the advantages which he conceived would be derived from applying the power of steam to carriages on undulating roads in preference to level rail-ways.

On Friday, at one P.M., the Chairmen of the Sections having read the minutes of their proceedings, the Rev. J. Challis made a Report on the progress of the Theory of Fluids. The President stated the appropriation * to certain scientific objects of a portion of the funds of the Association to the amount of 600*l.* Mr. Babbage, at the President's request, explained his views respecting the advantages which would accrue to science from such a collection of numerical facts as he had formerly recommended under the title of "Constants of Nature and Art." The President announced, that it had been resolved by the General Committee, that the Meeting of 1834 should take place at Edinburgh in the early part of the month of September; he read the names of the Officers and Members of the Council appointed for the ensuing year.

The thanks of the Meeting were then voted to the Vice-Chancellor and the other authorities of the University, to the retiring Officers and Members of the Council, to the President, the Secretaries for Cambridge, the Local Committee of Management, and the General Secretary.

The President, in his concluding Address to the Meeting, explained an irregularity which had occurred in the formation of a new Section. In addition to the five Sections into which the Meeting had been divided by the authority of the General

* For a particular account of these appropriations, see p. xxxvi.

Committee, he stated that another had come into operation, the object of which was to promote *statistical* inquiries. It had originated with some distinguished philosophers, but could not be regarded as a legitimate branch of the Association till it had received the recognition of the governing body ; there could be little doubt, however, that the new Section would obtain the sanction of the General Committee, with some limitation perhaps of the specific objects of inquiry. On this subject he made the following observations :—

" Some remarks may be expected from me in reference to the objects of this Section, as several Members may perhaps think them ill fitted to a Society formed only for the promotion of natural science. To set, as far as I am able, these doubts at rest, I will explain what I understand by science, and what I think the proper objects of the Association. By science, then, I understand the consideration of all subjects, whether of a pure or mixed nature, capable of being reduced to measurement and calculation. All things comprehended under the categories of space, time and number properly belong to our investigations ; and all phænomena capable of being brought under the semblance of a law are legitimate objects of our inquiries. But there are many important subjects of human contemplation which come under none of these heads, being separated from them by new elements ; for they bear upon the passions, affections and feelings of our moral nature. Most important parts of our nature such elements indeed are ; and God forbid that I should call upon any man to extinguish them ; but they enter not among the objects of the Association. The sciences of morals and politics are elevated far above the speculations of our philosophy. Can, then, statistical inquiries be made compatible with our objects, and taken into the bosom of our Society? I think they unquestionably may, so far as they have to do with matters of fact, with mere abstractions, and with numerical results. Considered in that light they give what may be called the raw material to political economy and political philosophy ; and by their help the lasting foundations of those sciences may be perhaps ultimately laid. These inquiries are, however, it is important to observe, most intimately connected with moral phænomena and economical speculations,—they touch the mainsprings of passion and feeling,—they blend themselves with the generalizations of political science; but when we enter on these higher generalizations, that moment they are dissevered from the objects of the Association, and must be abandoned by it, if it means not to desert the secure ground which it has now taken.

" Should any one affirm (what, indeed, no one is prepared to deny,) that all truth has one common essence, and should he then go on to ask why truths of different degrees should be thus dissevered from each other, the reply would not be difficult. In physical truth, whatever may be our difference of opinion, there is an ultimate appeal to experiment and observation, against which passion and prejudice have not a single plea to urge. But in moral and political reasoning, we have ever to do with questions, in which the waywardness of man's will and the turbulence of man's passions are among the strongest elements. The consequence it is not for me to tell. Look around you, and you will then see the whole framework of society put in movement by the worst passions of our nature; you will see love turned into hate, deliberation into discord, and men, instead of mitigating the evils which are about them, tearing and mangling each other, and deforming the moral aspect of the world. And let not the Members of the Association indulge a fancy, that they are themselves exempt from the common evils of humanity. There is that within us, which, if put into a flame, may consume our whole fabric,—may produce an explosion, capable at once of destroying all the principles by which we are held together, and of dissipating our body in the air. Our Meetings have been essentially harmonious, only because we have kept within our proper boundaries, confined ourselves to the laws of nature, and steered clear of all questions in the decision of which bad passions could have any play. But if we transgress our proper boundaries, go into provinces not belonging to us, and open a door of communication with the dreary wild of politics, that instant will the foul Dæmon of discord find his way into our Eden of Philosophy.

"In every condition of society there is some bright spot on which the eye loves to rest. In the turbulent republics of ancient Greece, where men seemed in an almost ceaseless warfare of mind and body, they had their seasons of solemnity, when hostile nations made a truce with their bitter feelings, assembled together, for a time, in harmony, and joined in a great festival; which, however differing from what we now see in its magnitude and forms of celebration, was consecrated, like our present Meeting, to the honour of national genius. Whatever have been the bitter feelings which have so often disgraced the civil history of mankind, I dare to hope that they will never find their resting-place within the threshold where this Association meets; that peace and good will, though banished from every other corner of the land, will ever find an honoured seat amongst us; and that the congregated philosophers of the empire, throwing aside bad passion and party animosity, will, year by year, come to their philosophical Olympia, to witness a noble ceremonial, to meet in a pacific combat, and share in the glorious privilege of pushing on the triumphal car of Truth.

"The last duty I have to perform this morning would be a painful one indeed, were our Assembly to be broken up into elements which were not again to be reunited. The Association is not, however, dissolved; its meeting is only adjourned to another year; and it has been a matter of great joy to me to announce to you, that the Committee has elected for your next President a distinguished soldier and philosopher; and that it will be your privilege to reassemble in one of the fairest capitals of the world,—in a city which has nursed a race of literary and philosophic giants,—in a land filled with natural beauties, and wedded to the imagination and the memory by a thousand endearing associations.

"There is a solemnity in parting words, which may, I think, justify me (especially after what has been so well said this morning by the Marquis of Northampton,) in passing the limits I have so far carefully prescribed to myself, and in treading for a moment on more hallowed ground. In the first place, I would entreat you to remember that you ought above all things to rejoice in the moral influence of an Association like the present. Facts, which are the first objects of our pursuit, are of comparatively small value till they are combined together so as to lead to some philosophic inference. Physical experiments, considered merely by themselves, and apart from the rest of nature, are no better than stones lying scattered on the ground, which require to be chiselled and cemented before they can be made into a building fit for the habitation of man. The true value of an experiment is, that it is subordinate to some law,—that it is a step toward the knowledge of some general truth. Without, at least, a glimmering of such truth, physical knowledge has no true nobility. But there is in the intellect of man an appetency for the discovery of general truth, and by this appetency, in subordination to the capacities of his mind, has he been led on to the discovery of general laws; and thus has his soul been fitted to reflect back upon the world a portion of the counsels of his Creator. If I have said that physical phænomena, unless connected with the ideas of order and of law, are of little worth, I may further say, that an intellectual grasp of material laws of the highest order has no moral worth, except it be combined with another movement of the mind, raising it to the perception of an intelligent First Cause. It is by help of this last movement that nature's language is comprehended; that her laws become pregnant with meaning; that material phænomena are instinct with life; that all moral and material changes become linked together; and that Truth, under whatever forms she may present herself, seems to have but one essential substance.

"I have before spoken of the distinctions between moral and physical science; and I need not repeat what I have said, unless it be once more solemnly to adjure you not to leave the straight path by which you are advancing,—not to desert the cause for which you have so well combined together. But let no one misunderstand my meaning. If I have said that bad passions mingle themselves with moral and political sciences, and that the conclusions of these sciences are made obscure from the want of our comprehending all the elements with which we have to deal, I have only spoken the truth; but still I hold that moral and political science is of a higher order than the physical. The latter has sometimes, in the estimation of man, been placed on a higher level than it deserves, only from the circumstance of its being so well defined, and grounded in the evidence of experiments appealing to the senses. Its progress is marked by indices the eye can follow; and the boundaries of its conquests are traced by landmarks which stand high in the horizon of man's history. But with all these accompaniments, the moral and political sciences entirely swallow up the physical in importance. For what are they but an interpretation of the governing laws of intellectual nature, having a relation in time present to the social happiness of millions, and bearing in their end on the destinies of immortal beings?

"Gentlemen, if I look forward with delight to our meeting again at Edinburgh, it is a delight chastised by a far different feeling, to which, had not these been parting words, I should not have ventured to give an utterance. It is not possible we should all again meet together. Some of those whose voices have been lifted up during this great Meeting, whose eyes have brightened at the presence of their friends, and whose hearts have beat high during the intellectual communion of the week, before another year may not be numbered with the living. Nay, by that law of nature to which every living man must in his turn yield obedience, it is certain that before another festival, the cold hand of death will rest on the head of some who are present in this assembly. If a thought like this gives a tone of grave solemnity to words of parting, it surely ought to teach us, during our common rejoicings at the triumphal progress of science, a personal lesson of deep humility. By the laws of nature, before we can meet again, many of those bright faces which during the past week I have seen around me may be laid low, for the hand of death may have been upon them; but wherever we reassemble, God grant that all our attainments in science may tend to our moral improvement; and may we all meet at last in the presence of that Almighty Being, whose will is the rule of all law, and whose bosom is the centre of all power!"

SECTIONAL MEETINGS.

The Sections assembled daily at eleven A.M., and occasionally also at half-past eight P.M., at their respective places of meeting, in the Schools, the Astronomical Lecture-room, and the Hall of Caius College. On Saturday, the Section of Natural History made an excursion to the Fens.

Abstracts of most of the Communications which were made to the Sections will be found in a subsequent part of the volume.

In addition to the communications of which abstracts are there given, notices of the following transactions appear on the minutes:—

M. Quetelet described the observations which he had made on Falling Stars. It was suggested that such observations might be available in certain cases for determining differences of longitude.

Mr. Potter communicated some calculations of the height of the Aurora Borealis, seen on the 21st of March 1833.

Mr. Hopkins gave an abstract of a paper on the Vibration of Air in Cylindrical Tubes of definite length.

Dr. Ritchie made some remarks on the Sensibility of the Eye, and the errors to which it is subject.

Mr. Barton gave a view of his opinions on the Propagation of Heat in solid bodies.

A letter was received from Mr. Frend regarding certain points in the Theory of the Tides.

The Rev. W. Scoresby described a Celestial Compass invented by Col. Graydon.

Mr. R. Murphy read some remarks on the utility of observing the Magnetic Dip in Mines.

M. Quetelet gave an account of some observations made by himself and M. Necker de Saussure, which corroborate the statements of M. Kuppfer, respecting the inequality of magnetic intensity at the top and the base of mountains.

Professor Christie stated his views relative to the cause of the Magnetism of the Earth.

Mr. A. Trevelyan read a paper on certain Vibrations of Heated Metals.

Mr. Brunel exhibited and explained a Model in illustration of his method of constructing Bridges without *centering*.

A notice of some experiments relative to Isomorphism, by Dr. Turner and Professor Miller, was read.

Dr. Daubeny made a communication on the Gases given off from the surfaces of the water in certain thermal springs.

The Rev. W. V. Harcourt exhibited specimens of Metal taken out of the crevices at the bottom of a mould in which a large bronze figure had been cast by Mr. Chantrey; together with fragments of the Bronze employed in the casting, from which the former specimens differed considerably in colour, frangibility, &c.

Mr. Lowe gave an account of various chemical products found in the retorts and flues of Gas Works.

Mr. Pearsall made a communication on the bleaching powers of Oxygen.

Mr. J. Taylor described the character of the Ecton Mine, and the occurrence of the copper ore in connected cavities which had been explored to a depth of 225 fathoms without reaching the termination of them.

Dr. Buckland described the manner in which fibrous Limestone occurs in the Isle of Purbeck and other situations.

Mr. Murchison stated, and illustrated by Maps and Sections, the principal results of his inquiries into the sedimentary deposits which occupy the western parts of Shropshire and Herefordshire, and are prolonged in a S.W. direction through the counties of Radnor, Brecknock, and Caermarthen, and the intrusive igneous rocks which occur in certain parts of the district. He mentioned the occurrence of freshwater Limestone in a detached Coal-field of Shropshire.

Professor Sedgwick described the leading features in the Geology of North Wales, the lines of elevation, the relation of the trap rocks to the slate system, the cleavage of the slate; pointed out the relations of this tract to that examined by Mr. Murchison; and drew a general parallel between the slate formations of Wales and Cumberland.

Mr. J. Taylor having read to the Section the concluding part of his Report on Veins, in the discussion which followed, M. Dufrénoy entered into a consideration of some phænomena of the igneous rocks of Britanny and Central France, viewed with reference to the connexion between them and the metalliferous veins of those districts, and remarked on the occurrence in Central France of mineral veins, only in the narrow zone at the junction of the unstratified and stratified rocks. He also made some remarks on the association of dolomite and gypsum, with the igneous rocks of the Alps and the Pyrenees.

Professor Sedgwick gave a general account of the Red Sandstones connected with the Coal-measures of Scotland, and the

Isle of Arran, with the view of showing that they are perfectly distinct from the similar rocks connected with the Magnesian Limestone.

Mr. Hartop exhibited a Map and Sections to illustrate the series of Coal Strata in South Yorkshire, and their direction and varying dip in the valley of the Dun, and to the north and south of that river; described the characters of the strata, and the influence of certain great dislocations on the quality of the coal.

Mr. Greenough exhibited a Map of Western Europe, on which the relative levels of land and water were represented by means of colours, instead of engraving. Mr. Greenough was requested to permit a map on this plan to be published.

The Rev. J. Hailstone communicated some notices relating to Mineral Veins.

Sections of the Well in the Dock Yard at Portsmouth, and of the Well in the Victualling Yard at Weevil, were communicated by the Rev. Mr. Leggat and Mr. Blackburn, on the part of the Portsmouth Philosophical Society; and a letter from Mr. Goodrich, explanatary of the Sections, was read.

Mr. Mantell exhibited a perfect Femur of the Iguanodon, and explained its distinctive anatomical characters.

Mr. W. C. Trevelyan exhibited specimens of Coprolites, and remains of Fishes, from the Edinburgh Coal-field.

Mr. Fox exhibited specimens of Fishes from the Magnesian Limestone and Marl-slate of Durham.

Mr. Gray made some remarks on the occurrence of Water in the Valves of Bivalve Shells, and exhibited a specimen of *Spondylus varius,* in which water was contained in both the valves.

Mr. Ogilby gave an account of his views respecting the classification of Ruminating Quadrupeds, which he proposed to found upon the presence or absence of horns on the female sex; the peculiar form of the upper lip; and the presence or absence of the subocular and submaxillary glands. He showed the application of these views to the division of *hollow-horned ruminating animals without horns in the female sex,* which he distributed into five new genera.

The Rev. W. Scoresby communicated some observations on the adaptation of the Structure of the Cetacea to their habits of life and residence in the Ocean; and suggested the use which might be made of the peculiar forms of the Whalebone in their classification.

Lieutenant Colonel Sykes exhibited a specimen of the Short-tailed Manis, and communicated some observations on its mode of progression.

Mr. Brayley communicated a memoir on the laws regulating the distribution of the powers of producing Light and Heat among Animals.

Mr. H. Strickland made some remarks on the *Vipera Chersea*, showing its specific difference from the common Viper.

The subject of the use of the Pith in Plants, was discussed by Professor Burnett, Professor Henslow, Mr. Curtis, and Mr. Gray.

Dr. Roupell exhibited some Drawings representing the effects of irritant Poisons upon the living membrane of the intestinal canal of Men and Animals.

Mr. Fisher communicated some observations on the physical condition of the Brain during sleep.

Mr. Brooke made some remarks on the physiology of the Eye and the Ear.

Dr. Marshall Hall gave an abstract of his views respecting the reflex function of the Medulla oblongata and Medulla spinalis.

COMMITTEES.

The General Committee met daily at ten A.M., and at other hours by adjournment, in the Hall of Trinity Hall. The Committees of Sciences met as soon after ten as the business of the General Committee permitted, in the rooms of their respective Sections. The General Committee made the necessary arrangements for the conduct of the Meeting; formed the Sectional Committees of Sciences; determined the place and time of the next Meeting; appointed the new Officers and Council; and passed the following Resolutions:—

1. That the thanks of the Association be given to the Societies and Institutions from which it has received invitations,—in Bristol, Birmingham, Liverpool, Newcastle and Edinburgh.

2. That Members of the Association whose subscription shall have been due for two years, and who shall not pay it on proper notice, shall cease to be Members, power being left to the Committee or Council to reinstate them on reasonable grounds within one year, on payment of their arrears.

3. That the number of Deputies which provincial Institutions shall be entitled to send to the Meetings as Members of the General Committee, shall be two from each Institution.

4. That the following instructions be given to each of the Committees of Sciences:—

To select those points of science, which, on a review of the former Recommendations of the Committees, or those contained in the Reports published by the Association, or from suggestions made at the present Meeting, they may think most fit to be advanced by an application of the funds of the Society, either in compensation for labour, or in defraying the expense of apparatus, or otherwise. The Committee are requested to confine their selections to definite as well as important objects; to state their reasons for the selection, and where they may think proper, to designate individuals to undertake the desired investigations; they are to transmit their Recommendations through their Secretaries to the General Committee.

The Committees of Sciences having complied with these instructions, the following Resolutions were passed by the General Committee:

1. That a sum not exceeding 200*l.* be devoted to the *discussion* of observations of the Tides, and the formation of Tide Tables, under the superintendence of Mr. Baily, Mr. Lubbock, Rev. G. Peacock, and Rev. W. Whewell.

2. That a sum not exceeding 50*l.* be appropriated to the construction of a Telescopic Lens, or Lenses, out of Rock Salt, under the direction of Sir David Brewster.

3. That Dr. Dalton and Dr. Prout be requested to institute experiments on the specific gravities of Oxygen, Hydrogen, and Carbonic Acid; and that a sum not exceeding 50*l.* be appropriated to defray the expense of any apparatus which may be required.

4. That a series of experiments on the effects of long continued Heat be instituted at some iron furnace, or in any other suitable situation; and that a sum not exceeding 50*l.* be placed at the disposal of a Sub-Committee, consisting of Professor Daubeny, Rev. W. V. Harcourt, Professor Sedgwick, and Professor Turner, to meet any expense which may be incurred *.

5. That measurements should be made, and the necessary data procured, to determine the question of the permanence or change of the relative Level of Sea and Land on the coasts of Great Britain and Ireland; and that for this purpose a sum not exceeding 100*l.* be placed at the disposal of a Sub-Committee, consisting of Mr. Greenough, Mr. Lubbock, Mr. G. Rennie, Professor Sedgwick, Mr. Stevenson, and Rev. W. Whewell;—the measurements to be so executed, as to furnish the means of reference in future times, not only as to the relative levels of the land and sea, but also as to waste or extension of the land.

* These experiments have been instituted by Mr. Harcourt, in Yorkshire, at the Low Moor Iron Works, the property of Messrs. Hird and Co., and at the Elsecar Furnace, belonging to Earl Fitzwilliam.

6. That the effects of Poisons on the Animal Economy should be investigated and illustrated by graphic representations; and that a sum not exceeding 25*l.* be appropriated for this object. Dr. Roupell, and Dr. Hodgkin were requested to undertake this investigation.

7. That the sensibilities of the Nerves of the Brain should be investigated; and that a sum not exceeding 25*l.* should be appropriated to this object. Dr. Marshall Hall and Mr. S. D. Broughton were requested to undertake these experiments.

8. That a sum not exceeding 100*l.* be appropriated towards the execution of the plan proposed by Professor Babbage, for collecting and arranging the *Constants* of Nature and Art*.

9. That a representation be submitted to Government on the part of the British Association, stating that it would tend greatly to the advancement of astronomy, and the art of navigation, if the observations of the sun, moon and planets, made by Bradley, Maskelyne and Pond, were reduced; and that a deputation† be appointed to wait upon the Lords of the Treasury with a request, that public provision may be made for the accomplishment of this great national object.

Proposals for the formation of a Statistical Section were approved. It was resolved, that the inquiries of this Section should be restricted to those classes of facts relating to communities of men *which are capable of being expressed by numbers, and which promise, when sufficiently multiplied, to indicate general laws.*

A Committee of Statistical Science was formed ‡. The Recommendations § of the several Committees of Science were revised and approved.

TRUSTEES OF THE ASSOCIATION.

* For an abstract of Mr. Babbage's plan, see the Appendix.

† The deputation consisted of Professor Airy, Mr. Baily, Mr. D. Gilbert and Sir John Herschel. The application was immediately complied with by the Government.

‡ For an account of the proceedings of this Committee, see the Appendix.

§ These Recommendations will be found marked with an asterisk in the collection of Recommendations and Suggestions printed in the latter part of the volume.

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TRANSACTIONS.

Report on the State of Knowledge respecting Mineral Veins. By John Taylor, F.R.S., *Treasurer of the Geological Society and of the British Association for the Advancement of Science, &c. &c.*

I HAVE found it very difficult to execute the task proposed to me in a manner satisfactory to myself, as we have at this time no digested account of the views entertained by geologists of the present day upon this interesting subject. The most perfect treatise is that of Werner, which deserves much attention for the observation of facts which it displays; but as it was written to propound a theory, and as that theory depended upon views of the structure of the crust of the earth which modern geology has at least thrown much doubt upon, so his work cannot be taken as an outline of our present state of knowledge.

Since his time but little has been attempted respecting vein formations; and the subject has been, I think, rather neglected by geologists, who have advanced other branches of the science with extraordinary skill, industry and success. Detached papers have, indeed, appeared by English authors, among which that on the veins of Cornwall, by Mr. Joseph Carne, holds a distinguished place.

As some proof that the subject of veins has not been much attended to, I would remark, that in the Second Series of the *Transactions of the Geological Society of London*, consisting now of the first and second volumes complete, and two Parts of the third volume, no paper expressly on veins is to be found. In the First Series there are two papers, one by the late Mr. W. Phillips, giving an outline of facts more generally observed with respect to veins in Cornwall, from observations made principally in the year 1800. Another is by Dr. Berger, on the physical structure of Devon and Cornwall, from observations made in 1809. The writer adopts the Wernerian theory, and mentions cases which he thinks confirmatory of its truth.

In the four volumes of the *Transactions of the Royal Geological Society of Cornwall*, we shall find this subject more attended to, and there are several communications relating to it: among the authors are Dr. Boase, Mr. Carne, Dr. Davey, Mr. R. W. Fox, and Mr. John Hawkins. One of the papers by Mr. Carne is that to which I have before alluded.

One of the most recent works by foreign writers is that of the late M. Schmidt of Siegen. He was an experienced practical miner, and wrote chiefly with a view to his art, describing the various derangements in mineral veins, and tracing the best rules to be observed in pursuing researches in difficult circumstances. He adopts the Wernerian theory of formations, and refers to the author of it as the great master of the subject.

Though no general theory has of late been produced in regular form, yet with the great attention that has been given to geology by so many eminent men, an extended field of observation has taken place, leading to a very general change of opinion on most important points; many conjectures respecting the formation of veins have sprung up, and which, when the facts are more investigated, and they shall have been recorded and classified, may form the groundwork for a more enlarged and rational theory, by which their phænomena and structure may be explained, and the causes of their formation, the manner of filling up, and the circumstances of the varied derangements and dislocations, may be traced and be better understood.

The subject is of threefold importance: first, as it relates to science, wherein a better knowledge of veins generally must very materially contribute to sound investigations as to the structure of the rocks that inclose them: secondly, as it is much owing to the pursuit of the minerals which are deposited in veins that we have acquired and may yet extend our knowledge of geology in general: thirdly, in relation to the question sometimes proposed as to the usefulness of geological science, the most ready answer may be given, if it be considered that this inquiry will relate to subjects of practical utility, in which mankind are universally and largely interested.

Before I proceed to any account of the opinions as to the formation of veins, I would offer some definition descriptive of their character and structure, that in proceeding with our subject we may clearly understand what is meant to be treated on.

Werner lays it down, "That veins are particular mineral repositories, of a flat or tabular shape, which in general traverse the strata of mountains, and are filled with mineral matter differing more or less from the nature of the rocks in which they occur.

"Veins cross the strata, and have a direction different from theirs. Other mineral repositories, such as particular strata or beds, of whatever thickness they occur, have, on the contrary, a similar direction with the strata of the rock, and instead of crossing, run parallel with them: this forms the characteristic difference."

Playfair says: "Veins are of various kinds, and may in general be defined, separations in the continuity of a rock, of a determinate width, but extending indefinitely in length and depth, and filled with mineral substances different from the rock itself. The mineral veins, strictly so called, are those filled with sparry or crystallized substances, and containing the metallic ores."

Mr. Carne says: "*By a true vein I understand the mineral contents of a vertical or inclined fissure, nearly straight, and of indefinite length and depth.* These contents are generally, but not always, different from the strata or the rocks which the vein intersects. True veins have regular walls, and sometimes a thin layer of clay between the wall and the vein; small branches are also frequently found to diverge from them on both sides."

Mr. Carne mentions other veins, which he distinguishes from the true ones as being shorter, crooked, and irregular in size; he considers these to have formed in a different manner: but this will be discussed hereafter.

These definitions seem to me to be sufficient for our purpose; but it may be advantageous here to introduce some further description of circumstances connected with veins, and to explain the terms usually employed to describe them.

Being tabular masses, generally of no great width, any one will, whether vertical or inclined, present at its intersection with the surface a line nearly straight: this may be from north to south, or from east to west, or in any intermediate course. This is usually called *the direction*; by miners frequently *the run of the vein*, or *the course of the vein*, and is denoted by the points of the compass it may cross.

The length, as Werner states, is indefinite, it being doubtful whether any vein has been pursued to a perfect termination.

The tabular mass, again, may be either vertical to the plane of the earth's surface, or may deviate from this position by inclining to one side or the other of the perpendicular. This deviation is called *the inclination* of the vein; by the Cornish miners *the underlie*. It is measured by the angle made with the perpendicular; and as the dip will be to one side of the direction, the latter being known, the other is easily expressed.

The depth to which veins descend into the earth is unknown, as well as the length, and for the same reason.

The only dimension we can ascertain is that across from one side to the other of the tabular mass, and is measured from one wall to the other, which is the term used in England for the cheeks or sides presented by the inclosing rock. This dimension is called the width, or frequently the size of the vein.

The width varies considerably in the same vein. In Europe a vein containing ore is considered to be a wide one if it exceeds five or six feet. In Mexico the width of veins is generally greater.

In metalliferous veins the deposits of ore are extremely irregular, forming masses of very diversified form and extent, and are separated from each other by intervening masses of veinstone or matrix, either entirely devoid of ore, or more or less mixed with it. It is rare to find a vein entirely filled with ore in any part.

In this respect they differ from most beds, where, as in those of coal, the whole is a uniform mass.

The layer of clay, which, as Mr. Carne says, is frequent in such veins, will deserve particular notice when we consider their general structure and the theories of their formation: this is called *Saal-bande* by the Germans, and *flookan* by the Cornish miners.

The clearest idea of a vein will be obtained by imagining a crack or fissure in the rocks, running in nearly a straight line, extending to great and unknown length and depth, and filled with various substances.

I do not intend by this description to convey any theoretic opinion as to the manner in which such fissures may have been formed, or as to the mode of their being furnished with their present contents. These are subjects on which the greatest diversity of opinion has existed in former times, and this diversity is continued to the present period. It is the main business of this Report to state these opinions, and to describe our present state of knowledge of this difficult subject. I feel great distrust of my power to do it justice; but I am encouraged by the idea that a feeble sketch may induce abler hands to pursue the design, and throw more and more light upon this interesting branch of geology.

It would be of little use to go into details of the conjectures of ancient authors, or into the mysteries with which this subject was enveloped in the age of alchemy.

The earliest writer who is worthy to be consulted is Agricola (whose proper name was Bauer): he resided in the Saxon Erzgebirge, and died in the middle of the sixteenth century. He has been called the father of mineralogy, and of the science of mining. He had the rare merit of emerging from the mists and clouds of an absurd school of philosophy, which had till then obscured the objects which it pretended to illustrate; and he first subjected them to inquiries prompted by sound reason and just views of nature.

His writings were numerous, and in such pure Latin that they are said to be entitled to a place among the classics. He treats of veins in a work called *Bermannus*, but more particularly in the third book of his great work *De Re Metallica.*

Agricola being held to be the first who has written anything certain on the formation of veins, and his theory of the manner of their being filled up having, with some modifications, been for a long period generally received, and in part even adopted by Werner, I shall commence from his time the notice of the opinions promulgated by various writers antecedent to Werner and Hutton.

Some have maintained, That veins and their branchings are to be considered as the branches and twigs of an immense trunk which exists in the interior of the globe:

That from the bowels of the earth metallic particles issued forth in the form of vapours and exhalations through the rents, in the same manner as sap rises and circulates in vegetables.

This speculation was proposed by Von Oppel, captain-general of the Saxon mines, who wrote in 1749. He was a skilful miner and an accurate observer; and it is singular that this opinion is not consistent with most that he has elsewhere said on the subject, which generally rather agreed with the views which were adopted by Werner and others.

Henkel, who wrote in the early part of the seventeenth century, and who has been held to be the father of mineralogical chemistry, first attributed the formation of the contents of veins to peculiar exhalations: he supposed the basis of each metal and mineral to have existed in the substance of the rock, and to have been developed by a peculiar process of nature.

Becher about the same time supported very similar views. Stahl, who commented upon the writings of Becher, had advanced a somewhat similar opinion; but he afterwards rejected this theory, and considered veins, as well as the substances of which they are composed, as having been formed at the same time with the earth itself.

Zimmerman, chief commissioner of mines in Saxony, who died in 1747, had an idea that the variety of minerals contained in veins had been produced by a transformation of the substance of the rock.

Charpentier, in 1778, supported nearly similar opinions, and combated strenuously against the theory which considers veins to have been rents that were afterwards filled up by different mineral substances.

This is the theory, however, which, from the time of Agricola to the present day, has been most generally received, namely, that *veins were fissures which have been since filled up by degrees with mineral matters.*

The causes of such fissures, and the mode of their contents being deposited, have been variously stated, and have given rise to much conjecture; and allowing for these differences, the main proposition has been supported by many writers. Among these I would name Agricola; Balthazar Rösler, an eminent miner of Freyberg, who died in 1673; Hoffman, a commissioner of mines at the same place, in 1746; Von Oppel, before mentioned, who, though he had indulged in other speculations, distinctly lays down in his *Introduction to Subterranean Geometry*, (Dresden, 1749,) that veins were formerly fissures, open in their superior part, and that they traverse and intersect the strata.

Bergman entertained opinions very similar, which were also supported by Delius, an author on mining, of considerable celebrity, who wrote about 1770.

Gerhard, in his *Essay on the History of the Mineral Kingdom*, (Berlin, 1781,) gives a collection of interesting facts concerning veins, and considers them to have originally been rents, which were afterwards filled up with mineral substances.

To this list may be added Lasius, in his *Observations on the Mountains of the Hartz*, in 1787; and Linnæus is stated "to have wondered at the nature of that force which split the rocks into those cracks; and adds, that probably the cause is very familiar, —that they were formed moist, and cracked in drying*."

In England we have testimony to the same opinion from Dr. Pryce, who wrote his *Mineralogia Cornubiensis* in 1778. He says, "When solid bodies were separated from fluid, certain cracks, chinks and fissures in various directions were formed, and as the matter of each stratum became more compact and dense by the desertion of moisture, each stratum within itself had its fissures likewise, which, for the most part, being influenced by peculiar distinct laws, were either perpendicular, oblique," &c.

He afterwards adds, that those very fissures are the wombs or receptacles of all metals, and most minerals. He assigns the derangements of veins to the effect of fracture by violence, and quotes subsidence as one of the probable causes of such dislocations. He says there can be no doubt that many alterations

* Hill.

have happened to various parts of the earth before, at, and after the Flood, from *inundations, earthquakes, and the dissolvent powers of subterranean fire and water*, which variety of causes and circumstances must infallibly have produced many irregularities in the disposition and situation of circumjacent strata and lodes*.

He describes twelve kinds of lodes or veins in Cornwall, naming them from their chief contents. But the most remarkable observation of Dr. Pryce is respecting the relative age of veins, of which he seems to have given the first intimation. Werner, long after, states this as a discovery of his own, and as an essential part of his theory. His translator, however, (Dr. Anderson,) does Pryce justice, and remarks that his observations must have been unknown to Werner, who showed much anxiety in all cases to confer on every writer the merit which was due to him.

Dr. Anderson quotes the passage as one of much importance. "Because the cross gossans or cross flookans run through all veins of opposite directions, without the least interruption from them, but, on the contrary, do apparently disjoint and dislocate all of them, it seems reasonable to conclude, *that the east and west veins were antecedent to cross veins*, and that some great event, long after the Creation, occasioned those transverse clefts and openings. But how or when this should come to pass, we cannot presume to form any adequate idea†."

Kirwan supports the doctrine that some veins were originally open, as appears from the rounded stones and petrifactions found in them. Thus, in the granitic mountain of Pangel in Silesia there is a vein filled with globular basalt. So also in veins of wacken, in Joachimstahl in Bohemia, trees and their branches have been found.

But he deems it improbable that all veins were originally open to day, and filled from above. He inclines to the theory of veins being filled by the percolation of solutions of the metals and earths.

Having now taken a cursory view of the opinions held before Werner published his *Theory of Veins*, and seen something of the state of knowledge relating to this subject, we may bear in mind the materials which he had to work with, and take into account his well-known views as to the origin of rocks from aqueous deposition, and we shall comprehend the system which he developed, with respect to veins, in the only work, I believe, which proceeded from his own hand, and which was published

* 'Lode' is the term used in Cornwall for a metalliferous vein.

† *Mineralogia Cornubiensis*, p. 101.

at Freyberg in 1791. Werner adopts, in the first place, the proposition that the spaces now occupied by veins were originally rents formed in the substance of rocks, and states that this is not a new opinion.

He claims the merit of having ascertained in a more positive manner the causes which have produced these rents, and of having brought forward better proofs of it than had formerly been done.

He admits that rents may be produced by many different causes, but he assigns the greater part to subsidence. He lays it down, that when the mass of materials of which the rocks were formed by precipitation in the humid way, and which was at first soft and moveable, began to sink and dry, fissures must of necessity have been formed, chiefly in those places where mountain chains and high land existed. He adds, that rents and fissures are still forming from time to time in mountains which have a close resemblance to those spaces now occupied by veins, and that this happens in rainy seasons and from earthquakes.

He adduces as a proof of his assertions, that veins, in respect of their form, situation and position, bear a strong resemblance to rents and fissures which are formed in rocks and in the earth; that is to say, both have the same tabular figure, and the deviations which they make from their general direction are few in number and very inconsiderable; and he remarks, that all the veins of a mining district, more particularly when they are of the same formation, have a similar direction, which shows them to have been produced by the same general cause.

But what Werner claimed as altogether new, and what he challenges as his own particular discovery is,

1. To have determined and described in a more particular manner the internal structure of veins, as well as the formation of the different substances of which they are composed, and to have settled the relative age of each.

2. To have given the most accurate observations and most perfect knowledge of the meetings and intersections of veins, and to have made these observations subservient to the determining their relative ages.

3. To have determined the different vein formations, particularly metalliferous veins, as well as their age.

4. To have been the first who entertained the idea that the spaces which veins occupy were filled by precipitations from the solutions, which at the same time formed by other precipitations the beds of mountains, and to have furnished proofs of this: and,

5. To have determined the essential differences that are found between the structure of veins and that of beds.

Werner illustrates his propositions by many observations, which his intimate acquaintance with the extensive mining districts in which he was engaged gave him the power of observing and recording; and it must be conceded, at least, that his statement of facts, and his arrangement of them, give him a manifest superiority over most writers upon this subject. Every one who has had opportunity to see much of these storehouses of nature will be struck with the accuracy of most of his descriptions, whether they admit the theory by which they are explained, or not.

He allows that the enrichment of veins, or their being filled with ores or metals, may have taken place by,

1. *a.* A particular filling up from above.
   b. By particular internal canals.
   c. By infiltration across the mass of the vein.

2. A metallic vein may be increased by the junction of a new metalliferous vein.

3. Though rarely, the richness of a vein may be the effect of an elective attraction or affinity of the neighbouring rock.

The mode assigned by Werner for the formation of the spaces now occupied by veins is still further demonstrated, in his opinion, by the relation which veins have to one another; as,

Their intersecting one another.
Their shifting one another.
Their splitting one another into branches.
Their joining and accompanying one another.
Their cutting off one another.

All these peculiarities, he remarks, are produced by the effects of a new fissure upon one that is older.

Subsidence having been the cause of fissures he thinks is proved by the difference in the level in the parts of the same stratum or bed in which a vein is inclosed; and this throwing up or down, as the miners term it, bears a proportion to the size of the vein.

The interior structure of many veins is quoted to show that the fissures had been originally open, and which had been afterwards filled by degrees.

Such veins are composed of beds, arranged in a direction parallel to their sides; their crystallizations are supposed to show these beds to have been deposited successively on each other, and that those next the walls have been first formed. A circumstance much relied on, also, is the existence of rolled masses or water-borne stones, fragments of the adjacent rock, sometimes forming a breccia, remains or impressions of organic bodies, coal and rock salt substances of recent formation, and other matters, which should appear to have come in from above.

This theory obtained considerable attention, and was very generally adopted from the time of its being made known; and it has, I believe, many adherents at this day, particularly among miners or those who have much opportunity of actual observation.

Hutton's *Theory of the Earth* was published afterwards, in 1795; and as his views regarding the operations employed in the formation of the structure of the rocks differed entirely from those who assigned to them an aqueous origin, so it will readily be supposed that he would promulgate a new explanation of the formation of veins.

According to Playfair, this theory embraced the following propositions:—

It allowed that veins are of a formation subsequent to the hardening and consolidation of the strata which they traverse, and that the crystallized and sparry structure of the substances contained in them shows that these substances must have concreted from a fluid state.

It assumes that this fluidity was simple like that of fusion by heat, and not compound like that of solution in a menstruum.

It is inferred that this is so from the acknowledged insolubility of the substances that fill the veins in any one menstruum, and from the total disappearance of the solvent, if there was any, it being argued that nothing but heat could have escaped from the cavities.

It is further maintained, that as the metals generally appear in veins in the form of sulphurets, the combination to which their composition is owing could only have taken place by the action of heat. And, further, that metals being also found native, to suppose that they could have been precipitated pure and uncombined from any menstruum, is to trespass against all analogy, and to maintain a physical impossibility.

It is therefore inferred, that the materials which fill the mineral veins were melted by heat, and forcibly injected in that state into the clefts and fissures of the strata.

The fissures must have arisen, not merely from the shrinking of the strata while they acquired hardness and solidity, but from the violence done to them when they were heaved up and elevated in the manner which the theory has laid down.

Slips or heaves of veins, and of the strata inclosing them, are to be explained from the same violence which has been exerted.

It is admitted as interesting to remark, that in the midst of the signs of disturbance which prevail in the bowels of the earth, there reigns a certain symmetry and order, which indicates a force of incredible magnitude, but slow and gradual in its effects.

Further, that as a long period was required for the elevation of the strata, the rents made in them are not all of the same date, nor the veins all of the same formation. A vein that forces the other out of its place, and preserves its own direction, is evidently the more recent of the two.

The parallel coats lining the walls or sides of the vein, which are attributed by Werner and others to aqueous deposition, are ascribed to successive injections of melted matter.

Veins have been considered as traversing only the stratified parts of the globe. They do, however, occasionally intersect the unstratified parts, particularly the granite; the same vein often continuing its course across rocks of both kinds without suffering material change.

It is asserted that all the countries most remarkable for their mines are primary, and that Derbyshire is the most considerable exception to this rule that is known.

This preference which the metals appear to give to the primary strata, is considered as consistent with Dr. Hutton's theory; and particularly as these strata, being the lowest, have also the most direct communication with those regions from which the mineral veins derive all their riches.

In arguing further upon this theory, it is assumed that nothing of the substances which fill the veins is to be found anywhere at the surface; and that, contrary to the allegation of some that mineral veins are less rich as they go further down, it is stated that this is not generally so, and that the mines in Derbyshire and Cornwall are richest in depth, as they would be if filled with melted matter from below.

Again, it is said that if veins were filled from above, and by water, the materials ought to be disposed in horizontal layers across the vein; and that this opinion is sufficiently refuted by the fact that rarely any metallic ore is found out of the vein, or in the rock on either side of it, and least of all where the vein is richest.

The foregoing seem to be the most important allegations in support of the Huttonian theory; and I have taken them nearly in the order in which they are given in Professor Playfair's illustrations of this celebrated system.

There is yet another doctrine regarding the formation of veins, which, though it is not of modern date, and has had but few supporters among writers upon the subject, has yet claims to be considered, and particularly as it has of late been urged upon our notice, and by some whose observations have been made in districts where veins of various order are abundant.

This theory is, in short, That veins were formed at the same time with the rocks themselves; that the whole was a contemporaneous creation; and that there have been neither fissures subsequent to the consolidation of the mass, nor filling up from above or below, or disturbances to produce the heaves or shifts which we see.

When this hypothesis was first proposed I do not know, but that it was long since we may infer, as Agricola regards the opinion which supposes veins such as we now see them to have been formed at the same time with our globe, to be at variance with fact, and he calls it the opinion of the vulgar. The same hypothesis was indeed supported by Stahl; but he seems to have adopted it rather on account of the difficulties attendant on any other explanation that had been proposed, than for any good reason that he had to give.

Such are, however, but assertions, to be received with doubt by any one who inquires freely and without prejudice. Partial evidence may appear for some such formations; but it is another affair to attribute all veins to such an origin, and thus to sweep away at once the difficulty of explaining many complicated appearances.

The doctrine of a contemporaneous formation of veins has lately found an advocate in Dr. Boase, in his paper on the geology of Cornwall. After commenting on the division into different orders, which Mr. Carne had indicated as to veins, according to certain appearances in their direction and the character of the substances with which they are filled, he says he cannot detect any characters which are not common to all the Cornish veins; and since some of them are generally acknowledged to be contemporaneous with the rock, he concludes that they have all the same origin.

Dr. Boase, however, candidly sets out by stating that he had purposely refrained from making inquiries at the mines concerning the phænomena of veins, and that his experience is therefore principally confined to those which occur in cliffs, quarries, and natural sections that are exposed to open view.

Lest this admission should create surprise, he remarks that such sources of information are invaluable as the only ones easily available to exercise the senses on the nature of veins; for, unless to those much accustomed to descend into mines, they may as well be visited blindfold.

He remarks, however, as to the veins of Cornwall, that their great irregularity in size and in form, their frequent ramifications, their similarity of composition and intimate connexion with the rocks which they traverse, and, above all, the large masses of slate which they envelop, are all circumstances to disprove their origin from fissures, and to support their contemporaneous origin.

Dr. Boase suggests that veins follow the arrangement of the joints of the rocks, and that it may thus be explained why the different series of veins cross each other, and why the veins of each series are respectively parallel.

And he thinks that thus we may suppose how veins which are crossed may seem to abut or terminate against those that are opposed thereto; having, when in the same line, that peculiar appearance that has been attributed to intersection, and the appearance of being heaved when on the opposite sides of the cross vein, they are not on the same line, but occur in the parallel joints of distant layers.

The latter occurrence, he remarks, although very common, is not however universal; for, in some instances, the part of the vein supposed to have been intersected has never been found.

As Mr. Carne had observed, that when contemporaneous veins meet each other in a cross direction, they do not exhibit the heaves and interruptions of true veins, but usually unite.

Dr. Boase says that this statement is opposed to his observations, and that the phænomenon of intersection is common to all kinds of veins. Further, he expresses a doubt whether heaves in veins are not after all rather apparent than real, but explains that he does not mean to assert that they do not exhibit these phænomena, but that this arrangement, as in the case of small veins, only gives the appearance of being moved from the original positions.

I have now stated the opinions which, as far as I know, have been generally received on the subject of the formation of veins, from which it will appear that there are three leading hypotheses.

1st. That which supposes them to have been open fissures, caused by disruption, and occasioned principally by subsidence of parts of the rocks, which fissures were afterwards filled up with various matters by deposits from aqueous solution, chiefly from above.

Modifications of this theory are, That such rents in the earth may have been caused in other ways, such as earthquakes, or certain great convulsions, as well as by subsidence:

That they may have been filled by the infiltration of solutions, which deposited the substances with which they were charged in the veins, or by the process of sublimation from below.

The second theory allows that veins were formed subsequently to the consolidation of the rocks; but the cause principally assigned for such fissures is the violence done to the strata by the elevation or upheaving of other rocks from below.

And it is an essential part of this theory that the materials which fill the veins were forcibly injected upwards in a state of complete fusion by heat.

The third theory is that denying any subsequent processes which might either cause rents and fissures, or might fill them with matter which differs from the rocks which inclose them: the whole formation was contemporaneous with the rocks themselves, the mineral substances which we find in veins having separated and arranged themselves into the forms in which we now see them to exist.

The advocates of these theories have each zealously asserted the truth of his own system, and refused to admit of causes or explanations which appeared to militate against it; and thus a boundary has been set, as it appears to me, to that freedom of inquiry which is so desirable in such cases, and a limit drawn round the reasoning faculties of man upon evidence which may come before him.

It will appear, from what has already been said, that veins have very different characters and appearances; and this might be made more clear, if it were here the proper place to enlarge upon the subject and point out the distinctions. For our purpose, however, it may be sufficient to remark upon two or three principal varieties. First, then, are those which have beyond all comparison been most explored and examined, on account of the stores which they contain,—the metalliferous veins. As these have been penetrated in all directions to the greatest extent that human power and ingenuity have been able to effect, so their structure is better known and more accurately observed.

Similar to these, and occurring with them, and therefore well known, are others, which, though barren of metals, are yet often called true veins; and these, as well as the first, come pretty fully under the view of the miner.

Next there are veins, regular in their structure to a great extent, filled with matter which has the character of being derived from igneous origin, such as are usually called dykes of trap, whinstone, &c., &c.; to which would be added by most geologists of the present day, the veins of granite, porphyry, quartz, &c.

Some of these have been examined below the surface, where they pass through coal-fields, or other deposits of useful minerals, but containing in themselves nothing to reward the toil of exploring them: little has been seen of their contents and configuration, and our knowledge of them is more limited.

Lastly, there are tortuous and irregular veins or ramifications in most rocks, extending to limited distances, as far as our observations permit us to judge, seldom offering a valuable return for any effort to explore them, and of which, therefore, our knowledge is but superficial.

Such veins, according to Mr. Carne, have been usually distinguished from true veins by their shortness, crookedness, and irregularity of size, as well as by the similarity of the constituent parts of the substances which they contain to those of the adjoining rocks, with which they are generally so closely connected as to appear a part of the same mass. Two other distinctive marks may be added; one is, that when they cross they do not exhibit the heaves of true veins, but usually unite; the other is, that when there is an apparent heave it is easy to perceive that what appear to be separate parts of the same vein are different veins terminating at the cross vein.

Such may be, probably, of contemporaneous formation; and there may be deposits of ore also which it would be difficult to refer the structure of to any other hypothesis, particularly such as contain ores so intimately mixed with the rocks as to form a constituent part of them.

I would suggest, that if from *any one* of these classes we were to form a judgement as to *the whole*, error would probably be the consequence, or, at any rate, the view would be a narrow and contracted one, and our decisions would be defective in many important respects.

To have conducted the inquiry in this manner seems to me to have been the error in many who have preceded us in forwarding the state of knowledge on vein formations. Nor do I mean to detract from the great merit of many of them on this account; the field of observation is too vast to become fully acquainted with it; it extends over the most rugged parts of the earth's surface, and its boundaries are not reached in the deep recesses of its bowels. It is no wonder that in the earlier stages of such inquiries men should be strongly impressed with what lay immediately before them, and should view with distrust what they might only learn from description.

Such impressions may be traced in looking at the authors of the systems which we have reviewed. Werner expressly tells us, that we are indebted to miners for the theories which he deemed most worthy of acceptation, and he names as such Agricola, Rösler, Henkel, Hoffman, Von Oppel, Charpentier, and Trebra. We may add his own name and that of Dr. Pryce, in our own country, as intimately acquainted with mining. Now all such men would be more acquainted with the metalliferous veins and such as accompany them; and from these they would derive much evidence in favour of the opinions which they advocated; at least, partaking, as I probably do, in the same prejudices, so it would appear to me, if by the labour of other inquirers I did not know that there were other facts requiring a different explanation.

Again, Dr. Hutton and his commentators had largely observed veins which may fairly be attributed to injection; they had found dykes of trap passing through coal-beds, and converting them into cinder. Such evidence of the effects of heat and of a filling up by matter in fusion is not to be resisted; but when we look at what is said of the metalliferous veins by some of the writers on this side of the question, we observe great want of practical knowledge and many errors, arising out of the attempt to make all bend to a single method of solving the problem.

For the third hypothesis of contemporaneous formation there is this to be said,—that some veins exist which seem to admit of no other explanation; and that this being allowed to such as will have but one theory, this is at once the easiest, because it gets rid of many difficulties without further trouble; but we can hardly be satisfied to adopt it as universal upon experience that has been principally confined to sections in quarries and in cliffs, or to such as are exposed to open view.

Our present state of knowledge as to the formation of veins should therefore, in my opinion, be allowed to admit that most of the causes which have been stated have operated at various periods and through a long succession of time, some prevailing at one epoch, and some at another, modified by circumstances which we can but imperfectly comprehend or explain.

In this view we may allow of a classification of veins according to their probable mode of origin; and such a classification has been thought of by some of our ablest geologists of the present day, and was indeed propounded in one of our sections at Oxford last year by our present learned President, who expressed his opinion that there were three different sets of veins: —1. Those which have been plainly mere fissures or cracks, and which have been subsequently filled; 2. Those of injection; 3. The contemporaneous veins, which might more aptly be termed veins of segregation.

Here I might close this Report, which is already much too tedious, were it not that I may be expected to notice briefly some of the facts adduced by the advocates of the respective theories, and, by comparing them, show how far they are entitled to be considered as objections on one side, or as proofs on the other, with the confidence which has been assigned to them.

Werner and Hutton agree in allowing that rents took place subsequently to the consolidation of the rocks, or at the time of their consolidation. They differ as to the cause of the rents: Werner ascribes it to subsidence, or to sinking and shrinking of the solid materials of our globe; Hutton, to violent upheaving of matter from below, breaking up the superinjacent strata.

Either of these causes seems adequate to the effect, and in either case corresponding strata might be found having different levels of position on opposite sides of the fissure, as is constantly the case. This by miners in the North of England is called the *throw* of the vein; and it is clear that one side may as well be thrown up as the other thrown down. Mr. Fox and Dr. Boase urge the great irregularity of the width of veins, the difficulty of supposing the sides to be supported, and some other objections to the hypothesis of open fissures. Irregularity of width is but a comparative term; and taking into consideration the immense extent of their dimensions in length and depth, it amounts in my opinion to but little.

The other objections are in a great degree anticipated and answered by Werner; and, after all, difficulties can hardly be urged against the positive testimony of some veins having been open, which is afforded by the substances found in them, such as rolled pebbles, petrifactions, &c.

The parallelism of veins of one formation is insisted upon by Werner as a proof of his view of the subject; and I confess that there appears to me to be considerable difficulty in explaining this, on the supposition that fissures were caused by a mass protruded upwards through strata already formed. From such a cause one should expect not to have a number of cracks parallel to each other, but rather to see them radiating from the centre of the greatest disturbance. In the metalliferous veins we may certainly observe this parallelism to a great extent. Mr. Carne has beautifully illustrated this in Cornwall, and has shown how the productive veins generally have an east and west course; how, as they differ in their contents, they differ also in their direction, each class being, however, parallel in itself; and how these facts illustrate relative ages of formation.

This tendency to an east and west direction of the metalliferous veins may be observed not only in Cornwall but in the stratified parts of England, in the mining districts of Europe, and in the range of the great veins of Mexico.

Mr. Robert Fox, having discovered galvanic action to ensue by the connexion of an apparatus, constructed to detect it, with portions of metalliferous veins, suggests whether some analogies may not be traced between electro-magnetic currents and the directions of veins: nothing upon which any hypothesis can be built seems, however, as yet to have been proposed; and it may be doubted whether, when this test is applied to masses of ore, the experiment is not liable to many objections. A principal one seems to be, that by the very act by which we gain access to the vein, we lay it open to atmospheric action, and consequently to decomposition. Chemical agency commences, and with it, very naturally, galvanic influences are excited.

Veins containing ores little subject to decomposition have, I apprehend, been found to give little or no indications of this nature.

It may, however, be that this general direction of metalliferous veins may not obtain as to veins of injection; and in that case we shall have additional reason to admit more causes than one to have been in operation. This is a matter deserving extensive observation.

Other veins have been stated to cross the metalliferous veins: they are generally filled in a different manner. If they contain any ores, they are frequently of different metals from those in the former. They pass through or traverse the other veins, cutting them through, and suffering a disturbance to take place in their linear direction, or what the miners significantly term a *heave*.

This fact is relied upon as proving that veins are of different ages, as first asserted by Pryce, much insisted upon by Werner, and allowed by Hutton and Playfair.

Those who dispute this inference, therefore, are the advocates for the sole operation of contemporaneous causes: they object that rules which have been proposed for ascertaining the exact tendency of such disturbances having been found to be subject to exceptions, the proof of dislocation is wanting, or that dislocation has taken place without motion. The latter proposition, at any rate, appears to me to be very difficult to understand; and I think if any part of this intricate subject is clear and intelligible, it is that the relative age of veins is made out by these facts, even although we may not yet be able to apply rules for every case,—a subject which has been considered as highly important in its practical application to the art of mining.

The greatest controversy, however, relates to the mode in which veins have been filled. Here, again, we must remark, how the opinions of observers have been influenced by the facts coming under their immediate observation.

Werner, and the mining authors on whom he relies, drew their inferences from metalliferous veins. Hutton and his followers regarded chiefly those of another class; and this great author and his commentator Professor Playfair were evidently ill informed as to metalliferous veins.

That certain veins have been filled by injection from below, and with matter in igneous fusion, seems to be rendered certain by evidence, which is clearer than most we possess on such subjects, and must be admitted at once. Thus, when we see a trap dyke traversing a bed of coal and charring the combustible matter, and affecting the rock itself with visible effects of great heat, we must assent to the cause assigned; and when we see matter of igneous origin not only filling the veins, but overflowing on the surface, or insinuating itself between adjacent beds, the case is plainer than most that occur in geological research.

But though one class of theorists have proposed this as the universal cause of the filling up of veins, ought we to admit this to be true, when we find so many in which no similar appearances are to be traced?

Why, for instance, if the ores were forced from below, did the power which injected them just limit itself to raising them within a short distance of the surface,—for where shall we find an instance of their being protruded above it?

If the metallic contents of veins were injected from below, we ought to be able to trace something like the direction of the currents in which the matter flowed; we ought to see some continuity in the operation, and some connexion between the masses of ore which occur in veins; whereas the contrary of each is notoriously evident to every observer.

It would seem also to be very probable, if the enrichment was from below, and the matter was forced in from those regions whence their treasures are supposed to be derived, that by a nearer approach to the depths of the earth we should find the riches more abundant.

Professor Playfair admits this inference, and disposes of the difficulty by arguing that it is so; and says, that though mines in Mexico and Peru are said to be less rich as they descend further, those of Derbyshire and Cornwall exhibit the very contrary.

He is unfortunate in this allegation, and the facts will not bear him out, as every one of common experience must know; and thus, as I have before observed, we have hypotheses supported by a limited knowledge of the facts.

The theory of the filling up of veins by precipitation from aqueous solutions, is defective in not being able to show what menstruum could render such substances soluble in water; and this difficulty must remain an important one, unless enlarged knowledge should hereafter afford the means of explaining it.

But when we are told that the supposition is absurd, that water cannot arrange its deposits in planes highly inclined, that no appearance of stalactites is to be found in veins, nor can we see in them any substance like those on the earth's surface, which aqueous action has removed,—it must be recollected that we know silex is soluble in water at high temperatures; that crystals do arrange themselves on the sides of vessels in planes highly inclined; that stalactites of chalcedony, of quartz, and of iron pyrites, have been found deep in the veins in Cornwall, and that much of the substance of the surrounding rocks, and such as we see on the surface, and adjoining and inclosing the veins themselves, is found in them, occupying much of their space, previously having been worn down into fragments, into loose sand, and into clay or mud, the latter of which is so common that, as I have before observed, it is relied on by the miner as a distinguishing character of regular veins*.

The action of water may, I think, be as fairly assumed as that of fire; and we may consider what their joint powers might be, when compelled, as it were, to act together, under circumstances that immense pressure might produce.

But in examining the contents of veins, we are, I think, likely to be struck, not only by the appearance of a complication of causes, but by evidence of their succession, admitting the probability not only of different agents having been employed, but of their having done their work separately as well as conjointly,

* Mr. Weaver describes the contents of the great vein of Bolaños in Mexico thus: "The chief mass of this vein may be said to consist of the detritus of the adjacent rocks, more or less consolidated, and generally hard; nay, in places, it is actually composed of a conglomerate. Proper vein-stones, such as fluor or calc spar, are, comparatively speaking, casualties. In this basis the finer delicate silver ores and native silver are dispersed, in common with the harder and coarser ores of blende, iron, and copper, besides lead ores."

—of having operated at different periods, and of one having produced effects for which another was inadequate.

As we cannot easily conceive how the metallic ores can have been deposited from solution in water, and appearances are much against their having been injected in a state of fusion, there is another supposition which, though not free from difficulties, has yet probability enough in its favour to have gained it many supporters,—which is, that these and some other substances have been raised from below by sublimation. This is not a new opinion, for though the older writers expressed it in an indistinct manner, and spoke of metallic vapours and exhalations,—and thus we shall find it proposed by Becher, Stahl, Henkel, and others,—yet their meaning evidently was, that substances had been volatilized by heat, and assumed their places in veins by condensation, or by combining with other materials.

We know for certain that some of the metallic sulphurets may be so volatilized, and will reassume their form and be produced in a crystallized state; and so far nothing is assumed beyond our knowledge: but as we find these sulphurets, which compose by far the greater part of the metallic contents of veins, in insulated masses, surrounded on all sides by other substances, which we can hardly conjecture to have been sublimed, we encounter much difficulty in explaining how the process can have taken place; and it becomes even more difficult when we see how very much these different classes of substances are incorporated, and how they completely, in most instances, envelop and inclose each other.

The hypothesis of filling up by sublimation would also seem to require that the deepest portions of veins should be richer, especially considering the very small extent to which after all they have been perforated; but yet, shallow as our workings into the earth really have been, there is much appearance of their having in many instances gone below the richest deposits of the metals.

This seems to have been the case in some of the deepest mines in Mexico, and in several in our own country. It is impossible, indeed, to say that greater deposits may not exist still lower down; and though veins have not been traced to their termination, they have in many instances been pursued until the indications of metallic produce have become faint and hopeless. And these unfavourable appearances have increased very commonly with increasing depth, which is as much, perhaps, as we are likely to know about it, as the operations of the miner are thus arrested, and the inducement to further experiment is taken away.

The agency of sublimation has lately been advocated by Professor Necker of Geneva, in a paper read before the Geological Society of London*; and he has extended an ingenious hypothesis of Dr. Boué, who would bring under a general law the relation of metalliferous veins and deposits to those crystalline rocks which, by the majority of modern geologists, are considered to have been produced by fire; and thus to lead to the inference that the metals were deposited in the former by sublimation from the latter.

M. Necker inquires, 1. Whether there is near each of the known metalliferous deposits any unstratified rock?

2. If none is to be found in the immediate vicinity, is there no evidence which would lead to the belief that an unstratified rock may extend under the metalliferous district?

3. Do there exist metalliferous deposits entirely disconnected from unstratified rocks?

Professor Necker answers these questions by showing that in various countries there are such relations as he supposes, and admits, in reply to the last, that there are cases where the deposits seem to be unconnected with any trace of unstratified rock.

If metalliferous deposits are commonly in crystalline rocks which are attributed to igneous origin, it must be allowed also that there are others abundantly rich where no apparent connexion is to be traced. M. Necker mentions the mountain limestone as such; but he does not seem aware of the extent of those deposits, which, with the beds of grit and shale which alternate with it, present numberless regular veins abounding with certain ores.

As this fact is indisputable, it seems necessary to show not only that unstratified rocks may be under them—which there is little doubt about,—but that there should be some connexion between the veins which contain the metals and similar channels or passages in the rocks below. No such evidence, I believe, at present exists; and I am not aware of any veins having yet been found to penetrate from the stratified rocks into those upon which they rest.

This supposition must therefore, like many others, be taken as a mere probability to account for some appearances in certain places, but not to explain all the phænomena.

There is one point which, before I conclude, I would endeavour to press on the attention and consideration of future observers, because, in the first place, it does not appear to have been much regarded by writers on the subject; and next, be-

* March 28th, 1832.

cause, though it seems to offer objections to some received theories, it may, when better understood, assist in developing the truth.

This is the relation that the contents of a vein bear to the nature of the rock in which the fissure is situated.

Thus in the older rocks, we see the same vein intersecting clay-slate and granite: it is itself continuous, and there is no doubt of its identity; and yet the contents of the part inclosed by the one rock shall differ very much from what is found in the other. In Cornwall, a vein that has been productive of copper ore in the clay-slate, passing into the granite becomes richer, or, what is more remarkable, furnishes ores of the same metal differently mineralized. If we pursue it further into the granite, the produce of metal frequently is found to diminish.

Veins in some cases cut through the elvan courses, as well as the clay-slate inclosing these porphyries: the ores are rich and abundant in the latter; in other instances they fail altogether.

Less striking differences in the structure of the rock seem to affect the contents of the veins; and appearances as to the texture and formation of the strata are often regarded by miners with more anxiety than the indications presented by the vein itself; and a *change of ground* is relied upon with an assurance, derived from experience, as a more certain basis to augur upon, for better or for worse, than almost any other which the difficult art of mining has to offer.

Numberless facts might be collected and adduced to show that this is not mere speculation; but it will nowhere appear more clearly than if we examine the various beds of limestone grit, &c., in the great lead mines in the North of England.

Here we shall find a series of stratified rocks, and that portion of the series which has been most productive of lead ores, occupying a thickness of nearly 280 yards. It is divided into 55 distinct beds, which are accurately described in Mr. Westgarth Forster's section, each having its name known to the miners of the country. Nine of these beds are of limestone, about 18 are of gritstone or siliceous sandstone, and the remainder are plate or black shale, with thin beds of imperfect coal.

Now the lead veins pass through all these beds, and have been worked more or less into all of them; and it has thus been proved, that though the fissure is common to all, yet lead ore is only found abundantly in particular beds, and those very much the same, if we examine the immense number of mines which are working in this district.

Where the veins pass through the shale, little or no ore is to be found in them; where they are inclosed by the gritstones, there they become more productive; but it is in one of the beds of limestone, and one only, that the great deposit of lead ore is to be found.

In the great mining field of Alstone Moor, this bed is called the great limestone, and yet its thickness is only about 23 yards out of the 280 which the series of lead measures occupy; and notwithstanding this, four-fifths of all the lead ore found in the district is derived from such parts of the veins as are inclosed by this particular stratum.

The veins equally passing through the other beds, and traced by innumerable workings through them, are yet only rich in metallic treasure where they repose in this favoured stratum*.

Though perhaps few cases are so striking as this, yet it is evident that the same thing takes place to a certain extent with all the metals, in all rocks and in all countries.

If it is a fact and correctly stated, it must be considered in reference to the theories propounded to us, and it seems directly opposed to the doctrine of forcible injection; but it may admit of probable explanation by calling in certain affinities, either by the advocates of precipitation from water, or by those who may contend that sublimed vapours might be attracted to particular spots.

* To illustrate the comparative bearing of the different beds in the manor of Alstone Moor, Mr. Thomas Dickinson, the Moor Master for Greenwich Hospital, extracted for me an exact account of the ore produced from each bed in all the mines of the manor in the year 1822, which gave the following results:—

Limestone Beds.—	Great limestone	20,827	bings.
	Little limestone	287	
	Four-fathom limestone	91	
	Scar limestone	90	
	Tyne bottom limestone	393	
		—	21,688
Gritstone Beds.—	High slate sill	107	
	Lower slate sill	289	
	Firestone	262	
	Pattinson's sill	259	
	High coal sill	327	
	Low coal sill	154	
	Tuft	306	
	Quarry hazel	44	
	Nattrass Gill hazel	21	
	Six-fathom hazel	576	
	Slaty hazel	18	
	Hazel under scar limestone	2	
		—	2,365
Whole produce of the mines of the manor			24,053 bings.

That metallic ores are found to repose in rocks which seem congenial to them, and that their combinations are modified by changes in the rocks, will not I think be disputed by practised miners, or by those who have most narrowly searched into the hidden recesses of the earth.

Facts must be observed and compared, effects must be traced to probable causes, and difficulties must be explained or candidly admitted, if we would enlarge and generalize our knowledge of vein formations. There are obstacles to the progress of this knowledge; for, as Dr. Boase has remarked, it is not easy for a person unaccustomed to it to use his eyes with much advantage, in the places where the study can best be pursued.

It is the miners' business, however, not only to see clearly, but to consider all the intricate appearances that veins exhibit; and I would exhort them not to be satisfied merely with the observations their art may seem to require, but to extend them to a larger view of the subject, and to contribute, as many of their eminent predecessors have done, to the common stock of general science.

If the imperfect view which I have thus endeavoured to give of prevalent opinions should assist in such endeavours, or should stimulate any persons in undertaking a further pursuit of the subject, it would be to me a source of great gratification; as the desire of promoting such inquiries must be my apology for attempting the task which I have undertaken.

[illegible]

On the Principal Questions at present debated in the Philosophy of Botany. By John Lindley, *Ph. D., F.R.S., &c., Professor of Botany in the University of London.*

If we compare the state of Botany at the end of the last century with its present condition, we shall find that it has become so changed as scarcely to be recognised for the same science. Improvements in the construction of the microscope, the discoveries in vegetable chemistry, the exchange of artificial methods of arrangement for an extended and universal contemplation of natural affinities, the reduction of all classes of phænomena to general principles, and, above all things, the adoption of the philosophical views of Göthe, together with the recognition of an universal unity of design throughout the vegetable world, are undoubtedly the principal causes to which this change is to be ascribed.

As the general nature of recent discoveries, and a sufficiently definite notion of the present state of botanical science, may be collected from the introductory works which have appeared in this country within the last three years, it is presumed that the object of the British Association will be attained if the present Report is confined to the most interesting only of those subjects upon which botanists have been recently occupied, and to an indication of the points to which it is more particularly desirable that inquiries should now be directed. I have also excluded everything that relates to mere systematic botany, in the hope that some one will take that subject as the basis of a separate Report.

Elementary Organs.—This country has, till lately, been remarkably barren of discoveries in vegetable anatomy, since the time of Grew, who was one of the fathers of that branch of science. Whatever progress has been made in the determination of the exact nature of those minute organs, by the united powers of which the functions of vegetation are sustained, it has been chiefly in foreign countries that it has taken place: the names of Mirbel, Moldenhauer, Kieser, Link and Amici, stand alone during the period when their works were published; and it has only been within a very few years that those of Brown, Valentine, Griffith and Slack have entered into competition with the anatomists of Germany and France.

By the researches of these and other patient inquirers, we have already reduced our knowledge of the exact internal structure of plants to a state of very considerable precision; although it must be confessed that vegetable anatomy is still the field where the greatest discoveries may be expected.

It is now generally agreed that the old opinions, that the tissue of plants is either a membrane doubled together in endless folds, or a congeries of cavities formed in solidifiable mucus by the extrication of gaseous matter, are equally erroneous, and that it really consists of distinct sacs or cells, pressed together and adhering to each other by the sides where they are in contact.

It is considered that this is proved by the following circumstances. 1. By the action of some powerful solvent, such as nitric acid, the cells may be artificially separated from each other. 2. In parts which become succulent, the cells separate spontaneously, as in the receptacle of the strawberry, the berry of the privet, &c. 3. When the parts are young, their tissue may be easily separated by pressure in water. 4. It is conformable to what has been observed in the growth of plants. Amici found that the new tubes of *Chara* appear like young buds from the points or axillæ of pre-existing tubes; an observation that has been confirmed by Mr. Henry Slack*. It has been distinctly proved by M. Mirbel†, that the same thing occurs in the case of *Marchantia polymorpha.* That learned botanist, in the course of his inquiries into the structure of this remarkable plant, may be said to have been present at the birth of its cellular tissue; and he found that in all cases one tube or utricle generated another, so that sometimes the young masses of tissue had the appearance of knotted or branched cords. He satisfied himself, by a beautifully connected series of observations, that new parts are not formed by the adhesion of vesicles originally distinct, as many have asserted, but by the generative power of one first utricle, which engenders others endowed with the same property.

It appears that when first formed the sacs are completely *closed up,* so that there is no communication between the one and the other, excepting through the highly permeable membrane of which they are composed. This, indeed, is not conformable to the observations of those who have described and represented pores or passages of considerable magnitude pierced in the sides of the sacs; but it has been satisfactorily shown by Dutrochet, that the spaces supposed by such observers to be

* *Transactions of the Society of Arts,* vol. xlix.

† "Recherches Anatomiques et Physiologiques sur le *Marchantia polymorpha,*" in *Nouv. Ann. du Muséum,* vol. i. p. 93.

pores are nothing more than grains of amylaceous matter sticking to the sides of the sacs; for he found that by immersing the latter in hot nitric acid, the supposed pores became opake, and by afterwards moistening them with a weak solution of caustic potash, they recovered their transparency: we also find that the supposed pores are readily detached from the sides of the sacs to which they adhere; and I think it may be added, that our microscopes are now alone sufficient to show what they are.

The question as to the perceptible porosity of vegetable tissue may therefore be considered, I think, disposed of as a general fact; for the objection that Dr. Mohl has taken to this explanation *,—namely, that in a transverse section we ought to find such grains projecting from the sides of the cells like little eminences,—cannot surely be entitled to much weight, if we oppose to this negative observation the positive evidence already mentioned, and especially if we consider that it is next to impossible for the keenest knife to make a section of such delicate parts without carrying away such particles upon its edge. There are, nevertheless, cases in which the point is still open to investigation.

Thus Mirbel, in his second memoir on the *Marchantia* †, positively declares that the curious cells which line the anther of the common gourd, are continuous membranes till just before the expansion of the flower, when they very suddenly enlarge, and their sides divide into the narrow ribands or threads which give their name to what we call fibrous cells. In this, and the multitudes of similar cases with which Purkinje has made us acquainted, there can be no doubt that the sides of the cells consist *ultimately* of nothing but openwork; but still it seems certain that during the principal part of their existence they were completely closed up.

It is also probable that in other cases the sides of the cells or vessels ultimately give way and slit; but this rending seems to be a phænomenon attendant upon the cessation of the ordinary functions of tissue, and independent of their original construction.

In coniferous plants the wood is in a great measure composed of closed tubes, tapering to each end, the sides of which are marked with circles, containing a smaller circle in their centre. These circles have long been considered undoubted pores, and it does not appear possible to prove them otherwise by any of the tests already mentioned.

* *Ueber die Poren des Pflanzen-Zellgewebes*, p. 11. Tubingen, 1828.
† *Archives de Botanique*, vol. i.

I have endeavoured to show * that they are glands of a peculiar figure, which stick to the sides of the tubes; and I have ascertained that the large round holes that are certainly found in coniferous tissue are caused by the dropping or rubbing off of such supposed glands. But a very different opinion is entertained by Dr. Mohl †, whose observations have been confirmed by Dr. Unger ‡. In the opinion of the former of these botanists the supposed glands of coniferous tissue are circular spaces where the membrane of the tube becomes abruptly extremely thin; and it is said that transverse slices of coniferous wood, made at an angle of forty-five degrees, demonstrate the fact. Dr. Mohl is also of opinion, as has been already said, that the porous appearances above mentioned, and ascribed to the adhesion of amylaceous matter to the sides, are of a similar nature.

It has been shown by Mr. Griffiths, that in the kind of tissue called the dotted duct, the suspicion of Du Petit Thouars that this form of tissue is composed of short cylindrical cells placed end to end, and opening into each other, is correct; their communication, however, is not by means of an organic perforation, but is produced by the absorption and rupture of the ends which come in contact. Mr. Slack has also stated, in a very good paper upon Vegetable Tissue §, that in other cases the vessels of plants open into each other where they come in contact; as, for example, at the conical extremities, where ducts join each other; but he represents this to be owing to the obliteration of their membrane at that point; the internal fibre, of which they are in part composed, remaining like a grating stretched across the opening where the enveloping membrane has disappeared.

In a short paper, published in the *Journal of the Royal Institution* in December 1831, I have endeavoured to show that membrane and fibre are to be considered the organic elements of vegetable tissue, contrary to the more usual opinion that membrane only is its basis: this was attempted to be proved, not only by the fact that the simple cells of the testa of *Maurandia*, &c., are apparently formed by a fibre twisted spirally in the inside of their membrane, but also by the elastic spires I had discovered on the outside of the seed of *Collomia*, in which it is plain that no membrane whatever is generated.

* *Introduction to Botany*, p. 16. t. 2. f. 7.
† *Ueber die Poren des Pflanzen-Zellgewebes.*
‡ *Botanische Zeitung*, October 7, 1832.
§ *Transactions of the Society of Arts*, vol. xlix.

It would, however, appear from the researches of Mirbel*, that the presence of a twisted fibre within a cell is not always the cause of the spiral or fibrous character so common in tissue. He finds, as has been already stated, that the cells that line the anther of a gourd are at first membranous and closed, and that they continue in this state till just before the bursting of the anther, when they suddenly divide in such a way as to assume the appearance of delicate threads, curved in almost elliptical rings, which adhere to the shell of the anther by one end; these rings are placed parallel with each other in each cell, to which they give an appearance like that of a little gallery with two rows of pilasters, the connecting arches of which remain after the destruction of the roof and walls. He also watched the development of the curious bodies called *elaters* in the *Marchantia*, which he describes to the following effect. At first they are long slender tubes, pointed at each end; at a subsequent stage their walls thicken, and become less transparent, and are marked all round through their entire length with two parallel, very close, spiral streaks; later still the tubes enlarge, and their streaks become slits, which divide the walls all round, from one end to the other, into two filaments; and, finally, the circumvolutions of the filaments separate, assume the appearance of a corkscrew, acquire a rust colour, and the elater is complete. These elaters he considers organically identical with the spiral vessel, and hence he concludes that every description of vessel is a cell, differing from ordinary cells in being larger.

Upon the general accuracy of these observations I am disposed to place great confidence; and I would even add, that the theory of pierced or open cellular tissue being produced by the spontaneous rending of its membrane, is apparently connected with an observation of my own†, that in some plants simple vegetable membrane will tear more readily in one direction than another. It is nevertheless to be observed, that the theory of fibre being one of the organic elements of tissue does not seem to have occurred to the experienced physiologist to whose observations I am referring, and that some of the appearances he mentions at a stage preceding transformation are very like those of the development of an internal fibre.

The opinion of the organic identity of all the forms of tissue has also been maintained by Mr. Slack, in the paper already referred to, and by Dr. Mohl, in his memoir on the comparative anatomy of the stem of *Cycadeæ*, *Coniferæ*, and *Tree Ferns*.

* *Archives de Botanique*, vol. i.

† *Introduction to Botany*, p. 2.

The latter considers that the dotted tubes of *Cycadeæ* undoubtedly pass directly into the vessels called by the Germans *vasa scalariformia*; but my own observations do not confirm this statement.

Circulation.—Whether or not plants have a circulation analogous to that of animals, is a topic that was more open to conjecture at a time when the real structure of the former was unknown, than it can be at the present day. Knowing, as we now do, that a tree is more analogous to a Polype than to a simple animal; that it is a congeries of vital systems, acting indeed in concert, but to a great degree independent of each other, and that it has myriads of seats of life, we cannot expect that in such productions anything absolutely similar to the motion of the blood of animals from and to one common point should be found. The idea of circulation existing in plants must therefore be abandoned; but that a motion of some kind is constantly going on in their fluids was sufficiently proved by the well-known facts of the flow of the sap, the bleeding of the vine, the immense loss plants sustain by evaporation, and by similar phænomena. The motion was for the first time beheld by Amici, the Professor at Modena, who discovered it in the *Chara*. He found that in this plant the cylindrical cells of the stem are filled with fluid, in which are suspended grains of green matter of irregular form and size. These grains were distinctly seen to ascend one side of each tube, and descend the other, after the manner of a jack-chain, and to be continually in action, in the same manner, as long as the cell retained its life; the motion of the grains was evidently due to the ascending and descending current in the fluid contained within the tube-like cell. It could not be ascertained that any kind of communication existed between the cells, but each was seen to have a motion of its own.

The observations of Amici have been verified in this country chiefly upon species of *Nitella*; and from the investigations of Mr. Solly, Mr. Varley, and Mr. Slack*, the nature of the phænomenon has been determined with considerable precision.

Among other things, it has been ascertained that in *Nitella* the currents have always a certain relation to the axis of growth, the ascending current uniformly passing along the side of the cell most remote from the axis, and the descending current along the side next the axis.

Similar motions have been seen in several other plants. In the cells of *Hydrocharis Morsus-Ranæ* the fluid has been ob-

* *Transactions of the Society of Arts*, vol. xlix.

served to move round and round their sides in a rotatory manner, which, however, has not been seen to follow any particular law. In the joints of the hairs of *Tradescantia virginica* several currents of a similar nature exist; and in the hair of the corolla of a species of *Pentstemon*, Mr. Slack has observed several currents taking various directions, some continuing to the summit of the hair, whilst others turn and descend in various places, two currents frequently uniting in one channel.

It may hence, possibly, be assumed that in the cells of plants, when filled with fluid, there is a very general rotatory movement, which is confined to each particular cell. This, it is obvious, can form no part of the general circulation of the system, which must often occur with great rapidity, and which must take place from the roots to the extremities. The rotatory motion may perhaps be considered a sort of motion of digestion, and connected with the chemical changes which matter undergoes in the cells from the united action of light, heat, and air.

What has been supposed to be a discovery of the universal motion of sap has been made by Professor Schultz of Berlin, who remarked two torrents, one of which was progressive, and the other retrogressive, in what he calls the vital vessels (apparently the woody fibre) in the veins of *Chelidonium majus*, and in the stipulæ of *Ficus elastica*.

His observations have been repeated by a Commission of the Institute, composed of MM. Mirbel and Cassini, who have reported* that they have also seen the motion described by Professor Schultz; and I have myself witnessed it as is represented by those observers. But it appears probable, from several circumstances, that the motion that has been seen has either been owing merely to the vessels in which it was remarked having been cut through, and emptying themselves of their contents, as Mr. Slack has suggested, or else was nothing but the common rotatory motion imperfectly observed.

Structure of the Axis.—From the period when M. Desfontaines first demonstrated the existence of two totally distinct modes of increase in the diameter of the stems of plants, it has been received as a certain fact that monocotyledonous plants increase by addition to the centre of their stem, and dicotyledonous by addition to the circumference. Nothing has yet arisen to throw any doubt upon the exactness of this notion in regard to dicotyledonous plants; but Dr. Hugo Mohl has endeavoured to show† that monocotyledonous stems are not

* *Annales des Sciences*, vol. xxii. p. 80.

† Mohl, "De Palmarum Structura," in Martius's *Genera et Species Palmarum.*

formed in the manner that has been supposed. According to him, the new matter from which the wood results is not a mere addition of new matter to the centre, but consists of bundles of wood, which, originating at the base of the leaves, take first a direction towards the centre, and then a course outwards towards the circumference, forming a curve; so that the stem of a Palm is, in fact, a mass of woody arcs intersecting each other, and having their extremities next the circumference of the trunk. I regret that I have not been able to consult Dr. von Martius's splendid work on Palms since this Report was commenced, and that I am therefore unable to state upon what evidence Dr. Mohl has rested his theory.

The same writer has stated* that *Cycadeæ*—that singular tribe, which is placed, as it were, on the boundary line between cellular and vascular plants,—are not in a great measure destitute of vessels as is commonly supposed, but, on the contrary, are composed exclusively of spiral vessels and their modifications, without any mixture of woody fibre. I have already adverted to this hypothesis in speaking of the same author's statement, that the dotted tubes of *Cycadeæ* are a slight modification of *vasa scalariformia*. Dr. Mohl is also of opinion that *Cycadeæ* are not exogenous in their mode of growth, as seems to be indicated by their appearance when cut, and by their dicotyledonous embryo, but that they are more like Palms in their manner of forming their wood, which is essentially endogenous. He asserts that the stem of *Cycadeæ*, in regard to its anatomical condition, must be considered intermediate between that of *Tree Ferns* and *Coniferæ*, just as their leaves and fructification undoubtedly are. He states that in *Cycadeæ* a body of wood is gradually formed of the fibres connected with the central and terminal bud; that so long as this original wood is soft, and capable of giving way to the fibres that are continually passing downwards, no second cylinder of wood is formed; but in time the original wood becomes hardened, and then the new fibres find their way outward and downward, collecting into a second cylinder on the outside of the original wood. It is obvious that this explanation is not so satisfactory as could be desired; for, in the first place, such a distinction between *Cycadeæ* and *Exogenæ* as that which Dr. Mohl states to exist, is verbal rather than real, since he admits that the second cylinder of wood is formed externally to the first; and secondly, it is obvious that if that structure which is represented in the 21st plate of the third volume of the *Hortus*

* *Ueber den Bau des Cycadeen Stammes und sein Verhaltniss zu den Stamm der Coniferen und Baumfarrn.* 4to. Munich, 1832.

Malabaricus be correct, where the stem of *Cycas circinalis* is shown to have several concentric zones, precisely as in other exogenous trees, it must follow that Dr. Mohl's explanation would be still more inadmissible; accordingly, this author discredits the fact of the stem of *Cycas circinalis* having numerous concentric zones. It is, however, certain, from the specimens brought to England by Dr. Wallich, that the structure of this *Cycas* is really such as is shown in the *Hortus Malabaricus*. It is nevertheless extremely well worth further inquiry whether there is not some important but as yet undiscovered peculiarity in the mode of forming their stem by *Cycadeæ*; for it must be confessed that growth by a single terminal bud, after the manner of Palms, is not what we should expect to meet with in exogenous trees.

Professor Schultz of Berlin has indicated* the existence of a group of plants, the structure of whose stems he considers at variance with all the forms at present recognised; and to this group he refers *Cycadeæ*: but the assemblage of orders which he collects under what he calls the same plan of growth is so extremely incongruous as to lead to no other conclusion than that subordinate modifications of internal structure are of no general importance, but are merely indicative of individual peculiarities.

Dr. Mohl further states, that Cryptogamic plants of the highest degree of organization, such as *Ferns*, *Lycopodiaceæ*, *Marsileaceæ*, and *Mosses*, in all which a distinct axis is found, have a mode of growth neither exogenous nor endogenous, but altogether of a peculiar nature. In these plants, when once the lower part of the stem is formed it becomes incapable of any further alteration, but hardens, and the stem continues to grow only by its point, which lengthens merely by the progressive development of the parts already formed, without sending downwards any fibrous or woody bundles, as both in exogenous and endogenous plants.

M. Lestiboudois, the Professor of Botany at Lille, distinguishes Monocotyledons from Dicotyledons, upon principles different from those generally admitted. According to this writer, dicotyledonous trees have two systems, one, the *central*, consisting of the medullary sheath and the wood; the other, the *cortical*, composing the bark. These two systems increase separately, so that in Dicotyledons there are two surfaces of increase, that of the central system, which adds to its outside, and that of the cortical system, which adds to its inside: but

* *Naturliches System des Pflanzenreichs nach seiner inneren Organization.* 8vo. Berlin, 1832.

in the stem of Monocotyledons there is only one surface of increase, namely, that on the inside; and hence he concludes that such plants have only a cortical system, and consist of bark alone. It must be obvious that there are too many anatomical objections to this theory to render it deserving of any other than this incidental notice*.

The *cause of the formation of wood* has always been a subject upon which physiologists have been unable to agree; and if the opinions held by the writers of the last century have been disproved, it cannot be added that those of the present day are by any means settled. It is now, indeed, admitted on all hands that wood is a deposit in some way connected with the action of leaves; for it has been proved beyond all question that the quantity of wood that is formed is in direct proportion to the number of leaves that are evolved, and to their healthy action, and that where no leaves are formed, neither is wood deposited. But it is a subject of dispute whether wood is actually organized matter generated by the leaves, and sent downwards by them, or whether it is a mere secretion, which is deposited in the course of its descent from the leaves to the roots. The former opinion has been maintained in different forms by De la Hire, Darwin, Du Petit Thouars, Poiteau, and myself, and would perhaps have been more generally adopted if it had not been too much mixed up with hypothetical statements, to the reception of which there are in the opinion of many persons strong objections. For example, it has been asserted that the wood of trees is an aggregation of the roots of myriads of buds in a state of action, and that consequently a tree is an association of individuals having a peculiar organic adhesion and a common system of growth, but each its own individual life. To this view it is no doubt very easy to raise objections, some of which it may be difficult, in the present state of our knowledge, to answer; and therefore it is better for the moment to leave this part of the proposition out of consideration, and to confine it to the simple statement that wood is organized matter, generated by the leaves, and sent downwards by them. In support of this it is argued: 1st, That an anatomical examination of a plant shows that the woody systems of the leaf and stem are continuous: 2ndly, That this is not only the fact in exogenous plants, but in all endogenous and cellular plants that have been examined; so that it may be considered a universal law: 3rdly, That in the early spring, and for some time after plants begin to grow, the woody matter is actually to be seen and traced

* Achille Richard, *Nouveaux Élémens de la Botanique*, 5me edit. p. 119.

descending in parallel tubes from the origin of the leaves, and from no other place: 4thly, That in all cases where obstacles are presented to the descent of such tubes, they turn aside, and afterwards resume their parallelism when the obstacle has been passed by: 5thly, That in endogenous plants, such as Palms, and in some exogenous trees, such as *Lignum Vitæ*, they cross and interlace each other in a manner which can only be accounted for by their passing downwards, the one over the other, as the leaves are developed: and, finally, That the perfect organization of the wood is incompatible with a mere deposit of secreted matter. To all which the following evidence has been added by M. Achille Richard. He states* that he saw in the possession of Du Petit Thouars a branch of *Robinia Pseudacacia*, on which *Robinia hispida* had been grafted. The stock had died, but the scion had continued to grow, and had emitted from its base a sort of plaster, formed of very distinct fibres, which surrounded the extremity of the branch to some distance, and formed a sort of sheath; thus demonstrating incontestibly that fibres do descend from the base of the scion, to overlay the stock.

To this several objections have been taken, the most important of which are the following. If wood were really organized matter, emanating from the leaves, it must necessarily happen that in grafted plants the stock ought in time to acquire the nature of the scion, because its wood would be formed entirely by the addition of new matter, said to be furnished by the leaves of the scion; so far, however, is this from being the fact, that it is well known that in the oldest grafted trees there is no action whatever exercised by the scion upon the stock, but that, on the contrary, a distinct line of organic demarcation separates the wood of the one from the other, and the shoots emitted from the stock by wood said to have been generated by the leaves of the scion, are in all respects of the nature of the stock. Again,—if a ring of bark from a red-wooded tree is made to grow in the room of a similar ring of bark of a white-wooded tree, as it easily may be made, the trunk will increase in diameter, but all the wood beneath the ring of red bark will be red, although it must have originated in the leaves of the tree which produces white wood. It is further urged, that in grafted plants the scion often overgrows the stock, increasing much the more rapidly in diameter, or that the reverse takes place, as when the *Pavia lutea* is grafted upon the common Horse-chestnut,—and that these circumstances are inconsistent

* *Nouveaux Élémens de la Botanique*, 5me edit. p. 105.

with the supposition that the wood is organic matter engendered by leaves. To these statements there is nothing to object as mere facts, for they are true; but they certainly do not warrant the conclusions that have been drawn from them. One most important point is overlooked by those who employ these arguments, namely, that in all plants there are two distinct simultaneous systems of growth, the cellular and the fibro-vascular, of which the former is horizontal, and the latter vertical. The cellular gives origin to the pith, the medullary rays, and the principal part of the cortical integument; the fibro-vascular, to the wood and a portion of the bark; so that the axis of a plant may be not inaptly compared to a piece of linen, the cellular system being the woof, the fibro-vascular the warp. It has also been proved by Mr. Knight* and M. De Candolle† that buds are exclusively generated by the cellular system, while roots are evolved from the fibro-vascular system. Now if these facts are rightly considered, they will be found to offer an obvious explanation of the phænomena produced by those botanists who think that wood cannot be matter generated in an organic state by the leaves. The character of wood is chiefly owing to the colour, quantity, size, and distortions of the medullary rays, which belong to the horizontal system; it is for this reason that there is so distinct a line drawn between the wood of the graft and stock, for the horizontal systems of each are constantly pressing together with nearly equal force, and uniting as the trunk increases in diameter. As buds from which new branches elongate are generated by cellular tissue, they also belong to the horizontal system; and hence it is that the stock will always produce branches like itself, notwithstanding the long superposition of new wood which has been taking place in it from the scion.

The case of a ring of red bark always forming red wood beneath it, is precisely of the same nature. After the new bark has adhered to the mouths of the medullary rays of the stock, and so identified itself with the horizontal system, it is gradually pushed outwards by the descent of woody matter from above through it: but in giving way it is constantly generating red matter from its horizontal system, through which the wood descends, which thus acquires a colour that does not properly belong to it. With regard to the instances of grafts overgrowing their stock, or *vice versâ*, it is obvious that these are susceptible of explanation upon the same principle. If the horizontal system of both stock and scion has an equal power of

* *Philosophical Transactions*, 1805, p. 257.
† *Physiologie Végétale*, p. 158.

lateral extension, the diameter of each will remain the same; but if one grows more rapidly than the other, the diameters will necessarily be different: where the scion has a horizontal system that develops more rapidly than that of the stock, the latter will be the smaller, and *vice versâ*. It is, however, to be observed, that in these cases plants are altogether in a morbid state, and will not live for any considerable time.

Those who object to the theory of wood being generated by the action of leaves, either suppose—1st, that liber is developed by alburnum, and wood by liber; or, 2ndly, that "the woody and cortical layers originate laterally from the cambium furnished by preexisting layers, and nourished by the descending sap*." The first of these opinions appears to be that of M. Turpin†, as far as can be collected from a long memoir upon the grafting of plants and animals; but I must fairly confess that I am not sure I have rightly understood his meaning, so much are his facts mixed up with gratuitous hypothesis and obscure speculations upon the action of what he calls globuline. The second is the opinion commonly entertained in France, and adopted by M. De Candolle in his latest published work.

The objections to the views of M. Turpin need hardly be stated in a Report like this, where conciseness is so much an object. Those which especially bear upon the view taken by M. De Candolle are, that his theory is not applicable to all parts of the vegetable kingdom, but to exogenous plants only; that it is inconceivable how the highly organized parallel tubes of the wood, which can be traced anatomically from the leaves, and which are formed with great rapidity, can be a lateral deposit from the liber and alburnum; that they are manifestly formed long before it can be supposed that the leaves have commenced their office of elaborating the descending sap; and, finally, that endogenous and cryptogamic plants, in which there is no secretion of cambium, nevertheless have wood.

Such is the state of this subject at the time I am writing. To use the words of M. De Candolle, "The whole question may be reduced to this,—Either there descend from the top of a tree the rudiments of fibres, which are nourished and developed by the juices springing laterally from the body of wood and bark, or new layers are developed by preexisting layers, which are nourished by the descending juices formed in the leaves‡."

As this is one of the most curious points remaining to be settled among botanists, and as it is still as much open to dis-

* De Candolle, *Physiologie Végétale*, p. 165.

† See *Annales des Sciences*, vols. xxiv. and xxv., particularly vol. xxv. p. 43.

‡ De Candolle, *Physiologie Végétale*, p. 157.

cussion as ever, I have dwelt upon it at an unusual length, in the hope that some Member of the British Association may have leisure to prosecute the inquiry. Perhaps there is no mode of proceeding to elucidate it which would be more likely to lead to positive results, than a very careful anatomical examination of the progressive development of the Mangel Wurzel root, beginning with the dormant embryo, and concluding with the perfectly formed plant.

Arrangement of Leaves.—It has for a long time been thought that the various modes in which leaves, and the organs which are the result of them, are arranged upon a stem might be reduced to the spiral, and that all deviations from this law of arrangement are to be considered as caused by the breaking of spires into verticilli. In the Pine Apple, for instance, the Pine Cone, the Screw Pine, and many other plants, the spiral arrangement of the leaves is so obvious that it cannot be overlooked; in trees with alternate leaves this same order of arrangement may be discovered if a line is drawn from the base of one leaf to that of another, always following the same direction; even in verticillate plants we not unfrequently see that the whorls are dislocated by the præternatural elongation of their axis, and then become converted into a spire; and the same phænomenon is of common occurrence among the verticilli of leaves in the form of calyx, corolla, stamens, and carpella, which compose the flower. This will be the more distinctly apparent if we consider that, as M. Adolphe Brongniart has shown *, what we call whorls in a flower often are not so, strictly speaking, but only a series of parts placed in close approximation, and at different heights, upon the short branch that forms their axis.

Dr. Alexander Braun has endeavoured † to prove mathematically that the spiral arrangement of the parts of plants is not only universal, but subject to laws of a very precise nature. His memoir is of considerable length, and would be wholly unintelligible without the plates that illustrate it. It is therefore only possible on this occasion to mention the results. Setting out with a contemplation of the manner in which the scales of a Pine Cone are placed, to which a long and ingenious method of analysis was applied, he found that several different series of spires are discoverable, between which there invariably exist peculiar arithmetical relations, which are the expression of the various combinations of a certain number of elements disposed in a regular manner. All these spires depend upon the posi-

* *Annales des Sciences*, vol. xxiii. p. 226.

† *Vergleichende Untersuchung über die Ordnung der Schuppen an den Tannenzapfen.* 4to. 1830.

tion of a fundamental series, from which the others are deviations. The nature of the fundamental series is expressed by a fraction, of which the numerator indicates the whole number of turns required to complete one spire, and the denominator the number of scales or parts which constitute it: thus $\frac{8}{21}$ indicates that eight turns are made round the axis before any scale or part is exactly vertical to that which was first formed, and the number of scales or parts that intervene before this coincidence takes place is twenty-one.

It does not appear that this inquiry has as yet led to any practical application, although one might have expected that as the natural affinities of plants are determined, in a great degree, by the accordance that is observable in the relative position of their parts, the spires of which those parts are composed might have had something in common which would be susceptible of being expressed by numbers. If any practical application can be made of Dr. Braun's fractions, it seems likely to be confined to the distinction of species. His observations seem, however, to have established the truth of the doctrine that, beginning with the cotyledons, the whole of the appendages of the axis of plants,—leaves, calyx, corolla, stamens, and carpella,—form an uninterrupted spire, governed by laws which are almost constant.

Structure of Leaves.—The leaves of plants have been found by M. Adolphe Brongniart to be not merely expansions of the cellular integument of stems, traversed by veins originating in the woody system, but to be organs in which the internal parenchyma is arranged with beautiful uniformity, in the manner most conducive to the end of exposure to light and air, and of elaboration, for which the leaves are chiefly destined. In their usual structure leaves have been found by this observer either to consist of two principal layers,—of which the upper, into which the ascending sap is first introduced, is formed of compact cells, more or less perpendicular to the plane of the cuticle, and the under, into which the returning sap is propelled, is formed of very lax cavernous tissue, more or less parallel with the cuticle of the lower surface,—or else of two layers perpendicular to the cuticle, with a central parallel stratum.

The observations of Drs. Mohl and Meyen generally confirm this; but at the same time the latter instances several cases in which the texture of the leaf has been found to be nearly the same throughout.

Dutrochet* states, in addition, that the interior of the leaf

* *Annales des Sciences*, vol. xxv. p. 245.

is divided completely by a number of partitions caused by the ribs and principal veins, so that the air cavities have not actually a free communication in every direction through the parenchyma, but are to a certain extent cut off from each other. This is conformable to what M. Mirbel has described in *Marchantia,* who finds the leafy expansions of that plant separated by partitions into chambers, between which he is of opinion that there is no other communication than what results from the permeability of the tissue *.

The statement of M. Adolphe Brongniart, that all leaves intended to exist in the air are furnished with a distinct cuticle on their two faces, while those which are developed under water have no cuticle at all, has not been disproved, unless in the case of *Marchantia* †, whose under surface can scarcely be said to have a distinct cuticle; but this plant, which can only exist in humid shady places, is perhaps rather a proof of the accuracy of the theory of M. Brongniart than an exception to it.

That the stomata in all cases open into internal cavities in the leaf, where the tissue is extremely lax and cavernous, appears also extremely probable. It was especially found to be the case by M. Mirbel in his so often quoted remarks upon *Marchantia.*

With regard to the stomata themselves, no one appears yet to have confirmed the observation of Dr. Brown ‡, that their apparent orifice is closed up by a membrane. On the contrary, the observations of M. Mirbel on *Marchantia,* if they are to be taken as illustrative of the usual structure of those singular organs, go to establish the accuracy of the common opinion that the stomata are apertures in the cuticle. That most skilful physiologist, while watching the development of *Marchantia,* remarked the very birth of the stomata, which he describes as taking place thus:—The appearance of a little pit in the middle of four or five cells placed in a ring is a certain indication of the beginning of a stoma. The pit evidently increases by the enlargement and separation of the surrounding cells. If the nascent stoma consists of five cells, of which one is surrounded by four others, then the central one is destroyed; but if it consists of three or four cells adjusted so as to form a disk, then the stoma is caused by the separation of their sides in the centre, by which means a sort of star is created. It is true that

* "Recherches Anatomiques et Physiologiques sur le *Marchantia polymorpha,*" in *Nouveaux Annales du Muséum,* vol. i. p. 7.

† *Ibid.* p. 93.

‡ *Suppl. primum Prodromi Floræ Novæ Hollandiæ,* p. 3.

the stomata of *Marchantia* are in some respects different from what are found upon flowering plants; yet I think we can hardly doubt that the plan upon which they are all formed is essentially the same.

Dutrochet also confirms * the statement of Amici, that the stomata are perforations; for he finds that when leaves are deprived of their air by the air-pump, it is chiefly on the under side, where the greatest number of stomata is found, that little air bubbles make their appearance; and that it is through the stomata that water rushes into the cavernous parenchyma to supply the loss occasioned by the abstraction of air.

Anther, &c.—Some curious remarks upon the nature of the tissue that lines the cells of the anther have been published by Dr. John E. Purkinje, Professor of Medicine at Breslau. His researches are chiefly directed to the determination of the nature of the tissue that is in immediate contact with the pollen; and he has demonstrated in an elaborate Essay †, that the opinion emitted by Mirbel in 1808 ‡, that the cause of the dehiscence of the anther is its lining, consisting of cellular tissue cut into slits and eminently hygrometrical, is substantially true. He shows that this lining is composed of cellular tissue chiefly of the fibrous kind, which forms an infinite multitude of little springs, that when dry contract and pull back the valves of the anthers by a powerful accumulation of forces which are individually scarcely appreciable: so that the opening of the anther is not a mere act of chance, but the admirably contrived result of the maturity of the pollen,—an epoch at which the surrounding tissue is necessarily exhausted of its fluid by the force of endosmosis exercised by each particular grain of pollen.

That this exhaustion of the circumambient tissue by the endosmosis of the pollen is not a mere hypothesis, has been shown by Mirbel in a continuation of the beautiful memoir I have already so often referred to §. He finds that, on the one hand, a great abundance of fluid is directed into the utricles, in which the pollen is developed a little before the maturity of the latter, and that by a dislocation of those utricles the pollen loses all organic connexion with the lining of the anther; and that, on the other hand, these utricles are dried up, lacerated, and disorganized, at the time when the pollen has acquired its full development.

* *Annales des Sciences*, vol. xxv. p. 247.

† *De Cellulis Antherarum fibrosis.* 4to. Wratislaviæ, 1830.

‡ "Observations sur un Système d'Anatomie Comparée de Végétaux, fondés sur l'Organization de la Fleur," in *Mémoires de l'Institut*, 1808, p. 331.

§ "Complément des Observations sur le *Marchantia polymorpha*," in *Archives de Botanique*, vol. i.

The *Origin of the Pollen*, connected as it intimately is with the singular phænomena of vegetable sexuality, has naturally been of late an object of some inquiry. To the important discoveries of the younger Brongniart and of Dr. Robert Brown, M. Mirbel has added some observations*, detailed with that admirable clearness and precision which give so great a value to all his writings, and which are the more interesting as they serve to explain what was before obscure, and to correct what appears to have been either inaccurately or imperfectly decribed. This he has been enabled to do by beginning his inquiry at the very earliest period when the organization of the anther can be discovered: his subject was the common Gourd. At a very early time the whole tissue of the anther is of the most perfect uniformity, consisting of cellules, the transverse section of which represents nearly regular hexagons and pentagons. In every cell, without even excepting those which compose the superficies of the anther, are found little loose bodies, so exceedingly minute that a magnifying power of 500 or 600 diameters is scarcely sufficient to examine them: they may be compared to transparent, nearly colourless vesicles, more or less round, and of unequal size. At a stage but little more advanced, you may observe on each side of the medial line of a transverse section of a lobe of an anther, a collection of cellules rather larger than the remainder: it will afterwards be seen that it is here that the pollen is engendered; such cells are therefore called *pollen-cells*. In a bud, a line and a half or two lines in diameter, some remarkable alterations were found to have taken place; the pollen-cells had enlarged and their granules had so much increased in number, that they nearly filled the cells in opake masses. These granules and pollen-cells formed together a greyish mass, connected with the rest of the tissue by the intervention of a cellular membrane, which, notwithstanding its organic continuity with the surrounding parts, is at once distinguishable; for while the cells of the surrounding parts elongate parallel to the plane of the surface, and to the plane of the base of the anther, those of the cellular membrane elongate from the centre to the circumference. In more advanced anthers, the sides of the pollen-cells, from being thin and dry, had changed to a perceptible thickness, and their substance, gorged with fluid, resembled a colourless jelly. When the buds were three or four lines long, an unexpected phænomenon presented itself. At first the thick and succulent walls of each pollen-cell dilated so as to leave an empty space between the inner face and the granules, not one of which separated

* "Complément des Observations," *&c.*, as above quoted.

from the general mass, which showed that some power kept them united. Shortly after, four appendages, like knife-blades, developed at equal distances upon the inner face of the cell, and gradually projected their edges towards the centre, till at last they divided the granular mass into four little triangular bodies; when the appendages had completely united at their edges, they divided the cavity of the pollen-cells into four distinct boxes, which then began to rounden, and finally became little spherical masses. Each of these was the rudiment of a grain of pollen, subsequently acquired a membranous integument, hardened, became yellow, and thus arrived at maturity.

What is perhaps most important in these observations is the demonstration of the original organic continuity of all the parts of the anther, against the statement of M. Adolphe Brongniart, and also against what appears to be the opinion of Dr. Brown, as far as can be collected from the manner in which he speaks of the evolution of the pollen in *Tradescantia virginica**.

Although it is not directly shown by these observations whether the perfect grain of pollen has one or two integuments,—a question that may still be said to be unsettled,—it nevertheless appears from other instances that M. Mirbel admits the existence of an outer not distensible coat, and of an inner highly extensible lining. A curious paper upon this point† has been published by a Saxon botanist named Fritzsche. By means of a mixture of two parts by weight of concentrated sulphuric acid, and five parts of water, he found that the grains of pollen can be rendered so transparent as to reveal their internal structure, and that the whole process of the emission of the pollen-tubes can be distinctly traced. He describes the universal presence of two coatings to the grains of pollen; and he also finds that the pollen contains a quantity of oily particles in addition to the moving corpuscles,—a fact which has also been noticed by Dr. Brown.

Although the generalizations in this work are less satisfactory than could be desired, it must nevertheless be considered a most valuable collection of facts, and as containing the best arrangement that has as yet appeared of the various forms under which the pollen is seen.

Fertilization.—The road which some years since was so happily opened by Amici to the discovery of the exact manner in which vegetable fertilization takes effect, is every day becoming more and more direct. The doubts of those who could not discern the tubes that are projected into the style by the

* *Observations upon* Orchideæ *and* Asclepiadeæ, p. 21.
† *Beiträge zur Kenntniss des Pollen.* 4to. Berlin, 1833.

pollen, have been removed; the important demonstration by Dr. Brown of the universal presence of a passage through the integuments of the ovulum at the point of the nucleus has been extended and confirmed by M. Mirbel in a paper of the highest interest*; the fact that it is at the point of the nucleus (where this passage exists,) that the nascent embryo makes it appearance, is now undisputed; the passage of the contents of the pollen down the pollen-tubes, and the curious discovery of a power of motion in the granules that are thus emitted, are also recognised: it now only remains to be proved that the pollen-tubes come in contact with the nucleus, and the whole secret of fertilization is revealed. A few remarkable contributions to this part of the subject have lately been made.

Some plants have the passage or foramen in their ovulum so remote from any part through which the pollen-tubes can be supposed to convey their influence, as to have thrown considerable difficulty in the way of the supposition that actual contact between the point of the nucleus and the fertilizing tissue is indispensable.

The manner in which, notwithstanding the apparent difficulty of such contact taking place, this happens in *Statice Armeria*, was long since made out by Dr. Brown, in whose possession I several years ago saw drawings illustrating this phænomenon; it has since been explained by M. Mirbel. Another case, presenting similar apparent difficulties, occurs in *Helianthemum*. In plants of that genus the foramen is at that end of the ovulum which is most remote from the hilum; and although the ovula themselves are elevated upon cords much longer than are usually met with, yet there are no obvious means of their coming in contact with any part through which the matter projected into the pollen-tubes can be supposed to descend. It has, however, been ascertained by M. Adolphe Brongniart†, that at the time when the stigma is covered with pollen, and fertilization has taken effect, there is a bundle of threads, originating from the base of the style, which hang down in the cavity of the ovarium, and, floating there, are abundantly sufficient to convey the influence of the pollen to the points of the nuclei. So again in *Asclepiadeæ*. In this tribe, from the peculiar conformation of the parts, and from the grains of pollen being all shut up in a sort of bag, out of which there seemed to be no escape, it was supposed that this tribe must at least form an exception to the general rule. But before the month of November 1828‡, the

* *Nouvelles Recherches sur la Structure de l'Ovule Végétal et sur ses Développements.* Also *Additions aux 'Nouvelles Recherches,' &c.*

† *Annales des Sciences,* vol. xxiv. p. 123. ‡ *Linnæa,* vol. iv. p. 94.

celebrated Prussian traveller and botanist Ehrenberg had discovered that the grains of pollen of *Asclepiadeæ* acquire a sort of tails which are all directed to a suture of their sac on the side next the stigma, and which at the period of fertilization are lengthened and emitted; but he did not discover that these tails are only formed subsequently to the commencement of a new vital action connected with fertilization, and he thought that they were of a different nature from the pollen-tubes of other plants; he particularly observed in *Asclepias syriaca* that the tails become exceedingly long and hang down.

In 1831 the subject was resumed by Dr. Brown* in this country, and by M. Adolphe Brongniart† in France, at times so nearly identical, that it really seems to me impossible to say with which the discovery about to be mentioned originated: it will therefore be only justice if the Essays referred to are spoken of collectively instead of separately. These two distinguished botanists ascertained that the production of tails by the grains of pollen was a phænomenon connected with the action of fertilization; they confirmed the existence of the suture described by Ehrenberg; they found that the true stigma of *Asclepiadeæ* is at the lower part of the discoid head of the style, and so placed as to be within reach of the suture through which the pollen-tubes or tails are emitted; they remarked that the latter insinuate themselves below the head of the style, and follow its surface until they reached the stigma, into the tissue of which they buried themselves so perceptibly that they were enabled to trace them, occasionally, almost into the cavity of the ovarium; and thus they established the highly important fact, that this family, which was thought to be one of those in which it was impossible to suppose that fertilization takes place by actual contact between the pollen and the stigma, offers the most beautiful of all examples of the exactness of the theory, that it is at least owing to the projection of pollen-tubes into the substance of the stigma. In the more essential parts these two observers are agreed: they, however, differ in some of the details; as, for instance, in the texture of the part of the style which I have here called stigma, and into which the pollen-tubes are introduced. M. Brongniart both describes and figures it as much more lax than the contiguous tissue, while on the other hand Dr. Brown declares that he has in no case been able to observe "the slightest appearance of secretion, or any dif-

* *Observations on the Organs and Mode of Fecundation of* Orchideæ *and* Asclepiadeæ. London, October 1831.

† *Annales des Sciences* for October and November 1831; from observations made in July, August and September of that year.

ferences whatever in texture between that part and the general surface of the stigma" (meaning what I have described as the discoid head of the style): but this is not the place for entering into the discussion of these subordinate points.

Orchideæ are another tribe in which similar difficulties have been found in reconciling structure with the necessity of contact between the pollen and stigma in order to effect impregnation. Indeed it seems in these plants as if every possible precaution had been taken by nature to prevent such contact. Nevertheless it is represented by M. Adolphe Brongniart, in a paper read before the Academy of Sciences of Paris in July 1831 *, that contact is as necessary in these plants as in others, and that in the emission of pollen-tubes they do not differ from other plants. These statements have been followed up by Dr. Brown †, in an elaborate Essay upon the subject, in which the results that are arrived at by our learned countryman are essentially to the same effect. To these there is at present nothing equally positive to oppose; but as the indirect observations of Mr. Bauer ‡, and the general structure of the order, are very much at variance with the probability of actual contact being necessary, and especially as Dr. Brown is obliged to have recourse to the supposition that the pollen of many of these plants must be actually carried by insects from the boxes in which it is naturally locked up,—it must be considered, I think, that the mode of fertilization in *Orchideæ* is still far from being determined. I must particularly remark that the very problematical agency of insects, to which Dr. Brown has recourse in order to make out his case, seems to be singularly at variance with his supposition § that the insect forms, which in *Ophrys* are so striking, and which he finds resemble the insects of the countries in which the plants are found, are intended rather *to repel* than to attract. It may be true, as Dr. Brown observes, that there is less necessity for the agency of insects in such flowers as the European *Ophrydeæ*; but what other means than the assistance of insects can be supposed to extricate the pollen from the cells in the insect flowers of such plants as *Renanthera Arachnites*, the whole genus *Oncidium*, *Tetramicra rigida*, several species of *Epidendrum*, *Cymbidium tenuifolium*, *Vanda peduncularis*, and a host of others?

* *Annales des Sciences*, vol. xxiv. p. 113.

† *Observations upon the Organs and Mode of Fecundation of* Orchideæ *and* Asclepiadeæ.

‡ *Illustrations of the Genera and Species of Orchideous Plants.* Part II. "Fructification," tabb. 5. 12. 13. 14.

§ "Proceedings of the Linnean Society," June 5, 1832, as given in the *London and Edinburgh Philosophical Magazine and Journal.*

Origin of Organs.—There is no part of vegetable physiology so obscure as that which relates to the origin of organs. We find a degree of simplicity that is perfectly astonishing in the fundamental structure of the whole vegetable kingdom; we are able to prove by rigorous demonstration that every one of the appendages of the axis is a modification of a leaf, to which there is a constant tendency to revert; we see that in some cases a part which usually performs one function assumes another, as in the *Alströmerias,* whose leaves by a twist of their petiole turn their under surface upwards: but we are entirely ignorant of the causes to which these changes are owing. An important step in elucidating the subject has been lately taken by M. Mirbel, in his memoir upon the structure of *Marchantia polymorpha.* The young bulbs by which this plant is multiplied are originally so homogeneous in structure, that there is no apparent character in their organization to show which of their faces is destined to become the upper surface, and which the under. For the purpose of ascertaining whether there existed any natural but invisible predisposition in the two faces to undergo the changes which subsequently become so apparent, and by means of which their respective functions are performed, or whether the tendency is given by some cause posterior to their first creation, the following experiments were instituted. Five bulbs were sown upon powdered sandstone, and it was found that the face which touched the sandstone produced roots, and the opposite face formed stomata. It was, however, possible that the five bulbs might have all accidentally fallen upon the face which was predisposed to emit roots; other experiments of the same kind were therefore tried, first with eighty and afterwards with hundreds of little bulbs,—and the result was the same as with the five. This proved that either face was originally adapted for producing either roots or stomata, and that the tendency was determined merely by the position in which the surfaces were placed. The next point to ascertain was, whether the tendency once given could be afterwards altered; some little bulbs, that had been growing for twenty-four hours only, had emitted roots; they were turned, so that the upper surface touched the soil, and the under was exposed to light. In twenty-four hours more the two faces had both produced roots; that which had originally been the under surface went on pushing out new roots; that which had originally been the upper surface had also produced roots: but in a few days the sides of the young plants began to rise from the soil, became erect, turned over, and finally recovered in this way their original position, and the face which had originally been the uppermost, immediately became covered with stomata. It, therefore, appears that the impulse once given, the predisposition to assume particular appearances or functions is absolutely fixed, and will not change in the ordinary course of nature. This is a fact of very high interest for those who are occupied in researches into the causes of what is called vegetable metamorphosis, an expression which has been justly criticised as giving a false idea of the subject to which it relates.

Morphology.—When those who first seized upon the important but neglected facts out of which the modern theory of morphology has been constructed, asserted that all the appendages of the axis of a plant are metamorphosed leaves, more was certainly stated than the evidence would justify; for we cannot say that an organ is a metamorphosed leaf, which in point of fact has never been a leaf. What was meant, and that which is supported by the most conclusive evidence, is, that every appendage of the axis, whether leaf, bractea, sepal, petal, stamen, or pistillum, is originally constructed of the same elements, arranged upon a common plan, and varying in their manner of development, not on account of any original difference in structure, but on account of special and local predisposing causes: of this the leaf is taken as the type, because it is the organ which is most usually the result of the development of those elements,—is that to which the other organs generally revert, when from any accidental disturbing cause they do not assume the appearance to which they were originally predisposed,—and, moreover, is that in which we have the most complete state of organization.

This is not a place for the discussion of the details upon which the theory of morphology is founded; it is sufficient to state that it has become the basis of all philosophical views of structure, and an inseparable part of the science of botany. Its practical importance will be elucidated by the following circumstance. Fourteen or fifteen years ago I was led to take a view of the structure of *Reseda* very different from that usually assigned to the genus; and when a few years afterwards that view was published, it attracted a good deal of attention, and gained some converts among the botanists of Germany and France. It was afterwards objected to by Dr. Brown upon several grounds; but I am not aware that they were considered sufficiently valid to produce any change in the opinions of those who had adopted my hypothesis. Lately, however, Professor

Henslow has satisfactorily proved*, in part by the aid of a monstrosity in the common *Mignonette,* and in part by a severe application of morphological rules, that my hypothesis must necessarily be false ; and I am glad to have this opportunity of expressing my full concurrence in his opinion.

It has long been known that the ligulate and tubular corollas of *Compositæ* are anatomically almost identical, and that their difference consists only in the five petals of the tubular corolla all separating regularly for a short distance from their apex, while the five petals of the ligulate corolla adhere up to their very points, except on the side next the axis of inflorescence, where two of them are altogether distinct except at their base. M. Leopold von Buch explains this circumstance in the following manner. He states that these ligulate corollas when unexpanded bear at their point a little, white, and very viscid body or gland, which is a peculiar secretion that dries up when it comes in contact with the atmosphere. The adhesion of this gland is too powerful to be overcome by the force of the style and stamens pressing against it from within. The corollas, which are gradually curved outwards by the growth of those in the centre of the inflorescence, at the same time bend down the style, which consequently presses up against the line of union of the two petals nearest the axis : although the style cannot overcome the adhesion of the viscid gland at the point of the corolla, it is able in time to destroy the union of the two interior petals, which finally give way and allow the stamens and style to escape. As soon as this takes place, the corolla can no longer remain erect, but falls back towards the circumference of the capitulum, and thus contributes to the radiating character of this sort of inflorescence. When the viscid body is either not at all, or very imperfectly produced at the point of the corolla, as sometimes happens in the genus *Hieracium,* especially *H. bifurcum,* tubular corollas are produced instead of ligulate ones.

The ovulum is the organ where the greatest difficulty has occurred in reducing the structure to anything analogous to that of other parts. It is true that Du Petit Thouars regarded it as analogous to a leaf bud ; but his view appears to have been purely hypothetical, for I am not aware that he had any distinct evidence of the fact. Some years ago M. Turpin, in showing the great similarity that exists between the convolute bracteæ of certain *Marcgraviaceæ* and the exterior envelope of the ovulum, took the first step towards proving that the hypothesis

* *Transactions of the Philosophical Society of Cambridge,* vol. v. Part I.

of Du Petit Thouars was susceptible of demonstration; it was more distinctly shown by the interesting discovery of Professor Henslow, that the leaves of *Malaxis paludosa* had on their margins what no doubt must be considered buds, but what in structure are an intermediate state between buds and ovula; and it has been recently asserted by Engelmann*, still, however, without the production of any proof, that "ovula are buds of a higher kind, their integuments leaves, and their funiculus the axis, all which, in cases of retrograde metamorphosis, are in fact converted into stem and green leaves." The nearest approach to a demonstration that has yet been afforded of ovula being buds is in a valuable paper by Professor Henslow, just printed in the *Transactions of the Philosophical Society of Cambridge*†, in which it is shown that in the *Mignonette* the ovula are in fact transformed occasionally into leaves, either solitary or rolled together round an axis, of which the nucleus is the termination.

M. Dumortier has endeavoured to prove‡ that the embryo itself is essentially the same as a single internodium of the stem with its vital point or rudimentary bud attached to it. Although the author's demonstration is a failure, and his paper a series of confused and illogical reasoning, yet there can be little doubt that the hypothesis itself is a close approximation to the truth.

Dr. George Engelmann has recently attempted§ to classify the aberrations from normal structure, which throw so much light upon the real origin and nature of the organs of plants. He has collected a very considerable number of cases under the following heads. 1. Retrograde metamorphosis (*Regressus*), when organs assume the state of some of those on the outside of them, as when carpella change to stamens or petals, hypogynous scales to stamens, stamens to petals or sepals, sepals to ordinary leaves, irregular structure to regular, and the like. 2. Foliaceous metamorphosis (*Virescentia*), when all the parts of a flower assume more or less completely the state of leaves. 3. Disunion (*Disjunctio*), when the parts that usually cohere are separated, as the carpella of a syncarpous pistillum, the filaments of monadelphous stamens, the petals of a monopetalous corolla, &c. 4. Dislocation (*Apostasis*); in this case the whorls of the flower are broken up by the extension of the axis. 5. Viviparousness (*Diaphysis*), when the axis is not only elongated, but continues to grow and form new parts, as in those

* *De Antholysi Prodromus*, p. 61. † vol. v. Part I.
‡ *Nova Acta Academiæ Naturæ Curiosorum*, vol. xvi. p. 245.
§ *De Antholysi Prodromus*.

instances where one flower grows from within another. And finally, 6. Proliferousness (*Ecblastesis*), when buds are developed in the axillæ of the floral organs, so as to convert a simple flower into a mass of inflorescence. A very considerable number of instances are adduced in illustration of these divisions, and the work will be found highly useful as a collection of curious or important facts.

The doctrines of morphology, and the evidence in support of them, may now be considered so far settled as to require but little further illustration for the present. This is, however, only true of flowering plants: in the whole division of flowerless plants there has been scarcely any attempt to discover the analogy of organs, and to reduce their structure to a corresponding state of identification. I some time since* endeavoured to excite attention to this subject, by hazarding some speculations which had at least the merit of novelty to recommend them; but I cannot discover that any one has since turned his attention to the inquiry, although it must be confessed that the comparative anatomy of flowerless plants is among the most interesting topics still remaining for discussion, and that it is rather discreditable to Cryptogamic botanists that the elucidation of so very curious a matter should be postponed to the comparatively unimportant business of distinguishing or dividing genera and species.

Gradual Development.—The theory of the gradual development of the highest class of organic bodies, in consequence of a combination and complication of the phænomena attendant upon the development of the lowest classes, has acquired so great a degree of probability among animals, that it has become a question of no small interest whether traces of the same, or a similar law, cannot be found among plants. In an inquiry of such a nature, it seems obvious that attention should in the first instance be directed to a search after positive and incontestable facts, and that mere hypotheses should in the beginning be totally rejected. The only circumstances that occur to me as bearing directly upon this point are the following. It has been ascertained by M. Mirbel, in his memoir on the *Marchantia*, that the sporule of that very simple plant is a single vesicle, which, when it begins to grow, produces other vesicles on its surface, which go on propagating in the same manner, every new vesicle engendering others; and that different modifications of this process produce the different parts that the perfect plant finally develops.

* *Outlines of the First Principles of Botany*, p. 533, &c. *Introduction to the Natural System of Botany*, p. 313, &c.

The same principle of growth appears to obtain in *Confervæ*, and probably is found in other vegetables of the lowest grade.

This is analogous to what takes place in the formation of the embryo of *Vasculares*. In the opinion of Dr. Brown and of Mirbel, the first rudiment of a plant far more complicated than *Marchantia*, consists also of a vesicle, but suspended by a thread to the summit of the cavity of the ovulum; and the difference between the one case and the other is, that while in the *Marchantia* the original vesicle, "as soon as it is formed, possesses all the conditions requisite for developing a complete plant on the surface of the soil; on the other hand, that of flowering plants must, on pain of death, commence its development in the interior of the ovulum, and cannot continue it further until it has produced the rudiments of root, stem, and cotyledons*.

Beyond this I do not think that any attempt has been made to elucidate the question.

Irritability.—Dr. Dutrochet has published† the result of some experiments with the air-pump upon the pneumatic system of plants. Independently of confirming the fact, already generally known, of plants having the means of containing a large quantity of air, he arrived at the unexpected result, that the sleep of plants and their irritability are certainly dependent upon the presence of air within them. A sensitive plant, left in the vacuum of an air-pump for eighteen hours, indicated no sign whatever of the accustomed collapse of its leaflets on the approach of night, nor when it was restored to the air could it be stimulated by the smartest shocks; but in time it recovered its irritability. When flowers that usually close at night were placed in a vacuum while expanded, they would not close; and when flowers already closed were placed in the same situation, they would not unfold at the return of morning; whence Dr. Dutrochet infers that the internal air of plants is indispensably necessary to the exercise of their alternate motions of sleeping and waking, and in general to the existence of the faculty they possess of indicating by their movements the influence of external exciting causes.

Action of Coloured Light.—Professor Morren, of Ghent, has mentioned‡ the result of some experiments upon the action of the coloured rays upon germination; and he has found that while those rays in which the illuminating power is the most feeble were, as might have been expected, the most favourable to germination, their power of decomposing carbonic acid, and

* *Archives de Botanique*, vol. i. † *Annales des Sciences*, vol. xxv. p. 243.
‡ *Annales des Sciences*, vol. xxvii. p. 201.

producing a green deposit in the parenchyma, is in proportion to their illuminating property; that no decomposed rays effect this so rapidly as white light; and that the yellow ray possesses the greening power in the highest degree, the orange in a very slight degree, and violet, red and purple not at all.

Colours.—Nothing can be named in the whole range of botany upon which information is so much wanted as the cause of the various colours of plants. It was, indeed, long since suspected by Lamarck that the autumnal colouring of leaves and fruits was a morbid condition of those parts; and it has subsequently been ascertained that all colours are owing to the presence of a substance, called *chromule* by De Candolle, which fills the parenchyma, assuming different tints. Green has also been clearly made out to be connected with exposure to light, and has been considered to be in all probability owing to the deposition of the carbon left upon the decomposition of carbonic acid. Some botanists have also observed the connexion of red colour with acidity; but still we had scarcely any positive knowledge of the cause of the production of any colour except green, till M. Macaire of Geneva* remarked, that just before leaves begin to change colour in the autumn, they cease parting with oxygen in the day, although they go on absorbing it at night; whence he concluded that their chromule is oxygenated, by which a yellow colour is first caused, and then a red,—for he found that in all cases a change to red is preceded by a change to yellow. He also ascertained that the chromule of the red bracteæ and calyx of *Salvia splendens* is chemically the same as that of autumnal leaves. Coupling this with the fact that petals do not part with oxygen, it would seem as if their colour, if yellow or red, may also be owing to a kind of oxygenation. But according to M. Theodore de Saussure†, coloured fruits part with their oxygen; so that, if this be true, red and yellow cannot always be ascribed to such a cause. M. De Candolle‡ has some excellent observations upon this subject in his recent admirable digest of the laws of vegetable physiology; in which he concludes, from the inquiries hitherto instituted, that all colours depend upon the degree of oxygenation. When oxygen is in excess, the colour seems to tend to yellow or red; and when it is deficient, or when the chromule is more carbonized, which is the same thing, it has a tendency to blue. Local additions of alkaline matters are also called in aid of an explanation of the various shades of colour that flowers and fruits present.

* *Mémoires de la Société Physique de Génève*, vol. iv. p. 50.
† *Ibid.* vol. i. p. 284. ‡ *Physiologie Végétale*, p. 906.

Dr. Dutrochet is of opinion* that the whitish spots we sometimes see in leaves, and the paler tint that generally characterizes the under side of the same organs, are owing to the presence of air beneath the cuticle. He finds that the arrow-head shaped blotch on the upper side of the leaf of *Trifolium pratense*, and the whitish spots on *Pulmonaria officinalis*, disappear when the leaves are plunged in water beneath the exhausted receiver of the air-pump, and that the lower surface of leaves acquires the same depth of colour as the upper under similar circumstances. This he ascribes to the air naturally found in the leaves being abstracted, and its place supplied with water; a conclusion which agrees with what might be inferred from the anatomical structure of the parts in question.

Excretions.—It has long been known that some plants are incapable of growing, or at least of remaining in a healthy state, in soil in which the same species has previously been cultivated. For instance, a new apple orchard cannot be made to succeed on the site of an old apple orchard, unless some years intervene between the destruction of the one and the planting of the other: in gardens, no quantity of manure will enable one kind of fruit-tree to flourish on a spot from which another tree of the same species has been recently removed; and all farmers practically evince, by the rotation of their crops, their experience of the existence of this law.

Exhaustion of the soil is evidently not the cause of this, for abundant manuring will not supersede the necessity of the usual rotation. The celebrated Duhamel long ago remarked, that the Elm parts by its roots with an unctuous dark-coloured substance; and, according to De Candolle, both Humboldt and Plenck suspected that some poisonous matter is secreted by roots; but it is to M. Macaire, who at the instance of the first of these three botanists undertook to inquire experimentally into the subject, that we owe the discovery of the suspicion above alluded to being well founded. He ascertained† that all plants part with a kind of fæcal matter by their roots, that the nature of such excretions varies with species or large natural orders: in *Cichoraceæ* and *Papaveraceæ* he found that the matter was analogous to opium, and in *Leguminosæ* to gum; in *Gramineæ* it consists of alkaline and earthy alkalies and carbonates, and in *Euphorbiaceæ* of an acrid gum-resinous substance. These excretions are evidently thrown off by the roots on account of their presence in the system being deleterious; and it was found by experiment, that plants artificially poisoned parted with the

* *Annales des Sciences*, vol. xxv. p. 246.

† De Candolle, *Physiologie Végétale*, p. 249.

poisonous matter by their roots. For instance, a plant of *Mercurialis* had its roots divided into two parcels, of which one was immersed in the neck of a bottle filled with a weak solution of acetate of lead, and the other parcel was plunged into the neck of a corresponding bottle filled with pure water. In a few days the pure water had become sensibly impregnated with acetate of lead. This, coupled with the well known fact that plants, although they generate poisonous secretions, yet cannot absorb them by their roots without death, as, for instance, is the case with *Atropa Belladonna,* seems to prove that the necessity of the rotation of crops is more dependent upon the soil being poisoned than upon its being exhausted.

This is a part of vegetable physiology of vast importance to an agricultural country like England, and may possibly cause a total revolution in our system of husbandry.

All that M. Macaire can be said as yet to have done, is to have discovered the fact and to have pointed out certain strong examples of it; but if the discovery is to be converted to a practically useful purpose, we require positive information upon the following points:—

1. The nature of the fæcal excretions of every plant cultivated by the farmer.

2. The nature of the same excretions of the common weeds of agriculture.

3. The degree in which such excretions are poisonous to the plants that yield them, or to others.

4. The most ready means of decomposing those excretions by manures or other means.

It would be superfluous to point out what the application would be of such information as this; but I cannot forbear expressing a hope that a question upon which so many deep interests are involved may be among the first to occupy the attention of the chemists of the British Association.

Report on the Physiology of the Nervous System. By William Charles Henry, M.D., *Physician to the Manchester Royal Infirmary.*

Introduction.—The science of Physiology has for its object to ascertain, to analyse, and to classify the qualities and actions which are peculiar to living bodies. These vital properties reside exclusively in organized matter, which is characterized by a molecular arrangement, not producible by ordinary physical attractions and laws. Matter thus organized consists essentially of *solids*, so disposed into an irregular network of laminæ and filaments, as to leave spaces occupied by *fluids* of various natures. 'Texture' or 'tissue' is the anatomical term by which such assemblages are distinguished. Of these the cellular, or *tela cellulosa*, is most elementary, being the sole constituent of several, and a partial component of all tissues and systems. Thus the membranes and vessels consist entirely of condensed cellular substance; and even muscle and nerve are resolvable, by microscopic analysis, into globules deposited in attenuated cellular element.

But though the phenomena, which are designated as vital, are never found apart from organization, and have even by some naturalists been regarded as identical with it, yet in the order of succession vital actions seem necessarily to stand to organized structures in the relation of antecedents; for the production of even the most rudimentary forms and textures implies the previous operation of combining tendencies or 'vital affinities'. The origin and early development of these vital tendencies, and of organized structures, are beyond the pale of exact or even of approximative knowledge. But it is matter of certainty, that life is the product only of life; that every new plant or animal proceeds from some pre-existent being of the same form and character; and thus that the image of the great Epicurean poet, "Quasi cursores vitai lampada tradunt," possesses a compass and force of illustration which, as a supporter of the doctrine of fortuitous production, he could not have himself contemplated.

The popular notions respecting life are obscure and indeterminate; nor are the opinions even of philosophers characterized by much greater distinctness or mutual accordance. Like other complex terms, 'life' can obviously be defined only by an enumeration of the phenomena which it associates. This enumeration will comprehend a greater or a smaller number of particulars, according to the station in the scale of living beings which is occupied by the object of survey. In its simplest manifestation, the principle of life may be resolved into the functions of nutrition, secretion and absorption. It consists, according to Cuvier, of the faculty possessed by certain combinations of matter, of existing for a certain time and under a determinate form, by attracting unceasingly into their composition a part of surrounding substances, and by restoring portions of their own substance to the elements. This definition comprehends all the essential phenomena of vegetable life. Nutritive matter is drawn from the soil by the spreading fibres of the root, through the instrumentality of spongioles or minute turgid bodies at their extremities, which act, according to Dutrochet, by a power which he has called 'endosmosis.' The same agency raises the nutrient fluid through the lymphatic tubes to the leaves, where it seems to undergo a kind of respiratory process, and becomes fit for assimilation. These changes, and the subsequent propulsion of the sap to the different parts and textures, plainly indicate independent fibrillary movements, which are represented in animal life by what Bichat has termed 'the phenomena of organic contractility'. The power residing in each part of detecting in the circulating fluid, and of appropriating, matters fitted to renovate its specific structure, is designated in the same system by the term 'organic sensibility'.

Ascending from the vegetable to the animal kingdom, the term 'life' advances greatly in comprehensiveness. The existence of a plant is limited to that portion of space in which accident or design has inserted its germ; while animals are for the most part gifted with the faculties of changing their place, and of receiving from the external world various impressions. Along with the general nutritive functions, the higher attributes of locomotion and sensation are therefore comprised in the extended compass of meaning which the term 'life' acquires with the prefix 'animal'. The nutritive functions, too, emerging from their original simplicity, are accomplished by a more complex mechanism, and by agencies further removed from those which govern the inanimate world.

Locomotion is effected either by means of a contractile tissue, or of distinct muscular fibres. These fibres have been said to consist of globules resembling, and equal in magnitude to, those of the blood, disposed in lines, in the elementary cellulosity, which by an extension of the analogy is compared to serum. But the latest microscopical observations of Dr. Hodgkin are opposed to this globular constitution of the contractile fibre. "Innumerable very minute but clear and fine parallel lines or striæ may be distinctly perceived, transversely marking the fibrillæ." Irritability, or the faculty of contracting on the application of a stimulant, is a property inherent in the living fibre. It is an essential element of all vital operations, except of those which have their seat in the nervous system, such as sensation, volition, the intellectual states, and moral affections. All the phenomena of life, in the higher animals, may then be ultimately resolved into the single or combined action of these two elementary properties,—irritability and nervous influence, each residing in its appropriate texture and system.

These preliminary remarks are designed to unfold the principles to be followed in classifying the vital functions. In general or comparative physiology, a strictly scientific arrangement would contemplate first the phenomena of the most elementary life, and would successively trace the more perfect development of those simple actions and their gradual transition into more complex processes, as well as the new functions, superadded in the ascending scale of endowment. But such a mode of classification is wholly inapplicable to the particular physiology of man and of the more perfect animals, viewed by itself and without reference to inferior orders of beings; for the nutritive functions of this class, which correspond with the elementary actions of the simplest vegetable life, are effected by a complex system of vessels and surfaces, deriving their vital powers from contractile fibres, and controlled, if not wholly governed, by nervous influence. It is then manifest, that in the higher physiology the general laws of contractility and 'innervation' must precede the description of the several functions, which all depend on their single or united agency. The particular functions will afterwards be classed, as they stand in more immediate relation to one or other of the two essential principles of life.

In the present state of physiological knowledge, it is impossible to determine absolutely, and without an opening to controversy, whether the functions of muscle or those of nerve are entitled to precedency. If each were equally independent of the other in the performance of their several offices, the question of priority would resolve itself into one of simple convenience. The actions of the nervous system, if contemplated for the short interval of time during which they are capable of persisting without renovation of tissue, are entirely independent of the contractile fibre. But it is certain that the cooperation of nerve is required in most, if not in all, the actions of the muscular system. Thus the voluntary muscles in all their natural and sympathetic contractions receive the stimulant impulse of volition through the medium of nerve; and though the mode, in which the motive impression is communicated to the involuntary muscles, is still matter of controversy, there seems sufficient evidence* to sanction the conclusion that nerve is in this case also the channel of transmission;—"that the immediate antecedent of the contraction of the muscular fibre is universally a change in the ultimate nervous filament distributed to that fibre." If this be correct, the physiological history of muscle cannot be rendered complete without reference to that of nerve.

In the higher manifestations of life, nervous matter is invested with the most eminently vital attributes. It is the exclusive seat of the various modes of sensation, and of all the intellectual operations; or, rather, it is the point of transition, where the physical conditions of the organs, which are induced by external objects, pass into states of mind, becoming perceptions; and where the mental act of volition first impresses a change on living matter. These two offices of conducting motive impressions from the central seat of the will to the muscles, and of propagating sensations from the surface of the body and the external organs of sense to the sensorium commune, have been of late years shown to reside in distinct portions of nervous substance.

The honour of this discovery, doubtless the most important accession to physiological knowledge since the time of Harvey, belongs exclusively to Sir Charles Bell. It constitutes, moreover, only a part of the new truths, which his researches have unveiled, regarding the general laws of nervous action, and the offices of individual nerves. His successive experiments on function, guided always by strong anatomical analogies in structure, in origin, or in distribution, have led to the entire remodelling of nervous physiology, and to the formation of a system of arrangement, based on essential affinities and on parity of intimate composition, instead of on *apparent* sequence or proximity of origin. Among the continental anatomists, MM. Magendie and Flourens have contributed most largely to our knowledge of this part of physiology; the former by repeating and confirming the experiments of Bell, as well as by various original inquiries; the latter by his important researches into the vital offices of the brain and its appendages. Much light,

* See "A Critical and Experimental Enquiry into the Relations subsisting between Nerve and Muscle," in the 37th vol. of the *Edinburgh Medical and Surgical Journal.*

too, has been thrown on the functions of several of the encephalic nerves, and especially of those supplying the face and its connected cavities, by Mr. Herbert Mayo, who has analysed their anatomical composition, and pursued their course with singular precision, and has thus been enabled to correct some errors of detail in the system of Sir Charles Bell.

Nervous System.—In man, and in other vertebrated animals, the nervous system consists of the cerebrum, cerebellum, medulla oblongata, medulla spinalis, and of the encephalic, spinal, and ganglionic nerves. It seems most natural to observe this order of anatomical sequence in recording what is known of nervous functions.

Cerebrum, or Brain-proper.—The physiology of the brain has received of late years very considerable accessions, and its vital offices, *viewed as an entire organ*, have now probably been ascertained with sufficient precision. Some portion of this newly acquired knowledge has been gathered from experiments on living animals, but the greater and more valuable part has flowed from the study of comparative development. In this latter field of inquiry, Tiedemann's elaborate history of the progressive evolution of the human brain during the period of fœtal existence, with reference to the comparative structure of that organ in the lower animals, merits an early and detailed notice. It had been discovered by Harvey, that the fœtus in the human species, as well as in inferior animals, is not a precise facsimile of the adult, but that it commences from a form infinitely more simple, and passes through several successive stages of organization before reaching its perfect development. In the circulatory system, these changes have been minutely observed and faithfully recorded *. Tiedemann has traced a similar progression in the brain and nervous system, and has moreover established an exact parallel between the *temporary* states of the fœtal brain in the periods of advancing gestation, and the *permanent* development of that organ at successive points of the animal scale. The first part of his work is simply descriptive of the nervous system of the embryo at each successive month of fœtal life. It constitutes the anatomical groundwork upon which are raised the general laws of cerebral formation, and the higher philosophy of the science. In the second part, Tiedemann has established, by examples drawn from all the grand divisions of the animal kingdom, the universality of the law of formation, as traced in the nervous system of the

* See an excellent Essay on the *Development of the Vascular System in the Fœtus of Vertebrated Animals*, by Dr. Allen Thomson.

human fœtus, and the existence of one and the same fundamental type in the brain of man and of the inferior animals.

The facts which have been unfolded by the industry of Tiedemann, besides leading to the universal law of nervous development, throw important light upon nervous function: for it is observed that the successive increments of nervous matter, and especially of brain, mark successive advances in the scale of being; and, in general, that the development of the higher instincts and faculties keeps pace with that of brain. Thus, in the zoophyta, and in all living beings destitute of nerves, nothing that resembles an instinct or voluntary act is discoverable. In fishes the hemispheres of the brain are small, and marked with few furrows or eminences. In birds they are much more voluminous, more raised and vaulted than in reptiles; yet no convolutions or anfractuosities can be perceived on any point of their surface, nor are they divided into lobes. The brain of the mammalia approaches by successive steps to that of man. That of the rodentia is at the lowest point of organization. Thus the hemispheres in the mouse, rat, and squirrel are smooth and without convolutions. In the carnivorous and ruminating tribes, the hemispheres are much larger and marked by numerous convolutions. In the ape tribe the brain is still more capacious and more convex; it covers the cerebellum, and is divided into anterior, middle, and posterior lobes. It is in man that the brain attains its greatest magnitude and most elaborate organization. Sömmerring has proved that the volume of the brain, referred to that of the spinal marrow as a standard of comparison, is greater in man than in any other animal.

Various attempts have been made of late years, chiefly by the French physiologists, to ascertain the functions of the brain by actual experiment. It will appear from a detailed survey of their labours, that little more than a few general facts respecting the function of its *larger* masses and great natural divisions have flowed from this mode of research. The offices of the *smaller* parts of cerebral substance cannot with any certainty be derived from the phenomena that have been hitherto observed to follow the removal of those parts, since the most practised vivisectors have obtained conflicting results. Nor is it difficult, after having performed or witnessed such experiments, to point out many unavoidable sources of fallacy. In operations on living animals, and especially on so delicate an organ as the brain, it is scarcely possible for the most skilful manipulator to preserve exact anatomical boundaries, to restrain hæmorrhage, or prevent the extension to contiguous parts, of the morbid actions consequent upon such serious injuries, and to distinguish the secondary and varying phenomena, induced by the pressure of extravasated blood, or the spread of an inflammatory process, from those which are essential and primary. The ablation of small and completely insulated portions of brain must, then, be classed among the "agenda" of experimental physiology.

The most decisive researches, that have been hitherto instituted on the functions of the brain, are those of M. Flourens. His mode of operating was to remove cautiously successive thin slices of cerebral matter, and to note the corresponding changes of function. He commenced with the hemispheres of the brain, which he found might be thus cut away, including the corpora striata and thalami optici, without apparently occasioning any pain to the animal, and without exciting convulsive motions. Entire removal of the cerebrum induces a state resembling coma; the animal appears plunged in a profound sleep, being wholly lost to external impressions, and incapable of originating motion; it is deprived, too, according to Flourens, of every mode of sensation. Hence the cerebrum is inferred to be the organ in which reside the faculties of perception, volition and memory. Though not itself sensible, in the ordinary acceptation of the word,—that is, capable, on contact or injury, of propagating sensation,—yet it is the point where impressions made on the external organs of sense become objects of perception. This absence of general sensibility observed in the brain has also been experimentally demonstrated in the nerves dedicated to the functions of sight, of smell and of hearing, and constitutes, perhaps, one of the most remarkable phenomena that have been disclosed by interrogating living nature. Flourens appears, however, to have failed in proving that *all* the sensations demand for their perception the integrity of the brain. He has himself stated that an animal deprived of that organ, when violently struck, "has the air of awakening from sleep," and that if pushed forwards, it continues to advance after the impelling force must have been wholly expended. Cuvier has therefore concluded, in his Report to the Academy of Sciences upon M. Flourens' paper, that the cerebral lobes are the receptacle in which the impressions made on the organs of sight and hearing only, become perceptible by the animal, and that probably there too all the sensations assume a distinct form, and leave durable impressions,—that the lobes are, in short, the abode of memory. The lobes, too, would seem to be the part in which those motions which flow from spontaneous acts of the mind have their origin. But a power of effecting regular and combined movements, on *external stimulation*, evidently survives the destruction of the cerebral hemispheres.

A very elaborate series of experiments on the functions of the brain in general, and especially on those of its anterior portion, have been since performed by M. Bouillaud*. That observer concurs with Flourens in viewing the cerebral lobes as the seat of the *remembrance* of those sensations which are furnished to us by sight and hearing, as well as of all the intellectual operations to which these sensations may be subjected, such as comparison, judgment and reasoning. But he proves that the ordinary tactual sensibility does not require for its manifestation the presence of the brain. For animals entirely deprived of brain were awakened by being struck, and gave evident indications of suffering when exposed to any cause of physical pain. Bouillaud observes, too, that the iris continues obedient to the stimulus of light, after ablation of the hemispheres, and on this ground calls in question the loss of vision asserted by Flourens. Nor are the lobes (he contends,) the only receptacle of intelligence, of instincts and of volition: for to admit this proposition of Flourens would be to grant that an animal which retains the power of locomotion, which makes every effort to escape from irritation, which preserves its appropriate attitude, and executes the same movements after as before mutilation, may perform all those actions without the agency of the will or of instinct. Another doctrine of Flourens, which has been experimentally refuted by Bouillaud, is, "that the cerebral lobes concur *as a whole* in the full and entire exercise of their functions; that when one sense is lost, all are lost; when one faculty disappears, all disappear;" in short, that a certain amount of cerebral matter may be cut away without apparent injury, but that when this limit is passed, all voluntary acts and all perceptions perish simultaneously. Bouillaud, on the contrary, has described several experiments which show that animals, from whom the anterior or frontal part of the brain had been removed, preserved sight and hearing, though deprived of the knowledge of external objects, and of the power of seeking their food.

The second part of M. Bouillaud's researches is entirely devoted to the functions of the anterior lobes of the brain. These were either removed by the scalpel, or destroyed by the actual cautery, in dogs, rabbits and pigeons. Animals thus mutilated feel, see, hear and smell; are easily alarmed, and execute a number of voluntary acts, but cease to recognise the persons

* Magendie, *Journal de Physiologie*, tom. x. p. 36.

or objects which surround them. They no longer seek food, or perform any action announcing a combination of ideas. Thus the most docile and intelligent dogs lost all power of comprehending signs or words which were before familiar to them, became indifferent to menaces or caresses, were no longer amenable to authority, and retained no remembrance of places, of things, or of persons. They saw distinctly food presented to them, but had ceased to associate with its external qualities all perception of its relations to themselves as an object of desire. The anterior or frontal part of the brain is hence inferred to be the seat of several intellectual faculties. Its removal occasions a state resembling idiotism, characterized by loss of the power of discriminating external objects, which, however, coexists with the faculties of sensation.

It will be unnecessary to describe fully in this place the experiments of Professor Rolando of Turin, performed in 1809, and published in Magendie's *Journal*, tom. iii., 1823, since the more important of his facts have reference, not to the brain-proper, but to the cerebellum. His paper certainly contains some curious anticipations of phenomena, since more accurately observed by Flourens and Magendie; yet as regards the brain, properly so called, his results are vague and inconclusive. Accident, rather than a well matured design, seems to have directed what parts of the brain he should remove; and from having comprehended in the same injury totally distinct anatomical divisions, he has rendered it impossible to arrive at the precise function of any one part. Thus we are told that injury of the thalami optici and tubercula quadrigemina in a dog was followed by violent muscular contractions. Now all subsequent experimenters agree, that irritation of the thalami is incapable of inducing convulsive motions; and Flourens has proved that this property has its beginning in the tubercula,—an important fact, which Rolando, with a little more precision in anatomical manipulation, could scarcely have failed to discover.

Magendie has described* some curious experiments on the corpora striata, which, though closely analogous in their results to those on the cerebellum, have their proper place in this section. Removal of one corpus striatum was followed by no remarkable change; but when both had been cut away, the animal rushed violently forwards, never deviating from a rectilinear course, and striking against any objects in its way. In his lecture of February 7, 1828, Magendie, in the presence of his class, removed both corpora striata from a rabbit. The animal

* *Journal de Physiologie*, tom. iii. p. 376.

attempted to rush forwards, and, if restrained, appeared restless, continuing in the attitude of incipient progression. One thalamus opticus was then cut away from the same animal. The direction of its motion was immediately changed from a straight to a curved line. It continued for some time to run round in circles, turning towards the injured side. When the other thalamus was removed, the animal ceased its motions and remained perfectly tranquil, with the head inclined backwards. These experiments, it may be observed, furnish no support to the opinions of MM. Foville and Pinel Grandchamps, who have assigned the anterior lobes and corpora striata as the parts presiding over the movements of the inferior extremities, and the posterior lobes and thalami as regulating the superior.

Cerebellum.—It may be regarded as nearly established by modern researches, that the cerebellum is more or less directly connected with the function of locomotion. The precise nature and extent of its control over the actions of the voluntary muscles are, however, far from being clearly determined. In the higher animals, the mental act of volition probably has its commencing point, as productive of a physical change, in the brain-proper; though it must be confessed that some of the experiments of Flourens, and all of those of Bouillaud, indicate the persistence of many instinctive, and even of some automatic motions, after destruction of the brain. But there *does* appear sufficient evidence to prove that those volitions which have motion as their effect, whatever be their origin, whether in the cerebrum, cerebellum, or medulla oblongata *, require for their accomplishment the cooperation of the cerebellum. This evidence has been mainly supplied by the same inquirers whose researches on the cerebrum have been already analysed.

In the order of time, though not of importance, the experiments of Professor Rolando stand foremost. Injuries of the cerebellum, he observed, were always followed by diminished motive power; and this partial loss of power was always in direct proportion to the amount of injury. A turtle survived upwards of two months the entire removal of the cerebellum, continuing sensible to the slightest stimulus; but when irritants were applied, it was totally unable to move from its place. M. Flourens has since arrived at similar, but more definitive results. He removed in succession thin slices from the cerebellum. After the first two layers had been cut away, a slight weakness and want of harmony and system in the automatic movements were noticed. When more cerebellic substance had

* Flourens, *Mémoires de l'Académie*, tom. ix.

been removed, great general agitation became apparent. The pigeon which was the subject of operation retained, as at first, the senses of sight and hearing, but was capable of executing only irregular unconnected muscular efforts. It lost by degrees the power of flying, of walking, and even of standing. Removal of the whole cerebellum was followed by the entire disappearance of motive power. The animal, if laid upon its back, tried in vain to turn round; it perceived and was apprehensive of blows, with which it was menaced, heard sounds, seemed aware of danger, and made attempts to escape, though ineffectually, —in short, while it preserved, uninjured, sensation and the exercise of volition, it had lost all power of rendering its muscles obedient to the will. The cerebellum is hence supposed by Flourens to be invested with the office of "balancing, regulating or combining separate sets of muscles and limbs, so as to bring about those complex movements depending on simultaneous and conspiring efforts of many muscles, which are necessary to the different kinds of progressive motion." Bouillaud, who has successfully disputed several of the opinions of Flourens respecting the functions of the cerebrum, fully concurs with him as to those of the cerebellum.

Yet, it must be admitted, that there exists also conflicting experimental testimony on this subject. M. Fodera* states that he has found the removal of a part of the cerebellum to be followed, in all cases, either by motion *backwards*, or by that position of the body which precedes retrograde movement. The head is thrown back, the hind legs separated, and the fore legs extended forwards, and pressed firmly against the ground. More complete destruction of the cerebellum occasions the animal to fall on its side; but the head is still inclined rigidly backwards, and the anterior extremities agitated with convulsive movements, tending to cause retrograde motion of the body. Injuries of one side of the cerebellum were observed to produce paralysis of the same side of the body; as might, indeed, have been anticipated from the direct course, without decussation, of the restiform columns which ascend to form the cerebellum. Magendie has described† precisely the same results. A duck, whose cerebellum had been destroyed, could swim only backwards. In the course of his experimental lectures, Magendie, having removed the cerebellum in several rabbits, demonstrated to his class the phenomena of retrograde movement, exactly as they have been recorded by Fodera. It is, then, impossible to regard the conclusions of Flourens as

* *Journal de Physique*, July 1823. † *Ibid.* tom. iii. p. 157.

fully established, opposed as they are by those of so skilful an experimenter as Magendie. Indeed, while Flourens conceives the cerebellum to preside over motion, MM. Foville and Pinel Grandchamps attribute to it the directly opposite function of sensation: and this doctrine seems to derive some support from anatomical disposition; for it has been proved by Tiedemann that the cerebellum is nothing more than an expansion or prolongation of the corpora restiformia, and posterior columns of the spinal medulla, which columns have been shown by Sir Charles Bell to have the office of conveying sensations. But it is not the less true that all recent experiments, even those of Fodera and Magendie, point to some connexion between the cerebellum and the power of voluntary motion. In the present state of our knowledge it would be unsafe to contend for more than the probable existence of some such general relation.

This, then, is all that seems deserving of confidence respecting the functions of the cerebellum itself. But there are some singular phenomena which, though residing in other structures more or less near to the cerebellum, are so analogous to those already described as to call for notice in this place. Magendie has described* the results of injury to the crura cerebelli of a rabbit. Complete division of the right crus was followed by rapid and incessant rotation of the body upon its own axis, from left to right. This singular motion having continued two hours, Magendie placed the rabbit in a basket containing hay. On visiting it the following day he was surprised to find the animal still turning round as before, and completely enveloped in hay. The eyes were rigidly fixed in different lines; that of the injured side being directed forwards and downwards, that of the other side backwards and upwards. If both crura were divided, no motion followed. Magendie hence concluded that these nervous cords are the conductors of impulsive forces which counterbalance one another, and that from the equilibrium of these two forces result the power of standing, and even of maintaining a state of rest, and of executing the different voluntary motions. The inquiry naturally presented itself, whether these forces are inherent in the crura themselves, or emanate from the cerebellum or some other source. To determine this question, portions of substance were removed from both sides of the cerebellum, but unequally, so as to leave intact $\frac{3}{4}$ on the left side and $\frac{1}{4}$ only on the right. The animal rolled towards the right side, and its eyes were fixed in the manner already described. But the left crus being divided, the animal rolled to

* *Journal de Physiologie*, tom. iv. 399.

the left side. Hence it appears that section of the crus has more influence over the lateral rotation of the body than injury of the cerebellum itself; and that the impulsive force does not belong (at least exclusively) to the cerebellum. When the cerebellum was divided precisely in the median line, the animal seemed suspended between two opposing forces, sometimes inclining towards one side, as if about to fall, and again thrown suddenly back to the opposite side. Its eyes were singularly agitated, and seemed about to start from the orbits. Similar movements followed division of the continuous fibres in the pons Varolii. Serres has described a case of similar rotatory motion occurring in the human subject. A shoemaker habituated to excess in alcoholic liquors, after great intemperance was seized with an irresistible disposition to turn round upon his own axis, and continued to move so till death ensued. On inspecting the brain, one of the crura cerebelli was found much diseased, and this was the only alteration of structure visible in any part of the nervous system.

M. Flourens has published in a recent volume of the *Mémoires de l'Académie des Sciences** a description of some striking abnormal motions which followed the division of the semicircular canals of the ears of birds. Though these organs have no anatomical relation to the cerebrum or cerebellum, the altered motions resulting from their division are so analogous to those observed by Magendie after lesions of the corpora striata and crura, that they may be most conveniently described in the same section. Two of the semicircular canals are vertical, and one horizontal. Division of the horizontal canals on each side occasioned a rapid horizontal movement of the head from right to left, and back again, and loss of the power of maintaining an equilibrium, except when standing, or when perfectly motionless. There was also the same singular rotation of the animal round its own axis which follows injury of the crura cerebelli. Section of the inferior vertical canal on both sides produced violent vertical movements of the head, with loss of equilibrium in walking or flying. There was in this case no rotation of the body upon itself, but the bird fell backwards, and remained lying on its back. When the superior vertical canals were divided, the same phenomena were observed as in section of the inferior, except that the bird fell forward on its head, instead of backward. All the canals, both vertical and horizontal, having been divided, in another pigeon, violent and irregular motions in all directions ensued. When,

* tom. ix. p. 454.

however, the bony canals were so cautiously divided as to leave their internal membranous investment uninjured, these abnormal motions were not produced. It is, therefore, in these membranes, or rather in the expansion of the acoustic nerve which overspreads them, that the cause of this phenomenon must reside. No explanation is proposed by Flourens of the control thus exercised by a nerve supposed to minister exclusively to the sense of hearing, over actions so entirely opposite in character. It is remarkable that the irregular movements should observe the same direction in their course as the canals, by the section of which they are induced. Thus the direction of the inferior vertical canal is posterior, that of the superior is anterior, corresponding perfectly with the directions of the abnormal motions.

Medulla Oblongata.—The medulla oblongata, or "bulbe rachidien," is reducible into six columns, or three pairs, viz. two anterior or pyramidal, which partially decussate, two middle or olivary, and two posterior or restiform, which proceed forwards without crossing. It is continuous in structure with the spinal marrow, and enjoys, by virtue of this relation, the same function of propagating motion and sensation. But it is distinguished from the spinal medulla by special and higher attributes, being endowed with the faculty of originating motions, as well as with that of regulating and conducting them. The medulla oblongata, with the cerebrum and cerebellum, constitute, in short, according to Flourens*, those portions of the nervous system which exercise their functions "spontaneously or primordially," and which originate and preside over the vital actions of the subordinate parts. To this latter order of parts, which require an exciting or regulating influence, belongs the spinal medulla. In the superior class, Flourens seems to assign even a higher place to the medulla oblongata than to the cerebrum or cerebellum. For the cerebrum, he observes, may act without the cerebellum; and this latter organ continues to regulate the motions of the body after removal of the cerebrum. But the functions of neither cerebrum nor cerebellum survive the destruction of the medulla oblongata, which seems to be the common bond and central knot combining all the individual parts of the nervous system into one whole.

The medulla oblongata was regarded by Legallois as the mainspring or "premier mobile" of the inspiratory movements. He repeated before a Commission of the Institute of France the leading experiments on which his opinion rested†. In a rabbit

* *Mémoires de l'Académie des Sciences,* tom. ix. p. 478.

† *Œuvres de Legallois,* tom. i. p. 247.

five or six days old, the larynx was detached from the os hyoides and the glottis exposed to view. The brain and cerebellum were then extracted without arresting the inspirations, which were marked by four simultaneous motions,—a gaping of the lips, an opening of the glottis, the elevation of the ribs, and the contraction of the diaphragm. Legallois next removed the medulla oblongata, when all these motions ceased together. In a second rabbit, instead of extracting at once the entire medulla, it was cut away in successive thin slices. The four inspiratory movements continued after the removal of the three first slices, but ceased after the fourth. It was found that the fourth had reached the origin of the eighth pair of nerves. If, instead of destroying the part in which this motive influence resides, it be simply prevented from communicating with the muscles which are subservient to inspiration, a similar effect ought to be produced. Now it is obvious that the medulla oblongata must transmit its influence to the muscles which raise the ribs, through the medium of the intercostal nerves, and therefore of the spinal marrow, and to the diaphragm through the phrenic nerves, and to these through the spinal marrow. In another rabbit, therefore, the medulla spinalis was cut across about the level of the seventh cervical vertebra. The effect of this operation was to arrest the elevation of the ribs, the other three inspiratory motions still continuing. A second section was made near the first cervical vertebra, and consequently above the origin of the phrenic, with the effect of suspending the contraction of the diaphragm. The par vagum was next divided in the neck, and the opening of the glottis ceased. There remained then, of the four inspiratory movements, only the gaping of the lips, which, however, was sufficient to attest that the medulla oblongata still retained the power of producing them all. This power had ceased to call forth the other three motions, only because it no longer had communication with their organs.

M. Flourens, in a recent memoir already referred to*, has confirmed and extended the views first announced by Legallois. He has distinctly traced the comparative action of the medulla spinalis and oblongata, on respiration, in the four classes of vertebrated animals. In birds, he found that all the lumbar and the posterior dorsal medulla might be destroyed without impeding the respiratory function, though it was arrested by removal of the costal medulla. In the mammalia the costal also

* *Mémoires de l'Académie*, tom. ix. 1830.

might be removed, for though the raising of the ribs ceased, the action of the diaphragm continued as long as the origin of the phrenic nerve remained uninjured. In frogs, all the spinal medulla may be destroyed, except the portion, whence spring the nerves supplying the hyoideal apparatus. Every part of the spinal marrow may be removed in fishes without affecting respiration; for all the nerves distributed to the respiratory organs of fishes have their origin in the medulla oblongata. It is hence apparent that the spinal marrow exercises only a variable and relative action on the respiratory function, in the different classes of vertebrated animals. In descending from the higher to the lower points of this scale, the spinal marrow is seen progressively to disengage itself from cooperation in these movements, while the medulla oblongata tends more and more to concentrate them in itself, till in fishes the proper functions of the two medullæ show themselves completely distinct, the spinal ministering to locomotion and sensation, and the oblongata to respiration. The medulla oblongata is, then, the "premier moteur" or the exciting and regulating principle of the inspiratory movements in all classes of vertebrated animals; besides participating, by virtue of its continuity with the spinal marrow, in the proper functions of that organ. From a second series of experiments, M. Flourens concludes that there exists a point in the nervous centres at which the section of those centres produces the sudden annihilation of all the inspiratory movements; and that this point corresponds with the origin of the eighth pair of nerves, commencing immediately above, and ending a little below, that origin,—a result precisely agreeing with that obtained by Legallois.

Spinal Marrow.—It is apparent, that the functions of the three grand divisions of the nervous system, already described, have not yet been distinctly and fully ascertained. Our knowledge of those, which next fall under survey, is more definite and substantial. The vital offices of the spinal medulla—regarded by Legallois as the mainspring of life, and as alone regulating the actions of the heart and nobler organs,—are now reduced to conveying to the muscles the motive impulse of volition, and to propagating to the sensorium commune, impressions made on the external senses. It is not invested with the power possessed by the cerebrum and cerebellum, and perhaps by the medulla oblongata, of spontaneously originating muscular motions. It is mainly, if not exclusively, a *conductor*; a medium of communication between the brain and the external instruments of locomotion and sensation. Flourens, indeed, conjectures that it also has the office of associating the partial contractions of individual muscles into "mouvemens d'ensemble," necessary to the regular motions of the limbs.

Before recording what is known of the spinal cord itself, it will be proper to advert to some recent experiments of Magendie on the serous fluid in which it is immersed. It would appear that a quantity of liquid, varying from two to five ounces in the human subject, is always interposed between the arachnoid tunic and the pia mater, or proper membrane of the cord. The intermembranous bag, occupied by this fluid, communicates with the ventricular cavities at the calamus scriptorius by a round aperture, often large and patent in hydrocephalic subjects. Magendie has therefore named this serous liquid 'cerebro-spinal'. In living animals, it issues in a stream from a puncture of the arachnoid. Its removal occasions great nervous agitation, and symptoms resembling those of canine madness. The sudden increase of its quantity induces coma. Its presence seems essential to the undisturbed and natural exercise of the nervous functions; and this influence probably is dependent upon its pressure, temperature and chemical constitution, since any variation of these conditions is followed by the phenomena of nervous disorder.

The great medullary cord is divided by a double furrow into two lateral halves; and each of these is again subdivided by the insertions of the ligamenta dentata into two columns, one posterior and one anterior. It has been long known that section of any part of the spinal marrow excludes from intercourse with the brain all those parts of the body, which derive their nerves from the cylinder of medulla *below* the point of injury. The muscles, so supplied, are no longer obedient to the control of the will, and the tegumentary membranes similarly situated entirely lose their sensibility. This interruption of the relations which subsist between the central seat of volition and sensation, and the rest of the body, whether due to direct injury of the great nervous masses or communicating nerves, or produced by the pressure of extravasated fluids, by morbid growths, or by various poisonous matters, constitutes the condition known by the name 'paralysis'. In cases of this kind it is frequently observed that the powers of sensation and locomotion are simultaneously impaired or destroyed. But examples are not wanting, even in the earliest clinical records, of the total loss of one of those faculties with perfect integrity of the other. Such facts naturally suggested the belief that the power of propagating sensations, and that of conveying motive impressions, resided in distinct portions of the nervous system. This opinion, however, remained mere matter of conjecture until a recent period, when it was unequivocally established by Sir Charles Bell. From the original experiments of that most distinguished physiologist, repeated and confirmed by Magendie, it follows that the faculty of conducting sensations resides exclusively in the two posterior columns of the medulla, while that of communicating to the muscular system the motive stimulus impressed by volition is the attribute of the two anterior columns. The same limitation of function is found in the nervous roots which spring from these separate columns. Thus each spinal nerve is furnished with a double series of roots, one set of which have their origin in the anterior medullary column, and one in the posterior. The spinal nerves are, in consequence of this anatomical composition, nerves of twofold function, containing in the same sheath distinct continuous filaments from both columns, and exercising, in the parts to which they are distributed, the double office of conductors of motion and sensation. It will afterwards appear, in our history of individual nerves, that all those which spring from the brain, except the fifth and eighth pairs, possess only a single function.

Sufficient experimental proof of the foregoing propositions has been furnished by Sir Charles Bell and by M. Magendie. Thus, division of the posterior roots of the spinal nerves is uniformly followed by total absence of feeling in the parts of the body to which the injured nerves are distributed, while their motive power remains undiminished. Magendie has further observed, that if the medullary canal be laid open, and the two posterior cords be touched or pricked slightly, there is instant expression of intense suffering; whereas, if the same or a greater amount of irritation be applied to the anterior columns, there are scarcely any signs of excited sensibility. The central parts of the medulla seem also nearly impassable*. They may be touched, and even lacerated, according to Magendie, without exciting pain, if precautions are taken to avoid the surrounding medullary substance. In general, the properties of the spinal marrow, and especially its sensibility, seem to reside mainly on its surface; for slight contact, even of the vascular membranes covering the posterior columns, caused acute pain.

The first experiment of Sir C. Bell consisted in laying open the spinal canal of a living rabbit, and dividing the posterior roots of the nerves that supply the lower limbs. The animal was able to crawl. In his second trial he first stunned the rabbit, and then exposed the spinal marrow. On irritating the

* *Annales de Chimie et de Physique*, tom. xxiii. p. 436.

posterior roots, no motion was induced in any part of the muscular frame; but on grasping the anterior roots, each touch of the forceps was followed by a corresponding contraction of the muscles supplied by the irritated nerve. Magendie has described* the following experiments, which he has since declared were made without any knowledge of the prior ones of Sir C. Bell. The subjects chosen for the operation were puppies about six weeks old; for in these it was easy to cut with a sharp scalpel through the vertebræ and to expose the medulla. In the first, the posterior roots of the lumbar and sacral nerves were divided, and the wound closed: violent pressure, and even pricking with a sharp instrument, awakened no sensation in the limb supplied by the nerves which had been cut; but its motive power was uninjured. A second and a third trial gave the same results. Magendie then divided in another animal, though with some difficulty, the anterior roots of the same nerves on one side. The hind limb became flaccid and entirely motionless, though it preserved its sensibility. Both the anterior and posterior roots were cut in the same subject with destruction of motion and sensation. In a second paper† Magendie has related the following additional facts. The introduction of nux vomica into the animal economy is well known to give rise to violent tetanic convulsions of the whole muscular system. This property was made available as a test of the functions of the separate orders of nervous roots. It was found that, while all the other muscles of the body were agitated, when under the influence of this poison, by violent spasmodic contractions, the limb, supplied by nerves whose anterior roots had been previously divided, remained supple and motionless. But when the posterior roots only had been cut, the tetanic spasms were universal. It would seem, however, that the seats of the two faculties of conducting motion and sensation are not strictly insulated by exact anatomical lines, but that they rather pass into each other with rapidly decreasing intensity. Thus irritation of the *anterior* roots, when connected with the medulla, gives birth, along with motive phenomena, to some evidences of sensibility; and, *vice versâ*, stimuli applied to the posterior roots, also undivided, occasion slight muscular contractions. In this last case it is, indeed, probable that the irritation travelled from the posterior roots upwards to the brain in the accustomed channel, and gave rise to a perception of pain, which prompted the muscular effort. Indeed, after division of the posterior nervous roots, ordinary stimulants, applied to the

* *Journal de Physiologie*, tom. ii. p. 276. August 1822. † *Ibid.* tom. ii. p. 366.

ends not connected with the medulla, produced no apparent effects; though the galvanic fluid directed upon either order of roots gave rise to muscular contractions. These were more complete and energetic when the anterior roots were the subjects of the experiment.

Besides the evidence thus obtained by direct experiments on living animals, several important facts have been gathered from the pathology of the nervous system in man. These consist of cases of insulated paralysis of either motion or feeling, referred to the changes in structure observed after death. Sir Charles Bell has himself recorded several examples of this kind strongly confirming his experimental results; and others of similar tendency are scattered through the successive volumes of Magendie's Journal*. But it must be admitted, that evidence of this kind is seldom distinct and conclusive. The structural changes, induced by disease, are rarely so circumscribed in seat and extent as to represent adequately the operations of the scalpel; and often when they are thus isolated within anatomical bounding lines, they affect, by pressure, or by the spread of the same morbid process, in a degree too slight to leave decided traces, the functions of contiguous parts, thus clouding the judgments of the best pathologists, and invalidating their inferences. There is, however, a very remarkable case described by Professor Royer Collard, to which these objections do not apply. Sprévale, an invalided soldier, was upwards of seventeen years the subject of medical observation in the Maison de Santé of Charenton. This individual remained for the last seven years of his life with the legs and thighs permanently crossed, and totally incapable of *motion*, though retaining their sensibility. On opening after death the spinal canal, there was found the pultaceous softening (*ramollissement*) of the whole *anterior* part of the medulla, and of almost the whole of the fibrous cords which form it. The *anterior* roots of the spinal nerves had also lost their accustomed consistency; while the posterior surface of the spinal cord, and its investing membrane, were healthy. Several of the cases observed by Sir Charles Bell furnish also unequivocal proof of the soundness of the views developed by experiment.

There exist, indeed, few truths in physiology established on so wide and solid a basis of experimental research and pathological observation, as those deduced by Sir Charles Bell, the original discoverer, and by Magendie, his successor in the path of inquiry, respecting the offices of the spinal medulla.

* See in particular Dr. Rullier's case, tom. iii. p. 173; and Dr. Koreff's, tom. iv. p. 376.

This organ may now be regarded as mainly, if not solely, a medium of intercourse between the external world and the brain, and again between the brain and the voluntary muscles, its two anterior columns being subservient to motion, its two posterior to sensation. In the present state of our knowledge it would be fruitless to try to penetrate into the minute philosophy of these actions: but it seems probable, from recent discoveries on the ultimate anatomy of tissue, that these actions are molecular, having their place in the globular elements, into which all living textures are resolvable by microscopic analysis;—that the physical changes, *e. g.* impressed by external objects on the delicate net-work of nerve which invests the tegumentary membranes and open cavities, are propagated thence, from particle to particle, along the continuous filaments, to their origins in the posterior spinal columns, and thence to the central point, where they become objects of perception;—and that the motive stimulus of volition is similarly transmitted down the anterior columns and nerves, to the organs of locomotion. Indeed, it is a legitimate inference from Sir Charles Bell's discoveries, that a simple nervous filament, or medullary column, can only propagate an impression in one line of direction, viz. either towards or from the central seat of perception and of will; and this curious law of nervous actions would seem to point at some insensible molecular *motion* as their essential condition.

It remains to investigate the arguments which have been supposed to prove the residence in the spinal marrow of the power of originating and controlling the actions of the heart. This question has been matter of eager controversy, from its bearing upon the general relations of nerve and muscle. Without prejudging this latter topic, it may simplify its future consideration, and will at the same time be more consistent with strict arrangement, to state here merely the facts which have reference to the spinal medulla.

The work of Legallois, entitled "*Expériences sur le Principe de la Vie, notamment sur celui des Mouvemens du Cœur et sur le Siége de ce Principe**," was the first remarkable essay on the relations between the heart and the spinal cord. It will, however, be sufficient to allude in general terms to the conclusions of Legallois, since they have been entirely subverted by the subsequent researches of Dr. Wilson Philip and M. Flourens. Legallois's main doctrine was, that the principle which animates each part of the body resides in that part of the spinal medulla whence its nerves have their origin; and that it is also

* *Œuvres de Legallois,* tom. i. pp. 97, 99, &c.

from the spinal cord that the heart derives the principle of its life and its motion*. The experimental proof supposed to establish these propositions consisted in destroying in different rabbits portions of the cervical, dorsal and lumbar medulla. Cessation of the heart's action was affirmed to be the constant result of the operation; but even in some of Legallois's own experiments†, the motions of the heart continued after considerable injury had been inflicted on the spinal cord, and especially on its lower divisions. Still more unequivocal is the evidence that has been advanced by Dr. Wilson Philip, in his *Inquiry into the Laws of the Vital Functions*. His experiments, which were very numerous and judiciously varied, show that the circulation continues long after entire removal of the spinal marrow, and that by artificially maintaining respiration, the motions of the heart may be almost indefinitely prolonged. Flourens, in the 10th vol. of the *Mém. de l'Académie*‡, has lately confirmed Dr. Philip's views : he has shown that the circulation is entirely independent of the spinal marrow. The influence apparently exerted is only secondary, being due to the suspension of the respiratory movements. Thus all those portions of the spinal marrow which can be destroyed in the different classes of animals without arresting respiration, may be removed without affecting the circulation. In fishes and frogs the entire spinal cord may be destroyed without checking the heart's motions, because in these classes the medulla oblongata presides exclusively over the respiratory function.

Nerves.—The classification of nerves, which is most convenient to the physiologist, is based upon their vital properties or functions. Such an arrangement would distribute them into—1, nerves of motion; 2, nerves both of motion and sensation; 3, the nerves ministering to the senses of sight, smell and hearing; and 4, the ganglionic system, or, according to Bichat, nerves of organic life. Sir Charles Bell has added a fifth class, comprising nerves which he supposes are dedicated to the respiratory motions. But it will afterwards appear, that the existence of an exclusive system of respiratory nerves is not supported by sufficient evidence.

The first class of nerves exercising the single office of conveying motion comprehends the 3rd, 4th, 6th, portio dura of the 7th, the 9th, and perhaps two divisions of the 8th, viz. the glossopharyngeal and spinal accessory. Mr. H. Mayo's experiments detailed in his *Anatomical and Physiological Commentaries*, No. 11. (and *Journal de Physique*, tom. iii.) throw much

* p. 259. † pp. 100, 101, 105. ‡ p. 625.

light on the functions of several of these nerves. The motions of the iris, he shows, require the integrity of the third pair, division of these nerves being always followed by full dilatation of the pupils, which cease to be obedient to the stimulus of light. If the divided end of the nerve communicating with the eye be pinched by the forceps, the iris contracts. Hence it is apparent that diminution of the aperture of the pupil is the result of action, and dilatation of the pupil the result of relaxation, of the iris. Flourens has shown that complete extirpation of the tubercula quadrigemina also paralyses the iris, and that irritation of those bodies excites its contractions. The same effect is noticed by Mayo to arise from division or irritation of the optic nerve. He divided the optic nerves within the cranium of a pigeon immediately after decapitation. When the end of the nerve connected with the ball of the eye was seized in the forceps, no action ensued; but when the end attached to the brain was irritated, the iris immediately contracted. These several experiments clearly indicate the dependence of the iris upon the optic nerve, upon the tubercula from which one root of that nerve springs, and upon the third pair. The stimulus of light impinges upon the retina, is conveyed along the optic nerve through the tubercle to the sensorium, whence the motive impression is propagated to the iris by the third encephalic nerve.

It is not so easy to define the precise mode of action of the pathetici, or fourth pair of nerves. Sir Charles Bell * supposes that they are destined "to provide for the insensible and instinctive rolling of the eyeball, and to associate this motion of the eyeball with the winking motions of the eyelids." He even conjectures that "the influence of the fourth nerve is, on certain occasions, to cause a relaxation of the muscle to which it goes." It is certain, however, from its exclusive distribution to the superior oblique muscle, that the fourth is a nerve of motion. The sixth nerve is also a nerve of voluntary motion, and is sent to the rectus externus of the eyeball.

Sir Charles Bell has placed the portio dura of the seventh pair among his respiratory nerves. There is, however, no doubt that it is simply a motive nerve, and that it is indeed the only nerve of motion, which supplies all the muscles of the face, except those of the lower jaw and palate. Division of this nerve occasions no expression of pain, according to Bell; but Mayo's experience is opposed to this absence of sensibility †. "The motion of the nostril of the same side instantly ceased,

* *Natural System of Nerves*, p. 358.

† See Mr. H. Mayo's *Anatomical and Physiological Commentaries*, Part I.; and *Outlines of Human Physiology*, 2nd edit., p. 334.

after its section in an ass*, and that side of the face remained at rest and placid during the highest excitement of the other parts of the respiratory organs." These and similar observations are all consistent with the opinion, that the seventh is simply a nerve of voluntary motion. It will afterwards appear that it has no claim to any further endowment.

Mr. Herbert Mayo infers from his experiments, that the three divisions of the eighth pair are all nerves both of motion and sensation. Thus the glossopharyngeus is a nerve of motion to the pharynx, and perhaps of sensibility to the tongue. He observed that "on irritating the glossopharyngeal nerve in an animal recently killed, the muscular fibres about the pharynx acted, but not those of the tongue†." Irritation of the spinal accessory produced both muscular contractions and pain. The par vagum, he conceives, bestows sensibility on the membrane of the larynx, besides conveying the motive stimulus to its muscles. This nerve has been the subject of experiment from the earliest times, and Legallois has minutely described the results obtained by successive inquirers‡. These were singularly discordant, and gave origin to the most opposite theories of the mode of action of the par vagum. In the greater number of experiments, section of this nerve was followed, after a longer or shorter interval, by death. Piccolhomini contended that the division of the nerve was fatal from its arresting the movements of the heart, and after him Willis supported the same doctrine. By Haller, on the contrary, the cause of death was sought in disturbance of the digestive functions. Bichat and Dupuytren seem to have been the first to obtain a glimpse of the true seat of injury. The former remarked that the respiration became very laborious after section of the nerve, and Dupuytren distinctly traced death to asphyxia. Legallois has established by numerous experiments the accuracy of this last view. He has shown that in very young animals death is the immediate consequence of the operation of cutting either the par vagum or its recurrent branch, and that the suddenness of the effect is due to the narrowness of the aperture of the glottis in early age. In adult animals, the asphyxia is induced by the effusion of serous fluids and ropy discoloured mucus into the bronchial tubes and air-cells. More recently, Dr. Wilson Philip has practised the section of the par vagum with an especial reference to its influence upon digestion. He divided the nerve below the origin of the inferior laryngeal branch, as in this case the

* pp. 105, 107. † *Outlines of Human Physiology*, 2nd edit., p. 337.

‡ *Œuvres*, p. 154 *et seq.*

dyspnœa is much less considerable than when the wound is inflicted on the higher portion*. It was found, in all these trials, that food introduced into the stomach after the operation remained wholly undigested. Hence Dr. Philip infers the dependence of secretion upon nervous influence, a conclusion, it has been remarked by Dr. Alison, not logically deducible from the experimental data†.

The par vagum cannot then, it is obvious, be included in the class of nerves subservient solely to motion; and it is even doubtful whether the other two divisions of the eighth pair are not also endowed with sensibility. Respecting the function of the ninth, or lingual, there is, however, no place for hesitation. It has been experimentally proved by Mr. Mayo to supply the muscles of the tongue; though he also asserts that pinching it with the forceps excited pain. Three of these nerves, the third, sixth, and ninth, arise, it was first remarked by Sir Charles Bell, from a tract of medullary matter continuous with the anterior column of the spinal marrow: and hence their exclusive office of conducting motive impressions.

II. There are thirty-two pairs of nerves of similar anatomical origin and composition, which possess the twofold office of communicating motion and sensation. Of these, all excepting one (the fifth pair of the cerebral nerves) spring from the spinal marrow. These thirty-one pairs are precisely analogous in formation, being all constituted of two distinct series of roots, one from the anterior column, and one from the posterior column of the spinal marrow. The posterior funiculi collected together form a ganglion, seated just before this root is joined by the anterior root. It has been already stated that the power of propagating sensation resides in the posterior column, and in the nervous roots arising from it, and that the motive faculty has its seat in the anterior column and roots. The evidence, also, supplied by Bell and Magendie, that the spinal nerves are hence nerves of double office, has been fully detailed. It remains, then, to establish the title of the fifth pair of cerebral nerves to be included in the same class with the spinal nerves.

The analogy in structure and mode of origin between the fifth pair and the nerves of the spine has been long matter of observation. Prochaska has thus distinctly noticed it in a passage of his Essay *De Structurâ Nervorum*, published in 1779, first pointed out to me by my friend Dr. Holme: "Quare omnium cerebri nervorum, solum quintum par post ortum suum

* *Experimental Inquiry*, 3rd edit., p. 109.

† Dr. Alison, *Journal of Science*, vol. ix. p. 106.

more nervorum spinalium, ganglion semilunare dictum, facere debet? sub quo peculiaris funiculorum fasciculus ad tertium quinti paris ramum, maxillarem inferiorem dictum, properat, insalutato ganglio semilunari, ad similitudinem radicum anteriorum nervorum spinalium?" Sömmerring has also pointed out with equal clearness the resemblance in distribution between the smaller root of the fifth and the anterior roots of the spinal nerves. But Sir Charles Bell was the first to establish the identity of their functions, and to arrange them prominently in the same natural division. His experiment consisted in exposing the fifth pair at its root, in an ass, the moment the animal was killed. "On irritating the nerve, the muscles of the jaw acted, and the jaw was closed with a snap. On dividing the root of the nerve in a living animal, the jaw fell relaxed." In another experiment the superior maxillary branch of the fifth nerve was exposed. "Touching this nerve gave acute pain; . . . . . the side of the lip was observed to hang low, and it was dragged to the other side." Sir Charles Bell concluded that the fifth nerve and its branches are endowed with the attributes of motion and sensation. This, though correct as regards the nerve itself, viewed as a whole, is strictly true only of the lowest of its three *divisions,* viz. the inferior maxillary. The ophthalmic and the superior maxillary, the subject of the last experiment, are nerves simply of sensation. Mr. Herbert Mayo in the Essay already referred to, has pointed out this error, and has defined with minute precision the relative offices of the fifth and seventh nerves. By a careful dissection of the fifth nerve he found that the anterior branch, or smaller root, which goes, as Prochaska was aware, entirely to the inferior maxillary, is distributed exclusively to the circumflexus palati, the pterygoids, and temporal and masseter *muscles.* He observed that section of the supra and infra orbitar branches, and of the inferior maxillary, near the foramina, whence they emerge, induces loss of sensation in the corresponding parts of the face. It may then be regarded as fully proved that the trigeminus or fifth pair is the nerve which bestows sensation on the face and its appendages, and motion only on the muscles connected with the lower jaw. The other muscles of the face derive their motive power from the portio dura of the seventh nerve.

M. Magendie has also published several memoirs on the functions of the fifth pair. In these he attempts to prove that the olfactory nerve is not the nerve of smell; that the optic is but partially the nerve of vision; and that the auditory is not the principal nerve of hearing. It is in the fifth pair that he supposes all these distinct and special endowments to reside.

But the experimental proof will be found to be singularly inconclusive. The olfactory nerves were entirely destroyed in a dog. After the operation it continued sensible to strong odours, as of ammonia, acetic acid, or essential oil of lavender; and the introduction of a probe into the nasal cavity excited the same motions and pain as in an unmutilated dog. The fifth pair was then divided in several young animals, the olfactory being left entire. All signs of the perception of strongly odorous substances, as sneezing, rubbing the nose, or turning away the head, entirely disappeared. From these facts Magendie infers that the seat of the sensations of smell is in the fifth, and not in the first pair of nerves. It is obvious that Magendie has confounded two modes of sensation, which are essentially distinct in their nature and in their organic seat, viz. the true perceptions of smell, and the common sensibility of the nasal passages. The phenomena, which he observed to cease after the section of the fifth nerve, are the results of simple irritation of the pituitary membrane, and are manifestly wholly unconnected with the sense of smelling, since they are producible by all powerful chemical agents, even though *inodorous*, as, for example, by sulphuric acid. No proof has been given that the true olfactory perceptions do not survive the destruction of the fifth pair. Indeed, in a subsequent paper, Magendie confesses that the loss of sensibility in the nasal membrane, after section of the fifth, does not prove the residence of the sense of smell in the branches of that nerve; but merely that the olfactory nerve requires, for its perfect action, the cooperation of the fifth pair, and that it possesses only a special sensibility to odorous particles.

There is even less ground for supposing that the fifth pair is in any degree subservient to the senses of sight and hearing. After cutting this nerve on one side, the flame of a torch was suddenly brought near the eye, without inducing contraction of the pupil; but the direct light of the sun caused the animal to close its eyelids. Thus the sensibility of the retina, though somewhat impaired, was not destroyed by division of the fifth pair. But section of the optic nerves was immediately followed by total blindness. In another rabbit Magendie divided the fifth pair on one side, and the optic nerve on the other. The animal, he states, was completely deprived of sight, though the eye, in which the fifth pair only had been cut, remained susceptible to the action of the solar rays. No evidence, however, is offered to show that the animal was entirely blind: on the contrary, the only change observed, on approaching a torch to an *uninjured* eye, was contraction of the iris; and this we are told was actually observed in the eye of the side, on which the fifth nerve had been divided.

Magendie has assigned another singular function to the fifth pair, viz. to preside over the nutrition of the eye. Twenty-four hours after section of this nerve, incipient opacity of the cornea was observed, which gradually increased till the cornea became as white as alabaster. There was also great vascularity of the conjunctiva extending to the iris, with secretion of pus, and formation of false membranes in the anterior chamber. About the eighth day, the cornea began to detach itself from the sclerotica, the centre ulcerated, and the humours of the eye finally escaped, leaving only a small tubercle in the orbit. In this experiment, the nerve had been divided in the temporal fossa, but when cut immediately after leaving the pons Varolii, the morbid changes were less marked, the movements of the globe of the eye were preserved, the inflammation was limited to the superior part of the eye, and the opacity occupied only a small segment of the circumference of the cornea. After division of the nerve near its origin in the medulla, no traces of disease were discoverable in the eye till the seventh day, and these symptoms never became very prominent. Several cases have been since recorded of structural disease of this nerve in the human subject, with the concomitant symptoms. That of Lainé, described by Serres in the 4th vol. of Magendie's *Journal*, furnishes strong support to the views of Magendie*.

A different explanation of this fact and of others which have a tendency to refer secretion and nutrition to the control of the nervous system has been proposed by Dr. Alison. Mucous surfaces are protected from the contact of air and foreign bodies by a copious secretion, which is evidently regulated in amount by their sensibility, since it is increased by any unusual irritation. This is especially true of the membrane of the eye. Now section of the fifth pair is known to paralyse the sensibility of that organ, and the contact of air or other irritating body upon the *insensible* membrane, instead of inducing an augmented mucous discharge, will excite the inflammatory process described by Magendie. The disorder of the digestive function†, which followed division of the par vagum in the experiments of Dr. Wilson Philip, and the ulceration of the coats of the bladder after injury of the lower part of the spinal marrow, are attributed by Dr. Alison to the same cause.

The class of nerves which comprehends the fifth pair and

* See also a case of destruction of the olfactory nerves, tom. v.

† *Outlines of Physiology*, p. 71.

the thirty-one pairs of spinal nerves, becomes, after the union of their roots, invested with a twofold endowment, and continues so throughout their entire course and final distribution to the muscular tissue. It would appear, indeed, from a later paper of Sir Charles Bell*, that nerves of sensation, as well as of motion, are necessary to the perfect action of the voluntary muscles. "Between the brain and the muscles there is a circle of nerves; one nerve conveys the influence from the brain to the muscle, another gives the sense of the condition of the muscle to the brain." In the case of the spinal nerves this circle of intercourse is at least probable; but proof of its necessity must be obtained, from observing the habitudes of those encephalic nerves, which minister exclusively to motion. Now it is found, on minute dissection, that the muscles of the eyeball, which are supplied by the third, fourth and sixth motive nerves, also receive sensitive filaments from the ophthalmic branch of the fifth; and that the muscles of the face, to which the portio dura is distributed, are also furnished with branches of sensation from the fifth. Sir Charles Bell has further shown that the muscles of the lower jaw, to which the motive impression is propagated by the muscular branch of the inferior maxillary, draw nervous supplies also from the ganglionic or sensitive branch of that division of the fifth pair. This complicated provision has its origin, he supposes, in its being "necessary to the governance of the muscular frame that there should be consciousness of the state or degree of action of the muscles."

III. The olfactory, auditory and optic nerves are gifted with a special sensibility to the objects of the external senses, to which they respectively minister. Magendie seems to have been the first to prove, experimentally, that they do not also share the common or tactile sensibility. He exposed the olfactory nerves, and found that, like the hemispheres of the brain from which they spring, they are insensible to pressure, pricking, or even laceration. Strong ammonia was dropped upon them without eliciting any signs of feeling. The optic nerve, and its expansion on the retina, participate with the olfactory in this insensibility to stimulants. This was proved by Magendie in the human subject as well as in animals. In performing the operation of depressing the opaque lens, he repeatedly touched the retina in two different individuals without awakening the slightest sensation. The portio mollis, or acoustic nerve, was also touched, pressed, and even torn without causing pain.

* *Philosophical Transactions*, 1826, p. 163.

IV. The functions of the ganglia, of the great sympathetic nerve, and its intricate plexuses and anastomotic connexions, are matter, at present, of conjecture. Dr. Johnstone, in an Essay on the Use of the Ganglions, published in 1771, has described a few inconclusive experiments on the cardiac nerves. He supposes that "ganglions are the instruments by which the motions of the heart and intestines are rendered uniformly involuntary,"—a notion which Sir Charles Bell has shown to be totally unsound. The best history of opinions, to which indeed our knowledge reduces itself, will be found in the physiological section of Lobstein's work, *De Nervi Sympathetici Fabrica, Usu, et Morbis**.

In the earliest of his communications to the Royal Society, as well as in his last work on the Nervous System †, Sir Charles Bell has maintained the existence of a separate class of nerves, subservient to the regular and the associated actions of respiration. The origins of these nerves ‡ "are in a line or series, and from a distinct column of the spinal marrow. Behind the corpus olivare, and anterior to that process, which descends from the cerebellum, called sometimes the corpus restiforme, a convex strip of medullary matter may be observed. From this tract of medullary matter, on the side of the medulla oblongata, arise, in succession from above downwards, the portio dura of the seventh nerve, the glossopharyngeus nerve, the nerve of the par vagum, the nervus ad par vagum accessorius, and, as I imagine, the phrenic and the external respiratory nerves." The fourth pair is also received into the same class.

This doctrine of an exclusive system of respiratory nerves, associated in function by virtue of an anatomical relation of their roots, has not, as Sir Charles Bell seems himself aware §, received the concurrence of many intelligent physiologists of this country or of the Continent. Mr. Herbert Mayo, in the admirable Essay already referred to, was the first to point out the true relations of the fifth and seventh nerves. He has shown that the muscles of the face, excepting those already enumerated, which elevate the lower jaw, receive their motive nerves exclusively from the seventh, and consequently that this nerve must govern *all* their motions, voluntary as well as respiratory. But Dr. Alison, in his very elaborate paper || "On the Physiological Principle of Sympathy," has cast considerable doubts on

* Paris 1823. † 4to, 1830.

‡ *The Nervous System of the Human Body*, p. 129. 4to, 1830.

§ *Op. cit.*, p. 115.

|| *Transactions of the Medico-chirurgical Society of Edinburgh*, 1826, vol. ii. p. 165.

the soundness of this part of Sir Charles Bell's arrangement, as respects not only the individual nerves thus classed together, but even the general principle on which the entire system rests. The reasoning of Dr. Alison consists, first, in referring the phenomena of natural and excited respiration to the comprehensive order of sympathetic actions. In these "the phenomenon observed is, that on an irritation or stimulus being applied to one part of the body, the voluntary muscles of another, and often distant part, are thrown into action." Now it has been long since fully established by Dr. Whytt, that these associations in function cannot be referred to any connexions, either in *origin* or in course, of the nerves supplying remote organs so sympathizing; and that a *sensation* is the necessary antecedent of the resulting muscular action. Thus it is known that these actions cease in the state of coma; are not excited when the mind is strongly impressed by any other sensation or thought; and that the same muscular contractions may be induced by the irritation of different parts of the body, provided the same sensation be excited. Dr. Alison has, however, failed to show* that the essential acts of inspiration, viz. the contractions of the diaphragm and intercostals, require the intervention of a sensation. Their continuance in the state of coma, and in the experiments of Legallois and Flourens after the entire removal of the brain, and their distinct reference by these two inquirers to the medulla oblongata, which has never been supposed to be the seat of sensation, prove them to be independent of the will and of perception. But this is true only of the essential, not of the associated respiratory phenomena.

Dr. Alison proceeds to show that there is equal reason for classing almost all the nerves of the brain, and many more of the spinal nerves, with those exclusively named respiratory by Sir Charles Bell. Thus the lingual nerve governs an infinite number of motions strictly associated with respiration: the inferior maxillary "moves the muscles of the lower jaw in the action of sucking,—an action clearly instinctive when first performed by the infant, frequently repeated voluntarily during life, and always in connexion with the act of respiration." Again, the sensitive branches of the fifth pair cooperate in the act of sneezing. But if these nerves be admitted into the system, the fundamental principle of that system, viz. origin in a line or series, is at once violated. Nor is this connexion in origin more than matter of conjecture, as regards two of the

* p. 176, and note.

most important of the nerves, classed by Sir Charles Bell himself as respiratory,—the phrenic and the external respiratory. These two nerves branch from the cervical or regular double-rooted series. Moreover, the circumstance of rising in linear succession is not found to associate nerves in function. "Between the roots of the phrenic nerve and those of the intercostals, there intervene in the same series the origins of the three lowest cervical nerves, and the first dorsal, which go chiefly to the axillary plexus and to the arm, and which are not respiratory nerves."

In recapitulation, the following facts are among the most important that have been fully ascertained in the physiology of the nervous system.

1. One universal type has been followed in the formation of the nervous system in vertebrated animals. The brain of the human fœtus is gradually evolved in the successive months of uterine existence; and these stages of progressive development strictly correspond with permanent states of the adult brain at inferior degrees of the animal scale.

2. These successive increments of cerebral matter are found to be accompanied by parallel advances in the manifestation of the higher instincts and of the mental faculties.

3. That the brain is the material organ of all intellectual states and operations, is proved by observation on comparative development, as well as by experiments on living animals, and by the study of human pathology. But there does not exist any conclusive evidence for referring separate faculties, or moral affections, to distinct portions of brain.

4. Certain irregular movements are produced by injuries of the corpora striata, thalami optici, crura cerebelli, and semicircular canals of the internal ear.

5. The tubercula quadrigemina preside over the motions of the iris, and their integrity seems essential even to the functions of the retina. They are also, according to Flourens, the points, at which irritation first begins to excite pain and muscular contractions.

6. The cerebellum appears to exercise some degree of control over the instruments of locomotion; but the precise nature and amount of this influence cannot be distinctly defined.

7. The cerebrum, cerebellum and medulla oblongata possess the faculty of acting primordially, or spontaneously, without requiring foreign excitation. The spinal cord and the nerves are not endowed with spontaneity of action, and are therefore termed subordinate parts.

8. The medulla oblongata exercises the office of originating and regulating the motions essential to the act of respiration. By virtue of its continuity with the spinal marrow, it also participates in the functions of that division of nervous matter.

9. The function of the spinal cord is simply that of a *conductor* of motive impulses, from the brain to the nerves supplying the muscles, and of sensitive impressions from the surface of the body to the sensorium commune. These two vital offices reside in distinct portions of the spinal medulla,—the propagation of motion in its anterior columns, the transmission of sensations in its posterior columns. There is no necessary dependence of the motions of the heart, and the other involuntary muscles, on the spinal marrow.

10. The nerves are comprehended in the four following classes:—I. Nerves simply of motion; II. Of motion and sensation; III. Of three of the senses; IV. The ganglionic system.

I. The nerves of motion are the third, fourth, sixth, portio dura of the seventh, and the ninth. It is not ascertained whether the glossopharyngeal and spinal accessory nerves belong to this or to the second class.

II. The function of ministering both to motion and sensation is possessed by the fifth pair of cerebral nerves, and by the spinal nerves, which agree precisely in anatomical composition. The par vagum, however, which is one of the irregular nerves, has also a twofold endowment.

III. This division comprises the first and second pairs, and the portio mollis of the seventh pair. These nerves are insensible to ordinary stimulants, and possess an exclusive sensibility to their respective objects,—viz. odorous matter, light, and aërial undulations.

IV. The system of the great sympathetic nerve, and its associated plexuses and ganglia.

Report on the present State of our Knowledge respecting the Strength of Materials. By Peter Barlow, *Esq., F.R.S., Corr. Memb. Inst. France, &c. &c.*

The theory of the strength of materials, considered merely as a branch of mechanical or physical science, must be admitted to hold only a very subordinate rank; but in a country in which machinery and works of every description are carried to a great extent, it certainly becomes a subject of much practical importance; and it was no doubt viewing it in this light which led the Committee of the British Association, at their last Meeting, to do me the honour to request me to furnish them with a communication on the subject. In drawing my attention to this inquiry, the Committee have subdivided it into the following heads:—1. Whether, from the experiments of different authors, we have arrived at any general principles? 2. What those principles are? 3. How modified in their application to different substances? And what are the differences of opinion which at present prevail on those subjects?

To these questions, without a formal division of the Essay, I shall endeavour to reply in the following pages, by drawing a concise sketch of the experimental and theoretical researches which have been undertaken with reference to these inquiries.

The subject of the strength of materials, from its great practical importance, has engaged the attention of several able men, both theoretical and practical, and much useful information has been thereby obtained. As far as relates to the mechanical effects of different strains, everything that can be desired has been effected; but the uncertain nature of materials generally, will not admit of our drawing from experiment such determinate data as could be wished. Two trees of the same wood, grown in the same field, having pieces selected from the same parts, will frequently differ from each other very considerably in strength, when submitted to precisely the same strain. The like may be said of two bars of iron from the same ore, the same furnace, and from the same rollers, and even of different parts of the same bar; and so likewise of two ropes, two cables, &c. We must not, therefore, in questions of this kind, expect to arrive at data so fixed and determinate as in many other practical cases; but still, within certain limits, much important information has been obtained for the guidance of practical men; and by tabulating such results in a subsequent part of this article, I shall endeavour to answer the leading questions of the Committee of the British Association, as far, at least, as relates to experimental results. In reference to theory, it must also be admitted that some uncertainty still remains; but this likewise is in a great measure to be referred to the nature of the materials, which is such as to offer resistances by no means consistent with any fixed and determinate laws.

Hence some authors have assumed the fibres or crystals composing a body to be perfectly incompressible, and others as perfectly elastic; whereas it is known that they are strictly neither one nor the other, the law of resistance being differently modified in nearly every different substance; and as it is requisite theoretically to assume some determinate law of action, it necessarily follows that some doubt must also hang over this branch of the subject. It is, however, fortunate that whatever may be the uncertainty on these points, the relative strength of different beams or bolts of the same material, of similar forms and submitted to similar strains, is not thereby affected; so that whatever may be the law which the fibres or particles of a body observe in their resistance to compression or extension, still, from the result of a well conducted series of experiments, the absolute resisting force of beams of similar forms, of the same materials, of any dimensions, submitted to similar strains, may, as far as the mean strength can be depended upon, be satisfactorily deduced. An examination of these different views taken of the subject by different writers will, it is hoped, be found to furnish a reply to the other queries of the Committee. The first writer who endeavoured to connect this inquiry with geometry, and thereby to submit it to calculations, was the venerable Galileo, in his *Dialogues*, published in 1633. He there considers solid bodies as being made up of numerous small fibres placed parallel to each other, and their resistance to separation to a force applied parallel to their length, to be proportional to their transverse area,—an assumption at once obvious and indisputable, abstracting from the defects and irregularities of the materials themselves. He next inquired in what manner these fibres would resist a force applied perpendicularly to their length: and here he assumed that they were wholly incompressible; that the fibres under every degree of tension resisted with the same force, and, consequently, that when a beam was fixed solidly in a horizontal position, with one end in a wall or other immoveable mass, the resistance of the integrant fibres was equal to the sum of their direct resistances multiplied by the distance of the centre of gravity of their section from the lowest point; about which point, according to this hypothesis, the motion must necessarily take place.

The fallacy of these assumptions was noticed, but not corrected, by several subsequent authors. Leibnitz objected to the doctrine of the fibres resisting equally under all degrees of tension, but admitted their incompressibility, thereby still making the motion take place about the lowest point of the section; but he assumed for the law of resistance to extension that it was always proportional to the quantity of extension. Accordingly as the one or the other of these hypotheses was adopted, the computed transverse resistance of a beam, as depending on the absolute strength of its fibres, varied in the ratio of 3 to 2; and many fanciful conclusions have been drawn by different authors relative to the strength of differently formed beams, founded upon the one or the other of these assumptions, which, however, it will be unnecessary to refer to more particularly in this article.

We have seen that each of these distinguished philosophers supposed the incompressibility of the fibres; but James Bernoulli rejected this part of Leibnitz's hypothesis, and considered the fibres as both compressible and extensible, and that the resistance to each force was proportional to the degree of extension or compression. Consequently, the motion instead of taking place, as hitherto considered, about the lowest point of the section, was now necessarily about a point within it; and his conclusion was, that whatever be the position of the axis of motion, or, as it is now commonly called, the neutral axis, the same force applied to the same arm of a lever will always produce the same effect, whether all the fibres act by extension or by compression, or whether only a part of them be extended, and a part compressed. Dr. Robison, in an elaborate article on this subject, also assumes the compressibility and extensibility of the fibres, and as a consequence, assumes the centre of compression as a fulcrum, about which the forces to extension are exerted, and the resistance of both forces to be directly proportional to the degree of compression or extension to which they are exposed; that is, he assumed each force, although not necessarily offering equal power of resistance, to be individually subject to the law of action appertaining to perfectly elastic bodies. In carrying on the experiments which laid the foundation of my *Essay on the Strength of Timber, &c.*, in 1817, I was led by several circumstances I had observed to doubt whether, in the case of timber, this assumption of perfect elasticity was admissible. And as some of the specimens used in my experiments showed very distinctly after the fracture the line about which the fracture took place, I thought of availing myself of this datum, and that which gave the strength of direct cohesion, in order to deduce the law of resistance from actual experiment, instead of using any assumed law whatever.

The result of this investigation implied that the resistance was nearly as first assumed by Galileo, and although very different from what I had anticipated, yet, as an experimental result, I felt bound to abide by it, attributing the discrepance to the imperfect elastic properties of the material. Mr. Hodgkinson, however, in a very ingenious paper read at the Manchester Philosophical Society in 1822, has pointed out an error in my investigation, by my having assumed the momentum of the forces on each side the neutral axis as equal to each other, instead of the forces themselves; consequently the above deduction in favour of the Galilean hypothesis fails. This paper did not come to my knowledge till the third edition of my Essay was nearly printed off, and the correction could not then be made; but being made, it proves that the law of actual resistance approaches much nearer to that of perfect elasticity than from the nature of the materials there could be any reason to expect; so that in cases where the position of the neutral axis is known, and also its resistance to direct cohesion, a tolerably close approximation may be made to the transverse strength of a beam of any form, by assuming the resistance to extension to be proportional to the quantity of extension, and the centre of compression as the fulcrum about which that resistance is exerted. But I have before observed, and beg again to repeat, that by far the most satisfactory data will always be obtained by experiments on beams of the like form (however small the scale,) and of the same material as those to be employed, because then the law of resistance forms no part of the inquiry, and does not necessarily enter into the calculation, the ultimate strengths being dependent on the dimensions only, whatever may be the absolute or relative resistance of the fibres to the two forces we have been considering.

At present I have only considered the resistance of a beam to a transverse strain; but there is another mode of application, in which, again, the law of resistance necessarily enters, and has led to many curious and even mysterious conclusions. This is when a force of compression is applied parallel to the length. In the case of short blocks, the resistance of the material to a crushing force is all that is necessary to be known; and in the *Philosophical Transactions* for 1818 we have a highly valuable table of experimental results on a great variety of materials, by

George Rennie, Esq., which contains nearly all the information on this subject that can be desired. But when a beam is of considerable length in comparison with its section, it is no longer the crushing force that is to be considered: the beam will bend and be ultimately destroyed by an operation very similar to that which breaks it transversely; and the investigation of these circumstances has called forth the efforts of Euler, Lagrange, and some other distinguished mathematicians.

When a cylindric body considered as an aggregate of parallel fibres is pressed vertically in the direction of its length, it is difficult to fix on data to determine the point of flexure, since no reason can be assigned why it should bend in one way rather than in another; still, however, we know that practically such bending will take place. And it is made to appear, by the investigations of Euler and Lagrange, that with a certain weight this ought theoretically to be the case, but that with a less weight no such an effect is produced,—an apparent interruption of the law of continuity not easily explained, which exhibits itself, however, analytically, by the expression for the ordinate of greatest inflection being imaginary till the weight or pressure amounts to a certain quantity. Another mysterious result from these investigations is, that while the column has any definite dimensions, and is loaded with a certain weight, inflection as above stated takes place; but if the column be supposed infinitely thin, then it will not bend till the weight is infinitely great. These investigations of two such distinguished geometers are highly interesting as analytical processes, but the hypothesis on which they are founded, namely, that of the perfect elasticity of the materials, is inconsistent with the nature of bodies employed in practice: they form, therefore, rather an exercise of analytical skill than of useful practical deductions. There is, however, one useful result to be drawn from these processes, which is, that the weight under which a given column begins to bend is directly as its absolute elasticity; so that, having determined experimentally the weight which a column of given elasticity will support safely, or that at which inflection would commence, we may determine the weight which another column of the same dimensions, but of different elasticity, may be charged with without danger.

M. Gerard, a member of the Institute of France, aware of the little practical information to be drawn from investigations wholly hypothetical, has given the detail of a great number of actual experimental results connected with this subject on oak and fir beams of considerable dimensions, carried on at the expense of the French Government, from which he has drawn the following empirical formulæ, viz.—

1. In oak beams $\frac{P f^3}{3\,b} = \frac{11784451\,(f + \cdot 03)\,a\,h^2}{1\cdot 3}$

2. In fir beams $\frac{P f^2}{3\,b} = 8161128\,a\,h^2$;

where P = half the weight in kilogrammes, a the less, and h the greater sides of the section, f half the length of the column, and b the versed sine of inflection, the dimensions being all in metres*.

How far these formulæ are to be trusted in practical constructions is, however, I consider, rather doubtful, because they are drawn from a number of results which differ very greatly from each other; and in one case in particular the result, as referred to the deflection of beams, has been satisfactorily shown to be erroneous by Baron Charles Dupin, in vol. x. of the *Journal de l'E'cole Polytechnique*, as also by a carefully conducted series of experiments in my *Essay on the Strength of Timber, &c.* I conceive it, therefore, to be very desirable that a set of experiments on this application of a straining force on vertical columns should be undertaken, and it is, perhaps the only branch of the inquiry connected with the strength of materials in which there is a marked deficiency of practical data; at the same time it is one in which both timber and iron are being constantly employed. We see every day in the metropolis houses of immense height and weight being built, the whole fronts of which, from the first floors, are supported entirely by iron or wooden columns; and all this is done without any practical rule that can be depended upon for determining whether or not these columns are equal to the duty they have to perform.

I say this with a full knowledge that Mr. Tredgold has furnished an approximate rule for this purpose; but the principle on which it is founded has no substantial basis. The extraordinary skill which Mr. Tredgold possessed in every branch of this subject, and the great ingenuity he has displayed in investigating and simplifying every calculation connected with architectural and mechanical construction, certainly entitle his opinion to high consideration; but still on a subject of such high importance, it would be much more satisfactory to be possessed of actual experimental data. The supposition he advanced was made entirely as a matter of necessity, and I am

* See *Traité Analytique de la Résistance des Solides.*

confident that no one would have been more happy than himself to have been enabled to substitute fact for hypothesis, had he possessed the means of adopting the former. But unfortunately such a series of experiments are too expensive and laborious to be undertaken by an individual situated as he was, having a family to maintain by his industry, and whose close and unremitting application to these and similar inquiries, in all probability shortened his valuable life*.

At present I have referred principally to experiments made with a view of determining the ultimate strength of materials; and with data thus obtained practical men have been enabled to pursue their operations with safety, by keeping sufficiently within the limits of the ultimate strain the materials would bear, or rather with which they would just break, some working to a third, others to a fourth, &c., of the ultimate strength, according to the nature of the construction, or the opinion of the constructor.

But it is to be observed, that although we may thus ensure perfect safety as far as relates to absolute strength, there are many cases in which a certain degree of deflection would be very injurious. It is therefore highly necessary to attend also to this subject, particularly as the deflection of beams and their ultimate strength depend upon different principles, or are at least subject to different laws. Hence most writers of late date give two series of values, one exhibiting the absolute or relative strength, and the other the absolute or relative elasticities. These values will of course be found to differ in different authors, on account of the uncertainty in the strength of the materials already referred to, but amongst recent experiments the difference is not important: they will also be found differently expressed, in consequence of some authors deducing these numbers from experiments differently made. Some, for example, have drawn their formulæ for absolute strength from experiments made on beams fixed at one end and loaded at the other, using the whole length; some, again, from experiments on beams supported at each end and loaded in the middle, using the half length. Some take the length in feet, and the section in inches; others all the dimension in inches; and a similar variety occurs in estimating the elasticity. Also, in the latter case, some authors employ what is denominated the modulus of elasticity, in which latter case the weight of the beam

* Mr. Tredgold's *Principles of Carpentry*, and his *Treatise on the Strength of Iron*, ought to be in the possession of every practical builder; besides which two works, he published many separate articles on the same subject in different numbers of the *Philosophical Magazine*.

itself, and consequently its specific gravity, enters. These varieties of expressions, however, are not to be understood as arising from any difference of opinion amongst the authors from whom they proceed, but merely as different modes of expressing the same principles: indeed, in reply to that inquiry of the Committee with reference to this point, I may, I think, venture to say there is not at present any difference of opinion on any of the leading principles connected with the strength of materials, with the exception of such as are dependent entirely upon the imperfect nature of the materials themselves, and which, as we have seen, will give rise to different results in the hands of the same experimenter and under circumstances in every respect similar.

As I distinguish the doctrine of the absolute resistance or strength of materials, which is founded on experiment, from that which relates to the amount and resolution of the forces or strains to which they are exposed, which is geometrical; and as I confine myself to the former subject only in this Essay, it is not, I conceive, necessary to extend the preceding remarks to any greater length. I shall therefore conclude by giving a table of the absolute and relative values of the ultimate strength and elasticity of various species of timber and other materials, selected from those results in which I conceive the greatest reliance may be placed.

Formulæ relating to the ultimate Strength of Materials in cases of Transverse Strain.—Let l, b, d, denote the length, breadth and depth in inches in any beam, w the experimental breaking weight in pounds, then will $\frac{l w}{b d^2} = S$ be a constant quantity for the same material, and for the same manner of applying the straining force; but this constant is different in different modes of application. Or, making S constant in all cases for the same material, the above expression must be prefixed by a coefficient, according to the mode of fixing and straining.

1. When the beam is fixed at one end, and loaded at the other,

$$\frac{l w}{b d^2} = S.$$

2. When fixed the same, but uniformly loaded,

$$\frac{1}{2} \times \frac{l w}{b d^2} = S.$$

3. When supported at both ends, and loaded in the middle,

$$\frac{1}{4} \times \frac{l w}{b d^2} = S.$$

4. Supported the same, and uniformly loaded,

$$\frac{1}{8} \times \frac{l\,w}{b\,d^2} = S.$$

5. Fixed at both ends, and loaded in the middle,

$$\frac{1}{6} \times \frac{l\,w}{b\,d^2} = S.$$

6. Fixed the same, but uniformly loaded,

$$\frac{1}{12} \times \frac{l\,w}{b\,d^2} = S.$$

7. Supported at the ends, and loaded at a point not in the middle. Then, $n\,m$ being the division of the beam at the point of application,

$$\frac{n\,m}{l^2} \times \frac{l\,w}{b\,d^2} = S.$$

Some authors state the coefficients for cases 5 and 6 as $\frac{1}{8}$ and $\frac{1}{16}$, but both theory and practice have shown these numbers to be erroneous.

By means of these formulæ, and the value of S, given in the following Table, the strength of any given beam, or the beam requisite to bear a given load, may be computed. This column, however, it must be remembered, gives the ultimate strength, and not more than one third of this ought to be depended upon for any permanent construction.

Formulæ relating to the Deflection of Beams in cases of Transverse Strains.—Retaining the same notation, but representing the constant by E, and the deflection in inches by δ, we shall have,

Case 1.	$\frac{32}{1} \times \frac{l^3\,w}{b\,d^3\,\delta} = E.$	Case 4.	$\frac{5}{8} \times \frac{l^3\,w}{b\,d^3\,\delta} = E.$
2.	$\frac{12}{1} \times \frac{l^3\,w}{b\,d^3\,\delta} = E.$	5.	$\frac{2}{3} \times \frac{l^3\,w}{b\,d^3\,\delta} = E.$
3.	$\frac{1}{1} \times \frac{l^3\,w}{b\,d^3\,\delta} = E.$	6.	$\frac{5}{12} \times \frac{l^3\,w}{b\,d^3\,\delta} = E.$

Hence, again, from the column marked E in the following Table, the deflection a given load will produce in any case may be computed; or, the deflection being fixed, the dimension of the beam may be found. Some authors, instead of this measure of elasticity, deduce it immediately from the formula $\frac{l^3 w}{3\, b\, d^2\, \delta} = \text{E}$, substituting for w the height in inches of a column of the material, having the section of the beam for its base, which is equal to the weight w, and this is then denominated the modulus of elasticity. It is useful in showing the relation between the weight and elasticity of different materials, and is accordingly introduced into the following Table.

The above formulæ embrace all those cases most commonly employed in practice. There are, of course, other strains connected with this inquiry, as in the case of torsion in the axles and shafts of wheels, mills, &c., the tension of bars in suspension bridges, and those arising from internal pressure in cylinders, as in guns, water-pipes, hydraulic presses, &c.; but these fall rather under the head of the resolution of forces than that of direct strength. It may just be observed, that the equation due to the latter strain is

$$t\,(c - n) = n\,\text{R},$$

where t is the thickness of metal in inches, c the cohesive power in pounds of a square inch rod of the given material, n the pressure on a square inch of the fluid in pounds, and R the interior radius of the cylinder in inches. Our column marked C will apply to this case, but here again not more than one third the tabular value can be depended upon in practice.

Table of the Mean Strength and Elasticity of various Materials, as deduced from the most accurate Experiments.

Names of Materials.	Specific Gravity.	C. Mean strength of cohesion on an inch section.	$S=\frac{lw}{4bd^2}$ Constants for transverse strains.	$E=\frac{l^3w}{bd^3\delta}$ Constants for deflections.	Modulus of Elasticity.	Remarks.
Woods.		lbs.				
Acacia	710	......	1800	4609000	3739000	of English growth.
Ash	760	17000	2026	6580000	4988000	ditto.
Beech	700	11500	1560	5417000	4457000	ditto.
Birch, Common	700	......	1900	6570000	5406000	ditto.
——, American Black	750	......	1500	5700000	3388000	American.
Box	1000	20000				
Bullet Tree	1030	......	2650	10512000	5878000	Berbice.
Cabacully	900	......	2500	7437000	4759000	ditto.
Deal, Christiana	680	11000	1550	6350000	5378000	
——, Memel	590	11000	1730	6420000	6268000	
Elm	540	5780	1030	2803000	3007000	English.
Fir, New England	550	12000	1100	5967000	6249000	
—, Riga	750	12600	1130	5314000	4080000	
—, Mar Forest	700	12000	1140	3400000	2797000	Scotland.
Green heart	1000	......	2700	10620000	6118000	Berbice.
Larch, Scotch	540	7000	1120	4200000	4480000	
Locust Tree	950	20580	3400	767000	4649000	America, South.
Mahogany	637	8000				
Norway Spars	580	12000	1470	5830000	5789000	
Oak, English from	700	9000	1200	3490000	2872000	Results very variable.
Oak, English to	900	15000	2260	7000000	47020000	
——, African	980	14400	2000	9500000	55830000	
——, Adriatic	990	14000	1380	3880000	2257000	
——, Canadian	872	12000	1760	8590000	5674000	
——, Dantzic	760	14500	1450	4760000	3607000	
Pear Tree	646	9800				
Poon	600	14000	2200	6760000	6488000	East Indies.
Pine, Pitch	660	10500	1630	5000000	4364000	
——, Red	660	10000	1340	7360000	6423000	
Teak	750	15000	2460	9660000	7417000	East Indies.
Tonquin Bean	1050	......	2700	10620000	5826000	Berbice.
Iron.						
Iron, Cast from	7200	16300	8100	69120000	5530000	Mean of English and Foreign.
Iron, Cast to	......	36000				
——, Malleable	7760	60000	9000	91440000	6770000	
——, Wire	......	80000	......	....	....	

Report on the State of our Knowledge respecting the Magnetism of the Earth. By S. Hunter Christie, *Esq., M.A., F.R.S. M.C.P.S., Corr. Memb. Philom. Soc. Paris, Hon. Memb. Yorkshire Phil. Soc.; of the Royal Military Academy; and Member of Trinity College, Cambridge.*

Had the discovery of the loadstone's directive power been made by a philosopher who at the same time pointed out its importance to the purposes of navigation, we might expect that his name would have been handed down to posterity as one of the greatest benefactors of mankind. The discovery was, however, most likely made by one so engaged in maritime enterprise that, in his eyes, this application constituted its whole value; and it is not improbable that, being for some time kept secret, it may have been the principal cause of the success of many enterprises attributed to the superior skill and bravery of the leaders. The knowledge of this property of the magnet, though gradually diffused, would long be guarded with jealousy by those who justly viewed it as of the highest advantage in their predatory or commercial excursions; and this is, perhaps, the cause of the obscurity in which the subject is veiled. If the discovery is European, there is no people, from the character of their early enterprises, and, I may add, from the nature of the rocks of their country, more likely to have made it than the early Norwegians; and as there is reason for believing that they were acquainted with the directive property of the loadstone at least half a century earlier than its use is supposed to have been known in other parts of Europe, it may be but justice to allow them the honour of having been the discoverers. Whether the discovery was made in Asia or in Europe, in the North or in the South, I am not, however, now called upon to decide, but to point out the consequences which have followed that discovery by unveiling gradually phænomena, though less striking, yet equally interesting, and some even more difficult of explanation.

These phænomena are, the variation of the magnetic needle, with its annual and diurnal changes; the dip of the needle; and the intensity of the magnetic force of the earth; which are, however, all comprised under two heads,—The Direction and the Intensity of the terrestrial magnetic force.

I. *The Direction of the Terrestrial Magnetic Force.*

1. *The Variation of the Needle.*—For some centuries after the directive property of the loadstone was discovered, it was generally supposed that the needle pointed correctly towards the pole of the heavens. It has however been said, on the authority of a letter by Peter Adsiger, that the variation of the needle was known as early as 1269; and if we fully admit the authenticity of this letter, we must allow that the writer was at that date not only aware of the fact, but that he had observed the extent of the deviation of the needle from the meridian*. It is possible that such an observation as this may have been made at this early period by an individual devoting his time to the examination of magnetical phænomena;

* This curious and highly interesting letter, dated the 8th of August 1269, is contained in a volume of manuscripts in the Library of the University of Leyden, and we are indebted to Cavallo for having published extracts from it. The variation is thus referred to: "Take notice that the magnet (stone), as well as the needle that has been touched (rubbed) by it, does not point exactly to the poles; but that part of it which is reckoned to point to the south declines a little to the west, and that part which looks towards the north inclines as much to the east. The exact quantity of this declination I have found, after numerous experiments, to be five degrees. However, this declination is no obstacle to our guidance, because we make the needle itself decline from the true south by nearly one point and an half towards the west. A point, then, contains five degrees." (Letter of Peter Adsiger, Cavallo *On Magnetism*, London 1800, p. 317.) It is certainly extraordinary, if so clear an account of the deviation of the needle from the meridian as this, was communicated to any one by the person who had himself observed that deviation, that for more than two centuries afterwards we should have no record of a second observation of the fact. This alone would throw doubt on the authenticity of the letter, and the estimate given of the variation may appear to confirm these doubts; for, according to the period of change which best agrees with the observations during more than two hundred years, the variation, if observed, would have been found to be westerly instead of easterly in 1269. It may however be urged, that as the whole period of change has not yet elapsed since observations were made, we are not in possession of a sufficient number of facts to authorize us to draw conclusions respecting the variation at such an early date; and also, that if the letter be spurious, or the original date have been altered to that which it bears, this or the fabrication can only have been for the purpose of founding claims in consequence of the contents of this letter; and as no such claims have been advanced, there appears no motive either for fabrication or alteration. In a preceding part of the letter the author gives methods for finding the poles of a loadstone; and certainly the direction of the axis could not be determined to within five degrees by either of these; so that, as regards the loadstone, we may, I think, conclude that the author did not make the observation. As a matter of curious history connected with magnetism, it is desirable that either the authenticity of this letter should be clearly established, or reasons given for doubting it, by those who have an opportunity of consulting the original.

and as it is probable that for some time subsequent to the discovery of the directive property of the needle the deviation in Europe was not of sufficient magnitude to have been easily detected by means of the rude instruments then in use, it may very likely be owing to this circumstance that we have not earlier records of the variation*. That Columbus, the most scientific navigator of his age, when he commenced his career of discovery, and undertook to show the western route to India, was not aware of it, is clear, since the discovery during his first voyage has been attributed to him. However, although Columbus may have noticed that the needle did not in every situation point due north, and Adsiger, long before him, may even have rudely obtained the amount of its deviation, the first observations of the variation on which any reliance can be placed appear to have been made about the middle of the sixteenth century, and shortly afterwards it was well known that the variation is not the same in all places†.

2. *Change in the Direction of the Needle.*—When it was first determined by observation, about 1541, that the needle did not point to the pole of the earth, it was found that this variation from the meridian, at Paris, was about 7° or 8° towards the east. In 1550 it was observed 8° or 9° east; and in 1580, $11\frac{1}{2}$° east. Norman appears to have been the first who observed the variation with any degree of accuracy in London. He states that he observed it to be 11° 15′ east‡, but he was not aware that it does not remain constant in the same place§. In 1580, Burough found the variation at Limehouse to be $11\frac{1}{4}$° or $11\frac{1}{3}$° east||, and his observations appear to be

* Another reason why the variation was not earlier observed may be that the natural magnet was first used for the purposes of navigation, and its directive line was that which pointed to the pole star. As it was therefore considered that the natural magnet indicated the direction of the meridian, and it was found that a needle touched by it had the directive power, when the needle was introduced it was assumed that this also pointed in the meridian.

† *The New Attractive*, by Robert Norman, chap. ix. London 1596.

‡ *Ibid.* No date is given for this observation; but from the circumstance of Burough referring to Norman's book in the preface to his *Discourse of the Variation of the Compasse*, dated 1581, (the copy of this to which I have access was printed in 1596, but the Bodleian Library contains one printed in 1581,) it would appear that there must have been an earlier edition of Norman's book than that of 1596, and that his observations must have been made before 1581. Bond, *Philosophical Transactions*, vol. viii. p. 6066, gives 1576 as the date of Norman's observations.

§ "And although this variation of the needle be found in travaile to be divers and changeable, yet at anie land or fixed place assigned, it remaineth alwaies one, still permanent and abiding." *New Attractive*, chap. ix.

|| The mean of his observations, which do not differ 20′, is 11° 19′ east.

entitled to much confidence; but he was of the same opinion as Norman with respect to the constancy of the variation*. Gunter, in 1612, found the variation in London to be 5° 36′ east; and Gellibrand, in 1633, observed it 4° 4′ east. Dr. Wallis considers Gellibrand to have been the discoverer of "the variation of the variation†;" but if Gunter had any confidence in his own observations and those of Burough, he must have been aware of the change in the variation. In 1630, Petit found the variation at Paris to be 4½° east, but suspected, at the time, that the earlier observations there had been incorrect; and it was not until 1660, when he found the variation to be only 10′ east, that he was satisfied of the change of the variation. About ten years later, Azaut, at Rome, where the variation had been observed 8° east, found it to be more than 2° west; and Hevelius, who at Dantzick in 1642 had found it to be 3° 5′ west, now found it to be 7° 20′ west.

3. *Diurnal Change in the Variation.*—This was discovered in 1722 by Graham, to whose talents and mechanical skill science is so deeply indebted. He found that with several needles, on the construction of which much care had been bestowed, the variation was not always the same; and at length determined that the variation was different at different hours in the day, the greatest westerly variation occurring between noon and four hours after, and the least about six or seven o'clock in the evening‡. Wargentin at Stockholm in 1750, and Canton in London from 1756 to 1759, more particularly observed this phænomenon; and the latter determined that the time of minimum westerly variation in London was between eight and nine in the morning, and the time of maximum between one and two in the afternoon. Canton likewise determined in 1759, that the daily variation was different at different times in the year, the maximum change occurring about the end of June, and the minimum in December§. Cassini, during more than five years and a half, namely, from May 1783 to January 1789, carefully observed, at particular hours, the direction of a needle suspended in the Observatory at Paris; and although he does not correctly state the course of the daily variation, overlooking altogether the second maximum west, and the progress of the needle towards the east in the early part of the

* "For considering it remayneth alwaies constant without alteration in every severall place, there is hope it may be reduced into method and rule." *Discourse*, chap. x.

† *Philosophical Transactions*, 1701, vol. xxii. p. 1036.

‡ *Ibid.* 1724, vol. xxxiii. p. 96.

§ *Ibid.* 1759, vol. xli. p. 398.

morning*, yet his observations and remarks are of great value as pointing out the annual oscillations of the needle†. Since this, the diurnal variation has been very generally observed, but by no one with greater care and perseverance than by the late Colonel Beaufoy‡.

In order to determine whether the course of the diurnal variation is influenced by the elevation of the place of observation, the zealous and indefatigable De Saussure undertook a series of observations on the Col du Géant, nearly 11,300 feet above the level of the sea. This series, after incurring much personal inconvenience and even risk in that region of snow and of storms, he completed; and he has compared the results with observations which he made immediately before and after at Chamouni and Geneva. From this comparison it appears that the course of the diurnal variation was nearly the same on one of the highest mountains, in a deep and narrow valley at its foot, and in the middle of a plain or of a large valley. The times of the maxima, east and west, are in each case nearly those previously determined by Canton, these maxima occurring rather later on the Col du Géant than at the other stations. Excluding in all cases the results where extraordinary causes appear to have operated, the extent of the diurnal variation at Chamouni exceeds that at Geneva and also that on the Col, the two latter being very nearly the same. The observations, however, are, as Saussure very justly remarks, much too limited to give correct means§.

5. *The Dip of the Magnetic Needle.*—Norman having found with different needles, and with one in particular on the construction of which he had bestowed much pains, that although perfectly balanced on the centre previously to being touched by the magnet, after this operation the north end always declined below the horizon, devised an instrument by which he

* *Journal de Physique*, Mai 1792, tom. xl. p. 345. † *Ibid.* p. 348.

‡ Many of the results of Colonel Beaufoy's observations are published in the *Edinburgh Philosophical Journal*, vols. i. ii. iii. iv. and vii.

§ Saussure, *Voyages dans les Alpes*, tom. iv. p. 302 au p. 312. As Saussure does not give the mean results, I insert them here.

	Time of absolute maximum.		Time of second maximum.		Extent of diurnal change.	Elevation above the sea.
	East.	West.	East.	West.		
	h m	h m	h m	h m	′ ″	Feet.
Geneva	7 56 A.M.	1 09 P.M.	6 26 P.M.	11 17 P.M.	15 42	1305
Chamouni ...	7 34	1 41	7 44	10 46	17 06	3453
Col du Géant	8 09	2 00	5 51	10 17	15 43	11274

could determine the inclination of the needle to the plane of the horizon*. The figure given of the instrument is sufficiently rude, but the principles of its construction, as stated by Norman, are correct. With this instrument he found the inclination of the needle to the horizon in London to be about 71° 50′, but gives no date to the observation, though Bond assigns 1576 as the time†. Although in a theoretical point of view it would be desirable to have so early a record of the dip, particularly as subsequent observations lead us to suppose that the dip attained its maximum after this time, yet, considering the uncertainty attending such observations, even with the present improved instruments, we cannot place much confidence in this result, however we may rely upon the author having used every precaution in his power to ensure accuracy. Having determined the dip of the needle in London, Norman states that this declining of the needle will be found to be different at different places on the earth‡, though he does not take a correct view of the subject, for he considers that the needle will always be directed towards a fixed point.

5. *Variation of the Dip.*—Subsequent observations by Bond, Graham, Cavendish, and Gilpin, and the more recent ones in our own time, have shown that the inclination of the needle to the horizon at the same place, like the angle which it makes with the meridian, is subject to change; but the diurnal oscillations of the direction are of too minute a character to have been ascertained with the imperfect instruments which we possess.

This is an outline of the phænomena hitherto observed, depending upon the direction of the forces acting upon the needle. Various attempts have been made to account for those observable at fixed points on the earth's surface at different periods, and also to connect those depending on the different positions of the places of observation, but hitherto with only very partial success. It is not my intention to enter into a detailed history of these attempts, but I may briefly notice some of the most remarkable.

To Gilbert we are indebted not only for the first clear views of the principles of magnetism, but of their application to the phænomenon of the directive power of the needle; and indeed we may say that, with the exception of the recent discoveries, all that has been done since, in magnetism, has for its foundation the principles which he established by experiment§. He con-

* *New Attractive*, chap. iii. iv.

† *Philosophical Transactions*, 1673, vol. viii. p. 6066.

‡ *New Attractive*, chap. vii.

§ Gilbert, *De Magnete, &c.* Lond. 1600.

sidered that the earth acts upon a magnetized bar, and upon iron, like a magnet, the directive power of the needle being due to the action of magnetism of a contrary kind to that at the end of the needle directed towards the pole of the earth. He applied the term "pole" to the ends of the needle directed towards the poles of the earth, according to the view he had taken of terrestrial magnetism, designating the end pointing towards the north, as the south pole of the needle, and that pointing towards the south, as its north pole *. It is to be regretted that some English philosophers, guided by less correct views, have since his time applied these terms in the reverse sense, which occasionally introduces some ambiguity, though now they are used in this country, as on the Continent, in the sense originally given to them by Gilbert.

In 1668 Bond published a Table of computed variations in London, for every year, from that time to the year 1716 †. The variations in this Table agree nearly with those afterwards observed for about twenty-five years, beyond which time they differ very widely; and I only notice this Table as the first empirical attempt at the solution of a problem which is, as yet, unsolved. Bond afterwards proposed to account for the change in the variation and dip of the needle by the motion of two magnetic poles about the poles of the earth. He professed not only to give the period of this motion, but to be able to point out its cause, and even proposed to determine the longitude by means of the dip ‡. He, however, did not make public either his methods or his views; but with regard to the longitude, it is probable they were the same as those afterwards adopted by Churchman.

Halley considered that the direction of the needle at different places on the earth's surface might be explained on the supposition that the earth had four magnetic poles §, and that the change in the direction at the same place was due to the motion of two of these poles about the axis of the earth, the other two being fixed. He does not enter into any calculations to show the accordance of the phænomena with such an hypothesis, but conjectures that the period of revolution of these poles is about 700 years ||.

Since this time, calculations have been made by various authors, both on the hypothesis of two magnetic poles and on that of four, with the view of comparing the results of these

* Gilbert, *De Magnete, &c.*, lib. i. cap. iv.

† *Philosophical Transactions*, 1668, vol. iii. p. 789.

‡ *Ibid.* 1673, vol. viii. p. 6065.

§ *Ibid.* 1683, vol. xiii. p. 208.

|| *Ibid.* 1692, vol. xvii. p. 563.

hypotheses with actual observation. The most recent attempt of this kind is that by Professor Hansteen. He adopts Halley's hypothesis of four magnetical poles, but considers that they all revolve, and in different periods, the northern poles from west to east, and the southern ones from east to west. The results calculated on this hypothesis agree pretty nearly with the observations with which they are compared; but as considerable uncertainty attends magnetical observations, excepting those of the variation made at fixed observatories, and especially the early ones of the dip and variation, on which the periods of the poles and their intensities must so much depend, it would certainly be premature to say that such an hypothesis satisfactorily explains the phænomena of terrestrial magnetism. If we admit that the progressive changes which take place in the direction of the needle are due to the rotation of these poles, we must look to the oscillations of the same poles for the cause of the diurnal oscillation of the needle. Any hypothesis which by means of two or more magnetic poles will thus connect the phænomena of magnetism, is of great advantage, however unable we may be to give a reason for the particular positions of the poles, or for their revolution. Hansteen refers these to the agency of the sun and moon.

Without assigning any cause either for the direction of the needle, or for the progressive change of that direction, attempts have been made to account for its diurnal oscillations. But before taking a review of these, it is necessary that I should state more particularly the precise nature of the phænomenon. This I cannot do better than by referring to the results deduced from Canton's observations*. From these it appears that in London, during the twenty-four hours, a double oscillation of the needle takes place, the absolute maximum west happening about half-past one in the afternoon, and the absolute maximum east, that is, the minimum west, about nine in the morning; besides which there was another maximum east about nine in the evening, and a maximum west near midnight or very early in the morning, the two latter maxima being small compared with the absolute maxima. Colonel Beaufoy's very extensive series of observations, made when the variation was between 24° and 25° west, (Canton's having been made when it was 19°,) give nearly the same results, the absolute maxima happening somewhat earlier, and the second maxima west about eleven in the evening.

Canton explained the westerly motion of the needle in the

* *Philosophical Transactions*, 1759, p. 398, and 1827, pp. 333, 334.

latter part of the morning, and the subsequent easterly motion, by supposing that the heat of the sun acted upon the northern parts of the earth as upon a magnet, by weakening their influence, but offered no explanation of the morning easterly motion of the needle.

Oersted's discovery of the influence of the closed voltaic circuit upon the magnetic needle, and the consequent discoveries of Davy, Ampère and Arago, immediately led to the consideration, whether all the phænomena of terrestrial magnetism were not due to electric currents; and the discovery of Seebeck, that electric currents are excited when metals having different powers of conducting heat are in contact,—which discovery with but few holds the rank to which it is eminently entitled,—pointed to a probable source for the existence of such currents. At the conclusion of a highly interesting paper on the development of electro-magnetism by heat, Professor Cumming remarks that "magnetism, and that to a considerable extent, it appears, is excited by the unequal distribution of heat amongst metallic, and possibly amongst other bodies. Is it improbable that the diurnal variation of the needle, which follows the course of the sun, and therefore seems to depend upon heat, may result from the metals, and other substances which compose the surface of the earth, being *unequally* heated, and consequently suffering a change in their magnetic influence?" And in the second part of a paper, detailing some thermo-magnetical experiments, read before the Royal Society of Edinburgh, Dr. Traill considers "that the disturbance of the equilibrium of the temperature of our planet, by the continual action of the sun's rays on its intertropical regions, and of the polar ices, must convert the earth into a vast thermo-magnetic apparatus:" and "that the disturbance of the equilibrium of temperature, even in stony strata, may elicit some degree of magnetism*." Previous to this, I had adopted the opinion that temperature, if not the only cause, is the principal one of the daily variation†. It did not, however, appear to me, that any of the experiments hitherto made bore directly on the subject, since the metals producing electric currents by their unequal conduction of heat were only in contact at particular parts, and in no case had such currents been excited by different metals having their surfaces symmetrically united throughout. I in consequence instituted a series of experiments with two metals so united, and found that electric currents were still excited on the

* *Transactions of the Philosophical Society of Cambridge*, vol. ii. p. 64.

† *Philosophical Transactions*, 1823, p. 392.

application of heat, the phænomena corresponding to magnetic polarization in a particular direction with reference to the place of greatest heat*. From these experiments I drew the conclusion that one part of the earth, with the atmosphere, being more heated than another, two magnetic poles, or rather electric currents producing effects referrible to such poles, would be formed on each side of the equator, poles of different names being opposed to each other on the contrary sides of the equator; and that different points in the earth's equator becoming successively those of greatest heat, these poles would be carried round the axis of the earth, and would necessarily cause a deviation in the horizontal needle†. On comparing experimentally the effects that would result from the revolution of such poles with the diurnal deviations at London, as observed by Canton and Beaufoy, also with those observed by Lieut. Hood at Fort Enterprise, and finally with the late Captain Foster's at Port Bowen, I found a close agreement in all cases in the general character of the phænomena, and that the times of the maxima east and west did not differ greatly in the several cases. The double oscillation of the needle, to which I have referred in Canton's and Beaufoy's observations, clearly resulted from this view of the subject. Some of the experiments to which I have referred showed that when heat was applied to a globe, the electric currents excited were such, that on contrary sides of the equator the deviations of the end of the needle of the same name as the latitude were at the same time always in the same direction, either both towards east or both towards west. No observations having at that time been made on the diurnal variation of the needle in a high southern latitude, I considered "that the agreement of the theoretical results with such observations would be almost decisive of the correctness of the theory." Captain Foster's observations at Cape Horn, South Shetland, and the Cape of Good Hope, show most decidedly that in the *southern* hemisphere the diurnal deviations of the *south* end of the needle correspond very precisely with those of the *north* end in the *northern* hemisphere; and most fully bear me out in the view which I had taken. These valuable observations have been placed in my hands by His Royal Highness the President, and the Council of the Royal Society, and I intend, when I have sufficient leisure, rigidly to compare them, and likewise those to which I have already referred in the northern hemisphere, with the diurnal deviations that would

* "Theory of the Diurnal Variation of the Magnetic Needle," *Philosophical Transactions*, 1827, pp. 321, 326.

† *Ibid.* pp. 327, 328.

result at the corresponding places on the earth's surface, on the supposition that such electric currents as I have supposed are excited on contrary sides of the equator, in consequence of different parts on the earth's surface becoming successively the places of greatest heat, during its revolution upon its axis. Should there be found in these results that accordance which I have reason to expect, there will, I think, be no doubt that the diurnal deviation of the needle is due to electric currents excited by the heat of the sun.

I have already adverted to the hypotheses of two or more poles, by means of which attempts have been made to explain the phænomena of terrestrial magnetism, and I may now remark, that if we admit the existence of such poles, we must be careful not to consider the magnetic meridians as great circles: they are unquestionably curves of double curvature. Nor must we consider these poles to be, like the poles of a magnet, centres of force not far removed from the surface. If such centres of force exist for the whole surface of the earth, the experimental determinations of the magnetic force at different places, to which I shall shortly advert, at least show that they cannot be far removed from the centre of figure.

In the delineation of magnetic charts, more attention has hitherto been paid to the Halleyan lines, or lines of equal variation, than to any others; and I am not disposed to undervalue charts where such lines alone are exhibited: to the navigator they are of the greatest value; but they throw little light on the phænomena in general. If the meridians were correctly represented, they would at least indicate clearly their points of convergence, if such in all cases exist; but the lines that would be most likely to guide us to a true theory of terrestrial magnetism, are the normals to the direction of the needle. If, as is highly probable, the direction of the needle is due to electric currents circulating either in the interior or near the surface of the earth, these normals would represent the intersection of the planes of the currents with the surface of the earth; and, by their delineation, we should have exhibited in one view the course of the currents and the physical features by which that course may be modified, so that any striking correspondences which may exist, would be immediately seized, and lead to important conclusions. Changes of temperature I consider to be the principal cause of the diurnal changes in the direction of the needle: and if any connexion exist between these electric currents and climate, we are to expect that the curvature of these normal lines will be influenced by the forms, the extent and direction of the continents or seas over which they pass, and also by the height, direction and extent of chains of mountains, and probably by their geological structure.

These normal lines may, to a certain extent, agree with the lines of equal dip, which have already been delineated upon some charts. In Churchman's charts they are represented in the positions they would have on Euler's hypothesis of the earth having two magnetic poles. The only use, however, of such hypothetical representations is, that by comparison with actual observation they become tests of the correctness of the theory, or they may point out the modifications which it requires, in order that it may accord with observation. In Professor Hansteen's chart the lines of equal dip are projected from observations reduced to the year 1780. Considering how very deficient we are, even now, in correct observations of the dip, I should not be disposed to place much reliance upon the accuracy of these lines, particularly where they cross great extents of sea affording no points of land necessary for observations of the dip. Of these lines of equal dip the most important is the magnetic equator, or that line on the earth at which the dipping needle would be horizontal. The observations giving this result can of course be but few, and are therefore very inadequate for the correct representation of this line. In order to obviate this difficulty, M. Morlet made use of all observations not very remote from the equator, determining the distance of that line from the place of observation by means of the law, that the tangent of the magnetic latitude is half the tangent of the dip, which is derived from the hypothesis of two magnetic poles near to the centre of the earth. By this means the position of the equator was determined throughout its whole extent; and a surprising agreement was found between the determinations of each point by means of different observations, which shows that, within certain limits near the equator, the hypothesis very correctly represents the observations. This line exhibits inflections in its course which have been attributed, and probably with justice, to the physical constitution of the surface in their vicinity*. It has been considered also that a general resemblance exists between the isothermal lines and the lines of equal dip on the surface of the earth†.

All the lines to which I have here referred have been hitherto represented on a plane, either on the stereographical, the globular, or Mercator's projection. Mr. Barlow has, however, very lately represented the lines of equal variation on a globe, from a great mass of the most recent documents connected with

* Biot, *Traité de Physique*.

† Hansteen, *Edinburgh Philosophical Journal*, vol. iii. p. 127.

the variation, furnished to him by the Admiralty, the East India Company, and from other sources. If to the lines of equal variation were added the magnetic meridians and their normals, the isodynamic lines, with those of equal dip, such a globe would form the most complete representation of facts connected with terrestrial magnetism that has ever been exhibited, and might indicate relations which have hitherto been overlooked.

Having discovered that a peculiar polarity is imparted to iron by the simple act of rotation, I was led to consider whether the principal phænomenon of terrestrial magnetism is not, in a great measure, due to its rotation. The subsequent discovery by Arago, that analogous effects take place during the rotation of all metals, and Faraday's more recent discovery, that electrical currents are not only excited during the motion of metals, but that such currents are transmitted by them, render such an opinion not improbable. It is, however, to be remarked, that, in all these cases, motion alone is not the cause of the effects produced; but that these effects are due to electricity induced in the body by its motion in the neighbourhood of a magnetized body. If, then, electrical currents are excited in the earth in consequence of its rotation, we must look to some body exterior to the earth for the inducing cause. The magnetic influence attributed by Morichini and Mrs. Somerville to the violet ray, and the effect which I found to be produced on a magnetized needle when vibrated in sunshine, and which appeared not to admit of explanation without attributing such influence to the sun's rays, might appear to point to the sun as the inducing body. The experiments, however, of Morichini and Mrs. Somerville, have not succeeded on repetition; and in a recent repetition of my own experiments, in a vacuum, by Mr. Snow Harris, the effects which I observed were not detected. I had found that the effects produced on an unmagnetized steel needle differed from those produced on a similar needle when magnetized, and therefore considered that the idea of these effects being independent of magnetism was precluded; but Mr. Harris's results may possibly be considered to indicate that they were due solely to currents of air excited by the sun's rays. These circumstances render it doubtful whether the sun's rays possess any magnetic influence independent of their heating power; but besides this, supposing such influence to exist, if electric currents were induced in the earth during its rotation, they would be nearly at right angles to the equator, and would therefore cause a magnetized needle to place itself nearly perpendicular to the meridians, or parallel to the equator.

Although it would therefore appear that the rotation of the earth is not the cause of its magnetism, yet it is highly probable, from Mr. Faraday's experiments *, that, magnetism existing in the earth independently of it, electrical currents may be produced, not only by the earth's rotation, but by the motion of the waters on its surface, and even by that of the atmosphere; so that the direction and intensity of the magnetic forces would be modified by the influence of these currents.

This subject is at present involved in obscurity: still, considering how many have been the discoveries made within a few years,—all bearing more or less directly upon it, though none afford a complete explanation of the phænomena,—it does not appear unreasonable to expect that we are not far removed from a point where a few steps shall place us beyond the mist in which we are now enveloped.

Mr. Fox, having observed effects attributable to the electricity of metalliferous veins, appears disposed to refer some of the phænomena of terrestrial magnetism to electrical currents existing in these veins †; but although we should not be warranted in denying the existence of these currents, independently of the wires made use of in Mr. Fox's experiments, or even their influence on the needle, yet I think we should be cautious in drawing conclusions from these experiments ‡.

II. *Intensity of the Terrestrial Magnetic Force.*

I have as yet said little on the intensity of the terrestrial magnetic forces. Graham, after having discovered the daily variation of the needle, suspected that the force which urges it varies not only in direction, but also in intensity. He made a great variety of observations with a dipping needle, but drew no general conclusion from his results. Indeed, with the instruments then in use, he was not likely to determine that which has almost escaped detection with instruments of more accurate construction, for the diurnal variation of the whole magnetic force may perhaps still be considered doubtful. Later observations, particularly those of Professor Hansteen, have shown that the time of vibration of a horizontal needle varies during the day, from which it was inferred that the horizontal force also varies. Professor Hansteen, by this means, found that the horizontal intensity of terrestrial magnetism has a diurnal variation, de-

* *Philosophical Transactions*, 1832, p. 176. † *Ibid.* 1830, .p 407.

‡ Mr. Henwood informs me that he has repeated the experiments of Mr. Fox in from forty to fifty places not before experimented on, and that he proposes greatly extending them. As far as he can yet see, he considers that his results go to confirm Mr. Fox's deductions,—I suppose with regard to the electricity of metalliferous veins.

creasing, at Christiana, until ten or eleven o'clock in the morning, when it attains its minimum, and then increases until four or five o'clock after noon, when it appeared to reach its maximum*. By observing, at different times of the day, the direction of a horizontal needle thrown nearly at right angles to the meridian, by the action of two powerful magnets, placed in the meridian, passing through its centre, after correcting the observations for the effect of changes of temperature on the intensity of the force of the magnets, I found that at Woolwich the terrestrial horizontal intensity decreased until $10^h\ 30^m$ A.M., when it reached its minimum, and increasing from that time, attained its maximum about $7^h\ 30^m$ P.M.†. This agreement, in results obtained by totally independent methods, removes all doubt respecting the diurnal variation of the horizontal force. The difference in the time of the maximum in the two cases may be accounted for, independently of the difference in the variation at the two places of observation, by the circumstance that no correction for the effect of temperature on the time of vibration is made in Professor Hansteen's observation. As no such correction had hitherto been made, it must have been considered that differences in the temperature at which observations were made had little influence on the intensity of the vibrating needle; but in the communication containing these observations, I pointed out the necessity of such a correction‡; and since then, in deducing the terrestrial intensity from the times of vi-

* *Edinburgh Philosophical Journal*, vol. iv. p. 297.

† *Philosophical Transactions*, 1825, pp. 50 & 57. An inconvenience attending the method which I employed is, that the observations require a correction for temperature which is not very readily applied, as will be seen by referring to my paper; but this might in a great measure be obviated, by rendering the temperature of the magnets employed always the same previous to observation. If, however, in order to retain the needle in its position nearly at right angles to the meridian, torsion were applied instead of the repulsive forces of magnets, the correction for temperature would be nearly reduced to that due to the effects produced on the intensity of the needle itself by changes of temperature. But even this method is not without objection; for the sensibility of the needle depending upon the number of circles of torsion requisite to bring it into the proper position, if a wire were employed, unless very long, its elasticity would be impaired by more than two or three turns; and it is doubtful whether a filament of glass of moderate length would bear more than this without fracture. I had proposed to the late Captain Foster, previous to his last voyage, that he should determine the horizontal intensity at different stations, and also its diurnal changes by this method, and had a balance of torsion constructed for him for the purpose; but as the instrument is extremely troublesome in its adjustments, I consider that the many other observations which he had to make did not allow him time for the extensive use of this instrument which he had proposed. It is, however, very desirable that it should be ascertained how far this method is applicable.

‡ *Philosophical Transactions*, 1825.

bration of a needle, it has been customary to apply a correction for differences in the temperatures at which the observations may have been made.

The horizontal intensity varying during the day, it becomes a question whether this arises from a change alone in the direction of the force, or whether this change of direction is not accompanied by a change in the intensity of the whole force. In a communication to the Philosophical Society of Cambridge *, I suggested that deviations, from whatever cause, in the direction of the horizontal needle, were referrible to the deviations which, under the same circumstances, would take place in the direction of the dipping needle. Adopting these views, Captain Foster infers, from observations made by him at Port Bowen, on the corresponding times of vibration of a dipping needle, supported on its axis and suspended horizontally, that the diurnal change in the horizontal intensity is due principally, if not wholly, to a small change in the amount of the dip. The observations, however, do not indicate that the force in the direction of the dip is constant. Captain Foster's observations at Spitzbergen † show, more decidedly, the diurnal variation of this force: there, its maximum intensity appears to have occurred at about $3^h\ 30^m$ A.M., and the minimum at $2^h\ 47^m$ P.M.; its greatest change amounting to $\frac{1}{83}$ of its mean value. The maximum horizontal intensity appears to have occurred a little after noon, and the minimum nearly an hour after midnight; but there is considerable irregularity in the changes which it undergoes. It would, however, appear, from these observations, that the variations in the absolute intensity were in opposition to those in the horizontal resolved part of it; so that the principal cause of the latter variations must have been a change in the dip itself. Captain Foster considers "that the times of the day when these changes are the greatest and least, point clearly to the sun as the primary agent in the production of them; and that this agency is such as to produce a constant inflection of the pole towards the sun during the twenty-four hours." This is in perfect accordance with the conclusions I had previously drawn from the experiments on which I founded the theory of the diurnal variation of the needle ‡, as I had shown that if the diurnal variation of the needle arise from the cause which I have assigned for it, the dip ought to be a maximum, in northern latitudes, nearly when the sun is on the south magnetic meridian, and a minimum when it has passed it about 130°.

* *Transactions of the Philosophical Society of Cambridge*, 1820.

† *Philosophical Transactions*, 1828. ‡ *Ibid.* 1827, pp. 345, 349.

Humboldt was the first who determined that the intensity of the whole magnetic force is different at different positions on the earth's surface. Having made observations on the times of vibration of the same dipping needle, at various stations in the vicinity of the equator, and approaching to the northern pole, he found that the intensity of the terrestrial force decreases in approaching the equator; but no precise law, according to which the intensity depends upon the distance from the equator, can be determined from these observations. Numberless observations have since been made in both hemispheres, with every precaution to ensure accuracy in the results, but they do not in general accord with the theoretical formulæ with which they have been compared.

On the hypothesis of two magnetic poles not far removed from the centre of the earth, if δ represent the dip, λ the magnetic latitude of the place of observation, I the intensity of the force in the direction of the dip, and m a constant, then

$$I = \frac{m}{\surd(4 - 3\sin^2\delta)},$$

$$\tan\delta = 2\tan\lambda;$$

and therefore,

$$I = \frac{m}{2}\surd(3\sin^2\lambda + 1);$$

or if i is the angular distance from the magnetic pole, or the complement of the latitude,

$$I = \frac{m}{2}\surd(3\cos^2 i + 1).$$

By comparing his own observations with the first of these formulæ, Captain Sabine came to the conclusion that they were "decisive against the supposed relation of the force to the observed dip, and equally so against any other relation whatsoever, in which the respective phænomena might be supposed to vary in correspondence with each other." Comparing them, however, with the last formula, he concludes that "the accordance of the experimental results with the general law proposed for their representation, cannot be contemplated as otherwise than most striking and remarkable." How the same set of observations should be in remarkable accordance with the one formula and at variance with the other, when these formulæ are dependent on each other, it is difficult to conceive; but the conclusion drawn by Captain Sabine from his observations, at least shows the danger of relying upon any single set of observations as confirmatory or subversive of theoretical views. I have not yet compared with these results of theory the numerous observations made by Captain Foster, both in the northern and in the southern hemispheres; but it is my intention to do this as soon as I can determine what correction ought to be made for the differences of temperature at the several stations: I do not, however, anticipate any very close accordance.

In Captain Sabine's observations, the observed intensities, compared with those deduced from the preceding formulæ, are in excess near the equator, and in defect near the pole; and it is not improbable that, as Mr. Barlow has suggested, this increase of magnetic action near the equator above that which the theory gives, is due to the higher temperature in the equatorial regions *. I am, however, disposed to assign even a more powerful influence than this to difference of temperature; for I think it very possible, and indeed not improbable, that this may be the primary cause of the polarity of the earth, although its influence may be much modified by other circumstances. At the conclusion of the paper on the diurnal variation †, to which I have already referred, I have suggested an experiment which I think might throw much light on this subject. I have proposed that a large copper sphere, of uniform thickness, should be filled with bismuth, the two metals being in perfect contact throughout, and that experiments should be made with it similar to those which I had made with one of smaller dimensions, but from which I was unable to obtain any very definite results, in consequence of the want of uniformity in the thickness of the copper and in the contact of the two metals. On heating the equator of such a sphere, the parts round the poles being cooled by caps of ice—which might not unaptly represent the polar ices, —we may expect that currents of electricity would be excited; in which case the direction of those currents would decide whether the experiment were illustrative of the principal phænomenon of terrestrial magnetism, or not. Should these currents of electricity be in the direction of the meridians,—which is improbable, since in this case opposing currents would meet at the poles, and there would be no means of discharge for them,—I think we might then conclude that the magnetism of the earth cannot be due to the difference in the temperature of its polar and equatorial regions; but if, on the contrary, the currents should be in a direction parallel to the equator,—in which case their action upon a magnetized needle would be to urge it in the direction of the meridians,—I should then say that, in order to account for the terrestrial magnetic forces, and the diurnal

* *Edinburgh New Philosophical Journal*, July 1827.

† *Philosophical Transactions*, 1827, p. 354.

changes in their direction and intensity, it would only be required to show, that electrical phænomena may be excited, in such bodies as the earth and the atmosphere, by a disturbance in their temperature when in contact. As I consider that if such an experiment were carefully made it must give conclusive results, I would strongly suggest to the Council of the British Association the importance of having it made.

It has been a question whether the intensity of terrestrial magnetism is the same at the surface of the sea and at heights above that surface to which we can attain. MM. Gay-Lussac and Biot, in their aërostatic ascent, could detect no difference at the height of 4000 metres *. Saussure had, however, concluded, from the observations which he made at Geneva, Chamouni, and on the Col du Géant, that the intensity was considerably less at the latter station than at either of the former, the difference in the levels being in the one case about 10,000 feet, in the other about 7800 †.

M. Kupffer ‡ also considers that his observations in the vicinity of Elbours, in which the difference of elevation of his two stations was 4500 feet, show clearly that the horizontal intensity decreases as we ascend above the surface; and he accounts for this decrease not having been observed by MM. Biot and Gay-

* Biot, *Traité de Physique.*

† *Voyages dans les Alpes,* tom. iv. p. 313.—I take for granted that, admitting the accuracy of Saussure's observations, they warranted the conclusions he drew from them; but some unaccountable errors must have crept in, either in transcribing or in printing them; for not only the means which he deduces do not result from the observations, but the numbers which he employs contradict his conclusions. I transcribe the passage from the only edition I can consult, published at Neufchâtel, 1796. "A' Génève ces vingt oscillations employerent $5^m\ 2^s$; $4^m\ 50^s$; 5^m; $4^m\ 40^s$; dont la moyenne étoit $5^m\ 0^s{\cdot}4$; le thermometre étant à 6 dégrés. A' Chamouni $5^m\ 33^s$; $5^m\ 34^s$; moyenne $5^m\ 33^s{\cdot}5$; thermometre 12 dég. Au Col du Géant $5^m\ 30^s{\cdot}3$; $5^m\ 30^s{\cdot}5$; $5^m\ 31^s{\cdot}4$; $5^m\ 34^s{\cdot}6$, moyenne $5^m\ 32^s{\cdot}45$; thermometre 12·4 dégrés."

"Or les forces magnétiques sont inversement comme les quarrés des tems. Mais, à Génève, le tems étoit $5^m\ 0^s{\cdot}4$ ou $300^s.4$, dont le quarré $=$ 111155·56; à Chamouni $5^m\ 33^s{\cdot}5 = 333^s{\cdot}5$, dont le quarré $=$ 111223. Au Géant $5^m\ 32^s{\cdot}45 = 332^s{\cdot}45$, dont le quarré $=$ 11523·0025; d'où il suivroit que la plus grande force étoit dans la plaine, et la plus petite sur la plus haute montagne, à peu pres d'une cinquieme: observation bien importante, si elle étoit confirmée par des expériences répetées, et faites à la même température."

The means of the above observations are $4^m\ 53^s = 293^s$, $5^m\ 33^s{\cdot}5 = 333^s{\cdot}5$, and $5^m\ 31^s{\cdot}7 = 331^s{\cdot}7$; and the squares of these numbers are 85849, 111222·25, 110024·89. So that, according to this, the force was greatest at Geneva, and least at Chamouni. Taking Saussure's numbers, $300^s{\cdot}4$, $333^s{\cdot}5$, $332^s{\cdot}45$, their squares are 90240·16, 111222·25, 110523·0025; so that still the general conclusions are the same.

‡ *Voyage dans les Environs du Mont Elbronz. Rapport fait à l'Académie Impériale des Sciences de St. Petersbourg,* p. 88.

Lussac, by its having been counteracted by the increase of intensity, arising from the diminution of temperature. Mr. Henwood informs me that he has made corresponding observations, consisting of two series, each of 3900 vibrations at each place; on Cairn Brea Hill, 710 feet above the level of the sea; at the surface of Dolcoath mine, 370 feet above the sea; and at a depth of 1320 feet beneath the surface in Dolcoath mine, or 950 feet below the level of the sea; and that, after clearing the results from the effects of temperature, the differences are so minute that he cannot yet venture to say he has detected any difference in the magnetic intensity at these stations. If, notwithstanding these results, we are to admit the correctness of M. Kupffer's conclusions, I think we must infer that the diminution of horizontal intensity at his higher station was due to an increase in the dip, which element would not probably be so much affected by a change of elevation in a comparatively level country, like Cornwall, as on the flank of such a mountain mass as Elbours.

Before dismissing the subject of the terrestrial intensity, I should mention that attempts have been made to delineate on charts the course of isodynamic lines. Professor Hansteen has published a chart in which this is done for the year 1824. Of all observations, however, requisite for graphic exhibitions connected with terrestrial magnetism, those on the authority of which such lines must be drawn are fewest in number and least satisfactory in their results; we should, therefore, be very cautious in drawing conclusions from such delineations.

Hitherto I have only referred to such changes in the direction of the magnetic force, and in its intensity, as appear to depend upon general causes; but, besides these, sudden and sometimes considerable irregular changes occur. These have very generally been attributed to the influence of the aurora borealis, whether visible or not at the place of observation; and I think it not improbable that some may be due to a peculiar electrical state of the atmosphere, independent of that meteor. The influence of the aurora borealis on the magnetic needle has, however, been denied by some, principally because, during the occurrence of that meteor at Port Bowen, Captain Foster did not observe peculiar changes in the direction of the needle, although, from his proximity to the magnetic pole, the diurnal change sometimes amounted to 4° or 5°; and, under such circumstances, it was considered that these changes ought to have been particularly conspicuous. In a paper inserted in the second volume of the *Journal of the Royal Institution,* I have, however, shown that Captain Foster's Port Bowen observations do not warrant the conclusions which have been drawn from them, and have pointed out circumstances which may, in this case, have rendered the effect of the aurora upon the horizontal needle less sensible than might have been expected. That changes in the direction and intensity of the terrestrial forces are simultaneous with the aurora borealis I feel no doubt, for I have seen the changes in the direction of the needle to accord so perfectly with the occurrence of this meteor, and to such an extent, that in my mind the connexion of the phænomena became unquestionable*. As, however, the magnetic influence of the aurora borealis has been doubted, I shall here point out the manner in which I consider the effects may be best observed.

If the magnetic forces brought into action during an aurora are in the direction of the magnetic meridian, they will affect a dipping needle adjusted to the plane of that meridian, but the direction of an horizontal needle will remain unchanged: on the other hand, if the resultant of these forces makes an angle with the meridian, the direction of the horizontal needle will be changed, but the dipping needle may not be affected. In order to determine correctly the magnetic influence of the aurora by means of an horizontal needle, it is therefore necessary not only to have regard to those forces which influence its direction, but likewise to those which affect the horizontal intensity. The effects of the former are the objects of direct observation, but those of the latter are not so immediately observable. As, during an aurora, the intensity may vary at every instant,—and it is these changes which are to be detected,—the method of determining the intensity by the time of vibration of the needle cannot here be applied, and other means must be adopted. The best method appears to me to be that which I employed for determining the diurnal variation of the horizontal intensity, the needle being retained nearly at right angles to the meridian by the repulsive force of a magnet, or by the torsion of a fine wire or thread of glass. For the purpose, then, of detecting in all cases the magnetic influence of the aurora, I consider that two horizontal needles should be employed; one, adjusted in the meridian, for determining the changes which may take place in the direction of the horizontal force, and the other at right angles to the meridian, to determine the changes in the intensity of that force, arising principally from new forces in the plane of the meridian, and which would affect the direction of the dipping needle alone. Both these needles should be deli-

* For the observations to which I here particularly refer, see the *Journal of the Royal Institution*, vol. ii. p. 272.

cately suspended, either by very fine wire, or by untwisted fibres of silk. In order to render the changes in the direction of the needle in the meridian more sensible, its directive force should be diminished by means of two magnets north and south of it, and having their axes in the meridian. These magnets should be made to approach the needle until it points about 30° on either side of the meridian, and they should be so adjusted that the forces acting upon the needle will retain it *in equilibrio* with its marked end at about 30° to the east and 30° to the west of north, and also at south. The needle is to be left with its marked end pointing south, for the purpose of observing the changes occurring in its direction. If magnets are employed to retain the second needle nearly at right angles to the meridian, they should be made to approach its centre until the points of equilibrium are at about 80° east, 80° west and south, the observations being made with the needle at 80° east or 80° west. An objection to this method of adjusting this needle by means of magnets, and to which I have already referred in a note, is that any change in their temperature will have a very sensible effect on the direction of the needle in this position; and should such change take place during the observations, corrections must be applied to the results before any accurate conclusions can be drawn from them. As, however, an aurora is not generally of long continuance, any change in the temperature of the magnets during the observations is much more easily guarded against than where the observations have to be continued during successive days and at different seasons of the year. I have before remarked that this inconvenience will be, in a great measure, obviated by employing the torsion of a fine wire, or a very fine filament of glass, to retain the needle at about 80° from the meridian. In this case, the ratio of the force of torsion to the terrestrial force acting upon the needle having been determined, a measure will be obtained of the changes which take place in the intensity of the terrestrial force during the occurrence of an aurora. It is very desirable that it should be ascertained whether the effects on the needle are simultaneous with any particular class of phænomena connected with the aurora; whether these effects are dependent on the production of beams and corruscations, or on the formation of luminous arches; or whether any difference exists in the effects produced by these. In order to determine this, it is necessary that the times of the occurrence of the different phænomena, and also of the changes in the directions of the needles, should be accurately noted; and for such observations, three observers appear to be indispensable.

Whether the direction of the needle may be influenced by the electrical state of the clouds, is much more doubtful than the influence of the aurora. I am not aware of any extended series of observations made with a view to determine this point. Having adjusted, in a particular manner, a needle between two magnets, so that the directive force was considerably diminished, I found that the changes in the positions of electric clouds was accompanied by changes in the direction of the needle; but, although the observations indicate that the needle was thus affected, they are of too limited a nature to draw any general conclusion from*. Some observations of Captain Sir Everard Home, however, indicate the same kind of influence. In a conversation which I had with him last year, having referred to the effect I had observed to be produced by the sun's rays, of bringing a vibrating needle to rest, it brought to his mind a similar effect which he observed during a thunder-storm. He has favoured me with his observations, and from these it appears that, in two instances, a needle came sooner to rest during a thunder-storm than it had previous or subsequent to it. The arc at which the vibrations ceased to be counted is not recorded, but the number of vibrations was reduced in one case from 100 to 40, and in another from 200 to 120. I have, in consequence of these observations, requested Lieutenant Barnett of the Royal Navy, who is engaged in the survey of the southern coast of the Gulf of Mexico, to make similar observations, should he have an opportunity; and as thunder-storms are so frequent, and of such intensity on that coast, I think he may obtain some important results as connected with the influence of the electric state of the atmosphere upon the vibrations and direction of the needle.

Upon a review of all the phænomena of terrestrial magnetism, and considering the intimate relation which has been established between magnetism and electricity, by which it appears that, if not identical, they are only different modifications of the same principle, there can, I think, be little doubt that they are due to electric currents circulating round the earth. How these currents are excited, whether by heat, by the action of another body, or in consequence of rotation, we are not at present able to determine; but however excited, they must, though not wholly dependent upon them, be greatly modified by the physical constitution of the earth's surface. We are, therefore, not to expect that symmetry in their course which would be the

* *Philosophical Transactions*, 1823, p. 354. The arrangements which I have just described for determining the influence of the aurora borealis are well adapted for deciding this point.

consequence of a symmetrical constitution of that surface. But even if such symmetry did exist, the action of all the currents at different stations on the surface could scarcely be referred to the same two points as centres of force; and without this symmetry, it would be absurd to expect it. The hypothesis, therefore, of only two poles, as explanatory of the phænomena, must be rejected; and if we are to refer these phænomena to centres of action, we must, besides two principal ones, admit the existence of others depending upon local causes.

It has been said that if we refer the magnetism of the earth to another body, we only remove the difficulty, and gain little by the supposition*. It, however, appears to me, that if we could show that the magnetism of the earth is due to the action of the sun, independent of its heat,—which, however, I think the more probable cause,—the problem would be reduced to the same class as that of accounting for the light of the sun, the heating and chemical properties of its rays: we only know the facts, and are not likely to know more.

If difficulties meet us at every step when we attempt to explain the general phænomena of terrestrial magnetism, these difficulties become absolutely insurmountable when we come to the cause of their progressive changes. Here, at least, we must for the present be satisfied with endeavouring to discover whether these changes are governed by any general laws: should they be so, their cause may possibly be discovered. Diligent and careful observation is the only means by which we can hope to attain this end, and indeed is that on which we must principally rely for gaining a more correct knowledge of all the phænomena, and of their causes; and, consequently, improvements in the methods of observation, and in the instruments to be employed, become of the highest importance.

This Report has already so far exceeded the limits within which I wished to have confined it, that I must restrict the remarks on this part of the subject to a few points.

In the observations of Humboldt, in those of M. Rossel, of Captain Sabine, and of Captain Foster, the terrestrial magnetic intensity had been determined by the vibrations of a dipping needle in the plane of the magnetic meridian; but as there is by this means, in consequence of the friction upon the axis, a difficulty in obtaining a sufficient number of vibrations to ensure accuracy, and a dipping instrument is besides ill adapted for carriage, Professor Hansteen proposed to determine the same by means of a small needle suspended horizontally by a few

* Hansteen's *Inquiries concerning the Magnetism of the Earth.*

untwisted fibres of silk. The advantages, however, attending this method of Professor Hansteen, I consider to be more apparent than real; for without determining the dip, the horizontal force, deduced from the vibrations of the horizontal needle, cannot be reduced to the force in the direction of the dip; and if the dip is determined, two instruments become necessary where, before, only one was requisite.

In order to obviate the inconveniences attending each of these methods, I have proposed a construction for a dipping needle, by means of which the observations which determine the direction of the terrestrial force will also give a measure of its intensity. The general principle of the construction is simply, that the centre of gravity of the needle should not be in its centre of figure, but in a line drawn from that centre at right angles, both to its axis of motion and to its magnetic axis; so that, by two observations, one with the centre of gravity upwards, and the other with it downwards, the dip, and likewise the relation which the static momentum of its weight bears to that of the terrestrial magnetic force acting upon the magnetism of the needle, may be determined. The principles on which these determinations depend, and the advantages which I propose from the adoption of this construction, are fully described in a paper read before the Royal Society, and which will appear in the *Philosophical Transactions* of this year.

Professor Gauss has proposed a method of determining the intensity and the changes it undergoes, by which he hopes to reduce magnetical observations to the accuracy of astronomical ones. By the vibrations of a magnetized bar he determines the product of the terrestrial magnetic intensity by the static momentum of its free magnetism. By introducing a second bar, and by observing at different distances the joint effects of the first, and of the terrestrial magnetism on this, he determines the ratio of the terrestrial intensity to the static momentum of the free magnetism of the first. Eliminating this last from the two equations, he obtains an absolute measure of the terrestrial magnetic intensity, independent of the magnetism of the bar. This is a most important result, for we shall thus be enabled to determine the changes which the terrestrial intensity undergoes in long intervals of time. It is, however, to be observed, that it is only the horizontal intensity which is thus determined, and that, in order to determine the intensity of the whole force, another element, namely, the dip, must also be observed; and I fear much that the introduction of this element will, in a great measure, counteract that accuracy of which the methods proposed for determining the times of vibration appear capable. This must be an objection, even where the observations are made in a fixed observatory; but where an apparatus has to be moved from one station to another, I think the method could scarcely be applied successfully, principally on account of the delicacy of the preliminary observations, and of the time requisite for making them, in addition to that required for the observations by which the terrestrial intensity and its variations are to be determined. However greatly I may admire the sagacity which Professor Gauss has shown in devising means for the determination of an absolute measure of the horizontal intensity, I cannot avoid seeing the difficulties which may occur in its practical application.

The method which Professor Gauss proposes, and has practised, of observing the course of the daily variation, and of determining the time of vibration, by means of a plane mirror fixed on the end of the needle, perpendicularly to its axis, and observing the reflected image of the divisions of a scale by means of a theodolite fixed at a distance, appears to admit of the greatest possible precision, and will probably supersede other methods of observing the daily variation.

I have adverted to the necessity of careful and diligent observation of all the phænomena of terrestrial magnetism, as the surest means of arriving at a knowledge of their causes: it is with reluctance I state it, but I believe it to be a fact, that this is the only country in Europe in which such observations are not regularly carried on in a national observatory. Such an omission is the more to be regretted, seeing that no one has, I believe, carried on a regular series of observations on the diurnal variation, since the valuable ones by Colonel Beaufoy were interrupted by his death, this interruption happening at a time when it was peculiarly desirable that the series should be unbroken. At this time the needle near London had begun to show a return towards the true meridian; but whether this was one of those oscillations which have occasionally been observed, or that, having really attained its maximum of westerly deviation, it was returning in the contrary direction, is, I believe, undecided at the present moment. Of all the data requisite for determining the laws which govern the phænomenon of the variation, the time of the maxima and their magnitude are the most important. I trust that ere long the important desideratum will be supplied of a regular series of magnetical observations in the national Observatory of Great Britain.

Royal Military Academy,
22nd June, 1833.

Report on the present State of the Analytical Theory of Hydrostatics and Hydrodynamics. By the Rev. J. Challis, *late Fellow of Trinity College Cambridge.*

The problems relating to fluids, which have engaged the attention of mathematicians, may be classed under two heads,—those which involve the consideration of the attractions of the constituent molecules, and the repulsion of their caloric; and those in which these forces are not explicitly taken account of. In the latter class the reasoning is made to depend on some property derived from observation. For instance, water is observed to be very difficult of compression; and this has led to the assumption of absolute incompressibility, as the basis of the mathematical reasoning: air at rest, and under a given state of temperature, is observed to maintain a certain relation between the pressure and the density; hence the fundamental property of the fluid which is the subject of calculation is assumed to be the constancy of this relation, to the exclusion of all the circumstances which may cause it to vary. The fluids treated of in this kind of problems are rather hypothetical than real, yet not so different from real fluids but that the mathematical deductions obtained respecting them admit of having the test of experiment applied. I propose in this Report to confine myself entirely to problems of the second class,—those in the *common theory* of fluids. The reasons for making this limitation are, that both kinds together would afford too ample matter for one Report, and that those which I have selected are distinguished from the others by the different purpose in regard to science which correct solutions of them would answer: for the treatment of any hydrostatical or hydrodynamical questions which involve the consideration of molecular attraction and the repulsion of heat, must proceed upon certain hypotheses respecting the mode of action of these forces, and the interior constitution of the fluid, as these are circumstances which from their nature cannot be data of observation; and hence, assuming the mathematical reasoning founded on the hypotheses to be correct, a satisfactory comparison of the theoretical deductions with facts must serve principally to establish the truth of the hypotheses, and so to let us into secrets of nature which probably could never be known by any other process. But when the basis of calculation, as in the questions that will come before us, is some *observed* and acknowledged fact, solutions which satisfy experiments will first of all serve to confirm the truth of the mathematical reasoning, and then give us confidence in the theoretical results, which, as often happens, cannot readily receive the test of experiment. Calculations of this kind do not add much to our conviction that the facts applied as the test of the theory are really consequences of those which are the basis of it. For instance, we feel satisfied, independently of any mathematical reasoning, that the motions of waves on the surface of water are consequences of the incompressibility of the fluid, and the law of equal pressure. But the purpose which these calculations answer of *confirming methods of applying analysis* is very important, particularly in regard to the higher class of physical questions, which M. Poisson has proposed to refer to a distinct department of science, under the title of *Mathématique Physique,* viz. those that require in their theoretical treatment some hypotheses respecting the interior constitution of bodies, and the laws of corpuscular action: for in questions of this nature, as well as in problems in the common theory of fluids, the mathematical reasoning conducts to partial differential equations; and if the method of treating these, and of drawing inferences from their integrals, be established in one kind, it may be a guide to the method to be adopted in the other. It is plainly, then, desirable that the mathematical processes be first confirmed in the cases in which the basis of reasoning is an observed fact, that the reasoning may proceed with certainty in those cases where it is based on an hypothesis, the truth of which it proposes to ascertain.

The subjects of this Report may now be stated to be, the leading hydrostatical and hydrodynamical problems recently discussed, which proceed upon the supposition of an incompressible fluid, or of a fluid in which the quotient of the pressure divided by the density is a constant; and the end it has in view is, to ascertain to what extent, and with what success, analysis has been employed as an instrument of inquiry in these problems. I am desirous it should be understood that I have not attempted to make a complete enumeration either of the questions that have been discussed in this department of science, or of the labours of mathematicians in those which have come under notice. It has rather been my endeavour to give some idea of the most approved methods of treating the leading problems, and the possible sources of error or defect in the solutions. In taking this course I hope I may be considered to have acted sufficiently in accordance with the recommendation of the Committee for Mathematics, which was the occasion of my receiving the honour of a request to take this Report in hand.

With the limitation above stated as to the subjects our Report is to embrace, we shall have scarcely anything to say on the analytical theory of hydrostatics. The problems of interest in this department were early solved, and present no difficulty in principle, and little in the detail of calculation. The determination of the height of mountains by the barometer is a hydrostatical question, the difficulty of which does not consist in the analytical calculation, but only in ascertaining the law of the distribution of the atmospheric temperature. We shall not have to speak of the theories that have been invented to overcome this difficulty. Neither does it fall within the scope of this Report to notice the very valuable memoir of M. Poisson on the equilibrium of fluids*, which has for its object the derivation of the general equations of equilibrium from a consideration of molecular attraction and the repulsion of caloric, and seems to have been composed in immediate reference to the theory of capillary attraction, which the author subsequently published. With regard to the problem of capillary attraction, we may remark, that it is not possible by any supposition respecting the forces which sustain or depress the fluid in the tube, to solve it as a question in the common theory of hydrostatics. M. Poisson has shown the insufficiency of Laplace's theory, and by taking into account the molecular forces and the effect of heat, has proved that the explanation of the phænomenon is essentially dependent on a modification of the property which is the basis of the common theory, viz. the incompressibility of the fluid. It does not fall within our province to say more on the celebrated theory of M. Poisson.

One improvement I consider to have been recently made in the common theory of fluids. It has been usual to take the law of equal pressure as a datum of observation. Professor Airy, in his Lectures in the University of Cambridge, has shown that this property may be derived, by reasoning according to established mechanical principles, from another of a simpler kind, the notion of which may be gathered from observation, viz. that the division of a perfect fluid may be effected without the application of sensible force; from which it immediately follows that the state of equilibrium or motion of a fluid mass is not altered by mere separation of its parts by an indefinitely thin partition. A definition of fluids founded on this principle, and

* *Mémoires de l'Académie des Sciences*, Paris, tom. ix. 1830.

a proof of the law of equal pressure, are given at the beginning of the *Elements of Hydrostatics and Hydrodynamics* of Professor Miller*. Several advantages attend this mode of commencing the mathematical treatment of fluids. The principle is one which perfectly characterizes fluids, as distinguished in the internal arrangement of their particles from solids. It may be rendered familiar to the senses. It is, I think, necessary for the solutions of some hydrostatical and hydrodynamical problems, particularly those of reflection†. Lastly, in reference to the department of science proposed to be called *Physical Mathematics,* the propositions of the common theory ought to be placed on the simplest possible basis, because the questions of most interest in that department are those which have in view the explanation of the phænomena that are the foundations of the reasoning in the other kind. The solution of one such question is a great step in scientific generalization. It is plainly, therefore, of importance that the fact proposed for explanation should be the simplest that direct observation can come at.

The analytical theory of hydrodynamics is of a much more difficult nature than that of hydrostatics. The assumptions it is necessary to make to obtain even approximate solutions of the simplest problems of fluid motion betray the difficulty and imperfection of this part of science. There are cases, however, of *steady* motion, that is, of motion which has arrived at a permanent state, so that the velocity is constantly the same in quantity and direction at the same point, which require a much more simple analysis than those which do not satisfy this condition. It does not appear that the equations applicable to this kind of motion were obtained in any general manner till they were given in an *Elementary Treatise on Hydrostatics and Hydrodynamics* by Mr. Moseley‡, who has derived them from a principle of so simple a nature, that, as it can be stated in a few words, it may be mentioned here. *When the motion is steady, each particle in passing from one point to another, passes successively through the states of motion of all the particles which at any instant lie on its path.* This principle is valuable for its generality: it is equally applicable to all kinds of fluids, and will be true, whether or not the effect of heat be taken into account, if only the condition of steadiness remains. The equations of motion are readily derived from it, because it enables us to consider the

* Cambridge 1831.

† Dr. Young employed an equivalent principle to determine the manner of the reflection of waves of water. See his *Natural Philosophy,* vol. ii. p. 64.

‡ Cambridge 1830.

motion of a single particle, in the place of the motion of an aggregate of particles. Though this mode of deriving them is the best possible on account of its simplicity, it was yet desirable to know how they may be obtained from the general equations of fluid motion. In a paper contained in the *Transactions of the Philosophical Society of Cambridge* *, the author of this Report has given a method of doing this, both for incompressible and elastic fluids, and has shown that a term in the general formulæ which gives rise to the complexity common to most hydrodynamical questions, disappears for this kind of motion. Euler had already done the same for incompressible fluids †. The instances in nature of fluid motion of the steady kind are far from uncommon; and it is probable that when the equations applicable to them are better known, and studied longer, they may be employed in very interesting researches. The motion of the atmosphere, as affected by the rotation of the earth, and a given distribution of the temperature due to solar heat, seems to be an instance of this kind.

We will now proceed to consider in order the principal hydrodynamical problems that have recently engaged the attention of mathematicians. For convenience we shall class them as follows:—I. Motion in pipes and vessels. II. The velocity of propagation in elastic fluids. III. Musical vibrations in tubes. IV. Waves at the surface of water. V. The resistance to the motion of a ball-pendulum.

I. The motion of fluids in pipes and vessels has not been treated with any success, except in the cases in which the condition of steadiness is fulfilled. The paper above alluded to, in the *Transactions of the Philosophical Society of Cambridge*, contains some applications of the equation of steady motion for incompressible fluids, to determine the velocity of water issuing from different kinds of adjutages in vessels of any shape: also a theoretical explanation of a phænomenon which a short while ago excited some attention,—that of the attraction of a disc to an orifice through which a steady current either of water or air is issuing.

In the *Memoirs of the Paris Academy of Sciences* ‡ there is an Essay by M. Navier on the motion of *elastic* fluids in vessels, and through different kinds of adjutages into the surrounding air, or from one vessel into another. For the sake of simplicity the author considers the fluid to be subject to a constant pressure, and consequently the motion to have arrived at a state of permanence. His calculations are founded upon the

* Vol. iii. Part III. † *Mémoires de l'Académie de Berlin*, 1755, p. 344.
‡ Tom. ix. 1830.

hypothesis of *parallel slices*, which assumes the velocity to be the same, and in the same direction, and the density to be the same at all points of any section transverse to the axis of the vessel or pipe. This hypothesis is one of those that the theory of hydrodynamics has borrowed from experience to supply its defects. Lagrange has, however, shown theoretically* that it always furnishes a first approximation, the breadth of the vessel being considered a quantity of the first order, and the effect of the adhesion of the fluid to the sides of the vessel being neglected. It is right to observe, that in the problems M. Navier has considered, this hypothesis might have been in a great measure dispensed with: the expression he has given,—more correct than that commonly adopted for the velocity of issuing through a small aperture by which airs of different densities communicate,—might have been obtained by employing the equation above mentioned of steady motion, as, in fact, Mr. Moseley has done†. This would be a preferable mode of treating such questions, because in every instance in which these auxiliary hypotheses are got rid of, something is gained on the side of theory. This memoir contains another hypothesis, which cannot be so readily dispensed with. Theory is at present quite inadequate to determine the retardation in the flow of fluids occasioned by sudden contractions or widenings in the bore of the pipe. It is found by experiments with water, that the retardation is sufficiently represented by taking account of the loss of *vis viva* which, on the hypothesis of parallel slices, will result from the sudden changes of velocity which must be supposed to take place at the abrupt changes in the bore of the pipe. M. Navier extends these considerations to elastic fluids. The theory manifests a sufficient agreement with the experiments it is compared with, and is valuable on account of the applications it may receive.

II. The most interesting class of problems in hydrodynamics are perhaps those which relate to small oscillations. Newton was the first to submit the vibrations of the air to mathematical calculation. The propositions in the second book of the *Principia*, devoted to this subject, and to the determination of the velocity of sound, may be ranked among the highest productions of his genius. He has assumed that the vibratory motion of the particles follows the law of the motion of an oscillating pendulum. It was soon discovered that many other assumed laws of vibration would, by the same mode of reasoning,

* *Mécanique Analytique*, Part II. § xi. art. 34.

† *Elementary Treatise*, p. 204.

conduct to the same velocity of propagation. This, which was thought to be an objection to the reasoning, is an evidence of its correctness: for the plain consequence is, that the velocity of propagation is independent of the kind of vibration which we may *arbitrarily* impress on the fluid;—and so experience finds it to be.

When the partial differential equation, which applies equally to the vibrations of the air and those of an elastic chord, had been formed and integrated, a celebrated discussion arose between Euler and D'Alembert as to the extent to which the integral could be applied; whether only to cases in which the motion was defined by a continuous curve, or also to motion defined by a broken and discontinuous line. It is well known that the question was set at rest by Lagrange, in two Dissertations published in vol. i. and vol. ii. of the *Miscellanea Taurinensia.* The difficulty that arrested the attention of these eminent mathematicians was one of a novel kind, and peculiar to physical questions that require for their solution the integrals of partial differential equations. The difficulty of integration, which is the obstacle in most instances, had been overcome by D'Alembert. It remained to draw inferences from the integral,—to interpret the language of analysis. When an aggregate of points, as a mass of fluid or an elastic chord, receives an arbitrary and irregular impulse, any point not immediately acted upon may have a correspondent irregular movement after the initial disturbance has ceased. This is a matter of experience. Was it possible, then, that these irregular impulses, and the consequent motions, were embraced by the analytical calculation? From Lagrange's researches it follows that the functions introduced by integration are arbitrary to the same degree that the motion is so *practically,* and that they will therefore apply to discontinuous motions. (Of course we must except the practical disturbances which the limitations of the calculation exclude,—those which are very abrupt, or very large.) This has been a great advance made in the application of analysis to physical questions. Had a different conclusion been arrived at, many facts of nature could never have come under the power of calculation. The *Researches* of Lagrange, which will ever form an epoch in the science of applied mathematics, establish two points principally: First, That the arbitrary functions, as we have been just saying, are not necessarily continuous: Secondly, That (in the instance he considered) they are equivalent to an infinite series of terms having arbitrary constants for coefficients, and proceeding according to the sines of multiple arcs. This latter result, which appears to be true for all linear partial differential equations of the second order, with constant coefficients, is valuable as presenting an analogy between arbitrary constants and arbitrary functions.

But the way in which Lagrange, after establishing these two points, proceeds to find the velocity of propagation, does not appear to me equally satisfactory with the rest of his reasoning. His method seems to be a departure from the principle which may be gathered from that of Newton. For, as was mentioned above, the reasoning of the *Principia* shows that the velocity of propagation is independent of all that is arbitrary. It seems important to the truth of the analytic reasoning, that it should not only obtain a constant velocity of propagation, but arrive at it by a process which is independent of the arbitrary nature of the functions; whereas the method which the name of Lagrange has sanctioned, is essentially dependent on the discontinuity of the functions, that is, on their being arbitrary. With a view of calling attention to this difficulty, and as far as possible removing it, the author of this Report read a paper before the Philosophical Society of Cambridge, which is published in Vol. iii. Part I. of their *Transactions*. I am far from asserting that that Essay has been successful; but some service, I think, will be done to science if it should lead mathematicians to a *reconsideration of the mode of mathematical reasoning to be employed in regard to the applications of arbitrary functions.* If the determination of the velocity of propagation in elastic fluids were the only problem affected by this treatment of arbitrary functions, it would not be worth while to raise a question respecting the principle of the received method, as no doubt attaches to the result obtained by it; but there are other problems, (one we shall have to consider,) the correct solutions of which mainly depend on the construction to be put upon these functions. The difficulty I am speaking of, which is one of a delicate and abstract nature, will perhaps be best understood by the following queries, which seem calculated to bring the point to an issue:—Can the arbitrary functions be *immediately* applied to any but the parts of the fluid immediately acted upon by the arbitrary disturbance, and to parts indefinitely near to these? To apply them to parts more remote, is it not necessary first to obtain the law of propagation? And do not the arbitrary functions themselves, by the quantities they involve, furnish us with means of ascertaining the law of propagation, independently of any consideration of discontinuity?

Euler and Lagrange determined the velocity of propagation in having regard to the three dimensions of the fluid, on the limited supposition that the initial disturbance is the same as to density and velocity, at the same distance in every direction from a fixed point, which is the centre of it. Laplace first dispensed with this limitation in the case in which two dimensions only of the fluid are taken account of*. The principal character of his analysis is a new method of employing definite integrals. Finally, M. Poisson solved the same problem for three dimensions of the fluid†. This memoir deserves to be particularly mentioned for the interesting matter it contains. The object of the author is to demonstrate, in a more general manner than had been before done, some circumstances of the motions of elastic fluids which are independent of the particular motions of the fluid particles, such as propagation and reflection. The general problem of propagation just mentioned he solves by developing the integral of the partial differential equation of the second order in x, y, z, and t, applicable to this case, in a series proceeding according to decreasing powers of the distance from the centre of disturbance, as it cannot be obtained in finite terms, and then transforming the series into a definite integral,—a method which has of late been extensively employed. The crossing of waves simultaneously produced by disturbances at several centres, is next considered, and this leads to a general solution of the problem of reflection at a plane surface. For the case in which the motions of the aërial particles are not supposed small, the velocity of propagation along a line of air is shown to be the same as when they are small. This result is an inference drawn from the arbitrary discontinuity of the motion, on which it does not seem to depend. In a paper before alluded to‡, the same result is obtained without reference to the principle of discontinuity. M. Poisson treats also of propagation in a mass of air of variable density, such as the earth's atmosphere. His analysis is competent to prove, in accordance with experience, that the velocity of sound is the same as in a mass of uniform density, and that its intensity at any place depends, in addition to the distance from the point of agitation, only on the density of the air where the disturbance is made. So that a bell rung in the upper regions of the air will not sound so loud as when rung by the same effort below, but will sound equally loud at all equal distances from the place where it is rung.

In seeking for the general equations of the motion of fluids, (first obtained by Euler,) a quantity§ is met with which, if it be

* *Mémoires de l'Académie*, An 1779.

† "Mémoire sur la Théorie du Son," *Journal de l'École Polytechnique*, tom. vii. cah. xiv.

‡ *Transactions of the Philosophical Society of Cambridge*, vol. iii. Part III.

§ In M. Poisson's writings this quantity is $u\,dx + v\,dy + w\,dz$.

an exact differential of a function of three variables, renders the subsequent analytical reasoning much simpler than it would be in the contrary case. This simplification has been proved by Lagrange to obtain in most of the problems of interest that are proposed for our solution *. Euler showed that the differential is inexact when the mass of fluid revolves round an axis so that the velocity is some function of the distance from the axis †. But no general method exists of distinguishing the instances in which the quantity in question is a complete differential, and when it is not. Nor is it known to what physical circumstance this peculiarity of the analysis refers. To clear up this point is a desideratum in the theory of hydrodynamics. M. Poisson has left nothing to be desired in the generality with which he has solved the problem of propagation of motion in elastic fluids; for in the *Memoirs of the Academy of Paris* ‡ he has given a solution of the question, without supposing the initial disturbance to be such as to make the above-mentioned quantity an exact differential. His conclusions are, that the velocity of propagation is the same as when this supposition is made; that the part of the motion which depends on the initial condensations or dilations follows the same laws as in that case, but the part depending on the initial velocity does not return completely to a state of repose after a determinate interval of time; that at great distances from the place of agitation there is no essential difference between the motion in the two cases.

III. We turn now to the theory of musical vibrations of the air in cylindrical tubes of finite length. Little has been effected by analysis in regard to this interesting subject. The principal difficulty consists in determining the manner in which the motion is affected by the extremities of the tube, whether open or closed, but particularly the open end. Those who first handled the question reasoned on the hypotheses, that at the open end the air is always of the same density as the external air to maintain an equilibrium with it, and at the closed end always stationary by reason of the stop. The latter supposition will be true only when the stop is perfectly rigid. It does not materially affect the truth of the reasoning; but if the other supposition were strictly true, the sound from the vibrating column of air in the tube would not cease so suddenly as experience shows it does, when the disturbing cause is removed; neither on this hypothesis could the external air be acted on so as to receive alternate condensations and rarefactions, and transmit

* *Mécanique Analytique*, Part II. § xi. art. 16.
† *Mémoires de l'Académie de Berlin*, 1755, p. 292.
‡ tom. x. 1831.

sonorous waves. These objections to the old theory have been stated by M. Poisson, who proposes a new mode of considering the problem *. He reasons on an hypothesis which embraces both the case of an open and a closed end, viz. that the velocity at each is in a constant ratio to the condensation. This ratio will be very large for the open end, and a very small fraction for the closed end. Its exact value in the latter case depends on the elasticity of the stop, and in the other on the mode of action of the vibrations on the external air,—to determine which is a problem of great difficulty, which M. Poisson has forborne to meddle with. His theory is not competent to assign *à priori* either the series of tones or the gravest that can be sounded by a tube of given length, but is more successful in determining the number of *nodes* and *loops*, and the intervals between them, when a *given* tone is sounded. To find the distances of the nodes and loops from the extremities of the tubes, he has recourse to the hypotheses of the old theory, which make the closed end the position of a node, and the open end the position of a loop. This, he says, will not be sensibly different from the truth, if, in the one case, the stop be very unyielding, and, in the other, the diameter of the tube be small. Recent researches on this subject, which we shall presently speak of, show that when the diameter is not very small the position of the loop is perceptibly distant from the open end.

The latter part of M. Poisson's memoir contains an application of the principles of the foregoing part to the vibrations of air in a tube composed of two or more cylinders of different diameters, and to the motion of two different fluids superimposed in the same tube. In the course of this latter inquiry, the author determines the reflection which sound experiences at the junction of two fluids; and by an extension of like considerations to luminous undulations, obtains the same expressions for the relative intensities of light perpendicularly incident, and reflected at a plane surface, as those given by Dr. Young in the Article Chromatics of the *Supplement to the Encyclopædia Britannica*. This subject was afterwards resumed by M. Poisson at greater length in a very elaborate memoir "On the Motion of two Elastic Fluids superimposed †," which is chiefly remarkable for the bearing which the results have upon the theory of light.

At the last meeting, in May this year, of the Philosophical Society of Cambridge, a paper was read by Mr. Hopkins, in which,

* *Mémoires de l'Académie des Sciences*, Paris, An 1817.
† *Ibid.* tom. x. p. 317.

by combining analysis with a delicate set of experiments, results are obtained which are a valuable addition to this part of the theory of fluid motion. His experiments were made on a tube open at both ends, and the column of air within it was put in motion by the vibrations of a plate of glass applied close to one end. The following are the principal results. The nodes are not points of quiescence, but of minimum vibration;—the extremity of the tube most remote from the disturbance is not a place of maximum vibration, but the whole system of places of maximum and minimum vibration is shifted in a very sensible degree *towards* it;—the distances of the places of maximum and minimum vibration from each other, and from that extremity, remain the same for the same disturbance, whatever be the length of the tube. This last fact Mr. Hopkins proves by his analysis must obtain. The shifting of the places of maximum and minimum vibration is not accounted for by the theory: nor is it probable that it can be, unless the consideration of the mode of action of the vibrations on the external air be entered upon,—an important inquiry, but, as I said before, one of great difficulty. I think also that the effect of the vibrations of the tube itself on the contained air ought to be taken into account.

IV. The problem of waves at the surface of water is principally interesting as furnishing an exercise of analysis. The general differential equations of fluid motion assume a very simple form for the case of oscillations of small velocity and extent, and seem to offer a favourable opportunity for the application of analytical reasoning. Yet mathematicians have not succeeded in giving a solution of the problem in any degree satisfactory, which does not involve calculations of a complex nature. We need not stay to inquire in what way Newton found the velocity of the propagation of waves to vary as the square root of their breadths: he was himself aware of the imperfection of his theory. The question cannot be well entered upon without partial differential equations. Laplace was the first to apply to it a regular analysis. His essay is inserted at the end of a memoir on the oscillations of the sea and the atmosphere, in the volume of the Paris Academy of Sciences for the year 1776. The differential equations of the motion are there formed on the supposition that the velocities and oscillations are always so small that their products, and the powers superior to the first, may be neglected. The problem without this limitation becomes so complicated that no one has dared to attempt it. Laplace's reasoning conducts to a linear partial differential equation of the second order, consisting of two terms, which is readily integrated; but on account of the difficulty of obtaining a general solution from this integral, he makes a particular supposition, which is equivalent to considering the fluid to be deranged from its state of equilibrium by causing the surface in its whole extent to take the form of a trochoid, *i. e.* a serpentine curve, of which the vertical ordinate varies as the cosine of the horizontal abscissa. The solution in question is of so limited a nature, that we may dispense with stating the results arrived at.

In the volume of the *Memoirs of the Academy of Berlin* for the year 1786, Lagrange has given* a very simple way of proving, in the Newtonian method of reasoning, that the velocity of propagation of waves along a canal of small and constant depth and uniform width, is that acquired by a heavy body falling through half the depth. In the *Mécanique Analytique*† the same result is obtained analytically. The principal feature of the analysis in this solution is, that the linear partial differential equation of the second order and of four variables, to which the reasoning conducts, is integrated approximately in a series. Lagrange is of opinion, that on account of the tenacity and mutual adherence of the parts of the fluid, the motion extends only to a small distance vertically below the surface agitated by the waves, of whatever depth the fluid may be; and that his solution will consequently apply to a mass of fluid of any depth, and will serve to determine, from the observed velocity of propagation, the distance to which the motion extends downwards.

The problem of waves was proposed by the French Institute for the prize subject of 1816. M. Poisson, whose labours are preeminent in every important question of Hydrodynamics, had already given this his attention. His essay, which was the first deposited in the bureau of the Institute, was read Oct. 2, 1815, just at the expiration of the period allowed for competition. It forms the first part of the memoir "On the Theory of Waves," published in the volume of the Academy for the year 1816, and contains the general formulæ required for the complete solution of the problem, and the theory, derived from these formulæ, of waves propagated with a *uniformly accelerated* motion. In the month of December following, an additional paper was read by M. Poisson on the same subject, which forms the second part of the memoir just mentioned, and contains the theory of waves propagated with a *constant* velocity. These are much more sensible than the waves propagated with an accelerated motion, and are in fact those which are commonly seen to spread in

* p. 192. † Part II. sect. xi. art. 36.

circles round any disturbance made at the surface of water. No theory of waves which does not embrace these can be considered complete. In the essay of M. Cauchy, which obtained the prize, and is printed in the *Mémoires des Savans**, the theory of only the first kind of waves is given. This essay, however, claims to be more complete than the first part of M. Poisson's memoir, because it leaves the function relative to the initial form of the fluid surface entirely arbitrary, and consequently allows of applying the analysis to any form of the body immersed to produce the initial disturbance. M. Poisson restricts his reasoning to a body, of the form of an elliptic paraboloid, immersed a little in the fluid, with its vertex downward and axis vertical; and as this form may have a contact of the second order, with any continuous surface, the reasoning may be legitimately extended to any bodies of a *continuous* form, but not to such as have summits or edges, like the cone, cylinder and prism. This restriction having been objected to as a defect in the theory†, M. Poisson answers‡ that his analysis is not at fault, but that one of the differential equations of the problem, which expresses the condition that the same particles of water remain at the surface during the whole time of motion, very much restricts the form which the immersed body may be supposed to have. When the initial motion is produced by the immersion of a body whose surface presents summits or edges, it is not possible, he thinks, to represent the velocities of the fluid particles by analytical formulæ, especially at the first instants of the agitation, when the motion must be very complicated, and the same points will not remain constantly at the surface.

With the exception of the particular we have been mentioning, the two essays do not present mathematical processes essentially different in principle. Attached to that of M. Cauchy, which was published subsequently to M. Poisson's memoir, are valuable and copious additions, serving to clear up several points of analysis that occur in the course of the work, and referring chiefly to integration by series and definite integrals, and to the treatment of arbitrary functions. Among these is a lengthened discussion of the theory of the waves uniformly propagated, the existence of which, as indicated by the analysis, had escaped the notice of both mathematicians in their first researches. In this discussion the velocities of propagation are determined of the two foremost waves produced by the immer-

* vol. iii.

† *Bulletin de la Société Philomatique*, Septembre 1818, p. 129.

‡ "Note sur le Problème des Ondes," tom. viii. of *Mémoires de l'Académie des Sciences*, p. 571.

sion and sudden elevation of bodies of the forms of a paraboloid, a cylinder, a cone, and a solid, generated by the revolution of a parabola about a tangent at its vertex. To bodies of the last three forms, M. Poisson objects to extending the reasoning; and in the "Note" above referred to, attempts to show that such an extension leads to results inconsistent with the principle of the coexistence of small vibrations. If we are not permitted to receive the analysis of M. Cauchy in all the generality it lays claim to, we must at least assent to the reasonableness of the following conclusion it pretends to arrive at, viz. that "the heights and velocities of the different waves produced by the immersion of a cylindrical or prismatic body depend not only on the width and height of the part immersed, but also on the form of the surface which bounds this part." There is also much appearance of probability in a remark made by the same mathematician, that the *number* of the waves produced may depend on the form of the immersed body and the depth of immersion.

We proceed to say a few words on the contents of M. Poisson's memoir. He commences by showing, as well by *à priori* reasoning as by an appeal to facts, that Lagrange's solution cannot be extended to fluid of any depth. In his own solution he supposes the fluid to be of any uniform depth, but principally has regard to the case which most commonly occurs of a very great depth: he neglects the square of the velocity of the oscillating particles, as all have done who have attempted this problem, and assumes, that a fluid particle which at any instant is at the surface, remains there during the whole time of the motion. This latter supposition seems necessary for the condition of the continuity of the fluid. With regard to the neglect of the square of the velocity, it does not seem that we can tell to what extent it may affect the calculations so well as in the case of the vibrations of elastic fluids, where the velocity of the vibrating particle is neglected in comparison of a known and constant velocity, that of propagation. M. Poisson treats first the case in which the motion takes place in a canal of uniform width, and, consequently, abstraction is made of one horizontal dimension of the fluid; and afterwards the case in which the fluid is considered in its three dimensions. The former requires for its solution the integration of the same differential equation of two terms* as that occurring in Laplace's theory. No use is made of the common integral of this equation, as, on account of the impossible quantities it involves, it would be difficult

* In M. Poisson's works this equation is $\frac{d^2\varphi}{d x^2}+\frac{d^2\varphi}{d y^2}=0.$

to make it serve to determine the laws of propagation. It is remarkable that this integral is not *necessary* for solving the problem, although, as M. Poisson has shown in his first memoir, "On the Distribution of Heat in Solid Bodies," and M. Cauchy in the Notes added to his "Theory of Waves," a solution may be derived from it equivalent to that which they have given without its aid. We may be permitted to doubt whether its meaning is yet fully understood, and to hope that, by overcoming some difficulty in the interpretation of this integral, the problem of waves may receive a simpler solution than has hitherto been given. Be this as it may, the process of integration adopted by M. Poisson leaves nothing to be wished for in regard to generality. It is easy to obtain an unlimited number of particular equations not containing arbitrary functions, which will satisfy the differential equation in question, and to combine them all in an expression for the principal variable (ϕ), developed in series of real or imaginary exponentials. This will be the most general integral the equation admits of, and (to use the words of M. Poisson,) "there exist theorems, by means of which we may introduce into expressions of this nature, arbitrary functions, which represent the initial state of the fluid: the difficulty of the question consists then in discussing the resulting formulæ, and discovering from them all the laws of the phænomenon. The theory of waves furnishes at present the most complete example of a discussion of this sort."

In a Report like the present, it is not possible to give any very precise idea of the analysis which has been employed for solving the problem of waves. I have thought it proper to call attention to a process of reasoning which has been very extensively employed by the French mathematicians of the present day, and indeed may be considered to be the principal feature of their calculations in the more recent applications of mathematics to physical and mechanical questions. To understand fully the nature and power of the method, the works of Fourier, particularly *The Analytical Theory of Heat*, the Notes, before spoken of, to M. Cauchy's "Theory of Waves," and the two memoirs of M. Poisson "On the Distribution of Heat in Solid Bodies," must be studied. I will just refer to some parts of the writings of the last-mentioned geometer, where he has been careful to state in a concise manner the principle of the method in question. There are some remarks on the generality of a main step in the process in the *Bulletin de la Société Philomatique**. The note before spoken of in the eighth volume of the

* An 1817, p. 180.

Memoirs of the Academy concludes with a brief account of the history and principle of this way of expressing the complete integral by a series of particular integrals, and introducing the arbitrary function. But I would chiefly recommend the perusal of the remarks at the end of a memoir by this author "On the Integration of some linear partial Differential Equations; and particularly the general Equation of the Motion of Elastic Fluids." To the memoir itself I beg to refer, by the way, as presenting a demonstration of the constancy of the velocity of propagation from an irregular disturbance in an elastic fluid, more simple and direct than that in the *Journal de l'Ecole Polytechnique.* It contains also a general integral of the linear partial differential equation of three terms, which occurs in the problem of waves for the case in which the three dimensions of the fluid are taken account of; but the author does not consider this integral of much utility, because of the impossible quantities involved in it, and rather recommends the method of expressing the principal variable by infinite series of exponentials. In fact, in the "Theory of Waves" this case is treated in a manner exactly analogous to that in which abstraction is made of one dimension of the fluid.

It may be useful to state some of the principal results obtained by theory respecting the nature of waves, to give an idea of what the independent power of analysis has been able to effect.

With respect, first, to the canal of uniform width, the law of the velocity of propagation found by Lagrange is confirmed by M. Poisson's theory when the depth is small, but not otherwise.

When the canal is of unlimited depth, the following are the chief results:

(1.) An impulse given to any point of the surface affects instantaneously the whole extent of the fluid mass. The theory determines the magnitude and direction of the initial velocity of each particle resulting from a given impulse.

(2.) "The summit of each wave moves with a uniformly accelerated motion."

This must be understood to refer to a series of very small waves, called by M. Poisson *dents,* which perform their movements as it were on the surface of the larger waves, which he calls "*les ondes dentelées.*" Each wave of the series is found to have its proper velocity, independent of the primitive impulse. Waves of this kind have been actually observed: they are small from the first, and quickly disappear.

(3.) At considerable distances from the place of disturbance, there are waves of much more sensible magnitude than the preceding. Their summits are propagated with a uniform velocity, which varies as the square root of the breadth *à fleur d'eau* of the fluid originally disturbed. Yet the different waves which are formed in succession are propagated with different velocities: the foremost travels swiftest. The amplitude of oscillations of equal duration are reciprocally proportional to the square root of the distances from the point of disturbance.

(4.) The vertical excursions of the particles situated directly below the primitive impulse, vary according to the inverse ratio of the depth below the surface. This law of decrease is not so rapid but that the motion will be very sensible at very considerable depths: it will not be the true law, as the theory proves, when the original disturbance extends over the whole surface of the water, for the decrease of motion in this case will be much more rapid.

The results of the theory, when the three dimensions of the fluid are considered, are analogous to the preceding, (1), (2), (3), (4), and may be stated in the same terms, excepting that the amplitudes of the oscillations are inversely *as the distances* from the origin of disturbance, and the vertical excursions of the particles situated directly below the disturbance vary inversely as the *square* of the depth.

There is a good analysis of M. Poisson's theory, and a comparison of many of the results with experiments, in a Treatise by M. Weber, entitled *Wellenlehre auf Experimente gegründet**. The experiments of M. Weber were made in a manner not sufficiently agreeing with the conditions supposed in the theory to be a correct test of it. They, however, manifest a general accordance with it, and confirm the existence of the small accelerated waves near the place of disturbance, and of a sensible motion of the fluid particles at considerable depths below the surface. In one particular, in which the theory admits of easy comparison with experiment, it is not found to agree. When the body employed to cause the initial agitation of the water is an elliptic paraboloid, with its vertex downwards and axis vertical, and consequently the section in the plane of the surface of the water an ellipse, theory determines the velocity of propagation to be greater in the direction of the major axis than in that of the minor in the proportion of the square root of the one to the square root of the other. This result, which it must be confessed has not an appearance of probability, is not borne out by experience.

* Leipzig, 1825.

The theory has been also put to the test of experiment by M. Bidone, who succeeded in overcoming in great measure an obstacle in the way of making the experiments according to the conditions supposed in the theory, arising from the adhesion of the water to the immersed body*. His observations confirm the existence and laws of motion of the accelerated waves.

V. Scarcely anything worth mentioning has been effected by theory in regard to the resistance of fluids to bodies moving in them. The defect of every attempt hitherto made has arisen from its proceeding upon some hypothesis respecting the law of the resistance; for instance, that it varies as the velocity, or as the square of the velocity: whereas the law, which cannot be known *à priori*, ought to be a result of the calculation, which should embrace not only the motion of the body, but that of every particle of the fluid which moves simultaneously with it. The only problem that has been attempted to be solved on this principle, is one of very considerable interest, relating to the correction to be applied to the pendulum to effect the reduction to a vacuum. The memoir of M. Poisson, "On the Simultaneous Motions of a Pendulum and of the surrounding Air," was read before the Royal Academy of Paris in August 1831, and is inserted in vol. xi. of their *Memoires*. He takes the case of a spherical ball suspended by a very slender thread, the effect of which is neglected in the calculations; the ball is supposed to perform oscillations of very small amplitude, so that the air in contact with its surface is sensibly the same during the motion. A simpler problem of resistance cannot be conceived. M. Poisson considers the effect which the friction of the particles of air against the surface of the ball may have on its motion, and comes to the conclusion that the time of the oscillations is not affected by it, but only their extent. The most important result of the theoretical calculation is, that the correction which has been usually applied for the reduction to a vacuum, and calculated without considering the motion of the air, must be increased by one half. This he finds to agree sufficiently with some experiments of Captain Sabine. He also adduces forty-four experiments of Dubuat, made fifty years ago, upon oscillations in water, and three upon oscillations in air. These give nearly the same numerical result, and agreeing nearly with the value $1\frac{1}{2}$. The experiments, however, of M. Bessel give results which coincide with Dubuat's for oscillations in water, but determine the correction in air for reduction to a vacuum to be very nearly double that hitherto

* See vol. xxv. of the *Memoirs of the Royal Academy of Turin.*

applied, instead of once and a half. M. Poisson thinks that the calculations of M. Bessel leave some room for doubt, and objects to the discordance of the values obtained for air and water, which, according to his own theory, ought to agree. More recent experiments of Mr. Baily *, which, from their number and variety, and the care taken in performing them, are entitled to the utmost confidence, give the value 1·864 for spheres of different materials one inch and a half in diameter, and 1·748 for spheres two inches in diameter, the latter being nearly the size of those for which M. Bessel obtained 1·946. The theory of M. Poisson does not recognise any difference in the value of the coefficient for spheres of different diameters. The discrepancies that thus appear between theory and experiment, and between the experiments themselves, show that there is much that requires clearing up in this important subject. As far as theory is concerned, it is easily conceivable that much must depend upon the way in which the law of transmission of the motion from the parts of the fluid immediately acted on by the sphere to the parts more remote is to be determined: and, as it is the province of this Report to point out any possible source of error in theory, I will venture again to express my doubts of the correctness of the principle employed in the solution of this problem, of making the determination of the law of transmission depend on the arbitrary discontinuity of the functions introduced by integration, the law itself not being arbitrary †.

A singular fact, relating to the resistance to the motion of bodies partly immersed in water, has been recently established by experiments on canal navigation, by which it appears that a boat, drawn with a velocity of more than four or five miles an hour, rises perceptibly out of the water, so that the water-line is not so distant from the keel as in a state of rest, and the resistance is less than it would be if no such effect took place. Theory, although it has never predicted anything of this nature, now that the fact is proposed for explanation, will probably soon be able to account for it on known mechanical principles.

The foregoing review of the theory of fluid motion, incom-

* *Philosophical Transactions* for 1832, p. 399.

† In an attempt at this problem made by myself, and published subsequently to the Meeting of the Association, the value of the coefficient is found to be 2, without accounting for any difference for spheres of different diameters. See the *London and Edinburgh Philosophical Magazine and Journal* for September 1833.

plete as it is, may suffice to show that this department of science is in an extremely imperfect state. Possibly it may on that account be the more likely to receive improvements; and I am disposed to think that such will be the case. But these improvements, I expect, will be available not so much in practical applications, as in reference to the great physical questions of light, heat and electricity, which have been so long the subjects of experiment, and the theories of which require to be perfected. For this purpose a more complete knowledge of the analytical calculation proper for the treatment of fluids in motion may be of great utility.

[illegible]

Report on the Progress and Present State of our Knowledge of Hydraulics as a Branch of Engineering. By George Rennie, *Esq., F.R.S., &c. &c.*

Part I.

The paper now communicated to the British Association for the Advancement of Science comprises a Report on the progress and present state of our knowledge of Hydraulics as a branch of Engineering, with reference to the principles already established on that subject.

Technically speaking, the term hydraulics signifies that branch of the science of hydrodynamics which treats of the motion of fluids issuing from orifices and tubes in reservoirs, or moving in pipes, canals or rivers, oscillating in waves, or opposing a resistance to the progress of solid bodies at rest.

We can readily imagine that if a hole of given dimensions be pierced in the sides or bottom of a vessel kept constantly full, the expenditure ought to be measured by the amplitude of the opening, and the height of the liquid column.

If we isolate the column above the orifice by a tube, it appears evident that the fluid will fall freely, and follow the laws of gravity. But experiment proves that this is not exactly the case, on account of the resistances and forces which act in a contrary direction, and destroy part of, or the whole, effect. The development of these forces is so extremely complicated that it becomes necessary to adopt some auxiliary hypothesis or abbreviation in order to obtain approximate results. Hence the science of hydrodynamics is entirely indebted to experiment. The fundamental problem of it is to determine the efflux of a vein of water or any other fluid issuing from an aperture made in the sides or bottom of a vessel kept constantly full, or allowed to empty itself. Torricelli had demonstrated that, abstracting the resistances, the velocities of fluids issuing from very small orifices followed the subduplicate ratio of the pressures. This law had been, in a measure, confused by subsequent writers, in consequence of the discrepancies which appeared to exist between the theory and experiment; until Varignon remarked, that when water escaped from a small opening made in the bottom of a cylindrical vessel, there appeared to be very little, or scarcely any, sensible motion in the particles of the water; from which he concluded that the law of acceleration existed, and that the particles which escaped at every instant of time received their motion simply from the pressure produced by the weight of the fluid column above the orifice, and that the weight of this column of fluid ought to represent the pressure on the particles which continually escape from the orifice; and that the quantity of motion or expenditure is in the ratio of the breadth of the orifice, multiplied by the square of the velocity, or, in other words, that the height of the water in the vessel is proportional to the square of the velocity with which it escapes; which is precisely the theorem of Torricelli. This mode of reasoning is in some degree vague, because it supposes that the small mass which escapes from the vessel at each instant of time acquires its velocity from the pressure of the column immediately above the orifice. But supposing, as is natural, that the weight of the column acts on the particle during the time it takes to issue from the vessel, it is clear that this particle will receive an accelerated motion, whose quantity in a given time will be proportional to the pressure multiplied by the time: hence the product of the weight of the column by the time of its issuing from the orifice, will be equal to the product of the mass of this particle by the velocity it will have acquired; and as the mass is the product of the opening of the orifice, by the small space which the particle describes in issuing from the orifice, it follows that the height of the column will be as the square of the velocity acquired. This theory is the more correct the more the fluid approaches to a perfect state of repose, and the more the dimensions of the vessel exceed the dimensions of the orifice. By a contrary mode of reasoning this theory became insufficient to determine the motions of fluids through pipes of small diameters. It is necessary, therefore, to consider all the motions of the particles of fluids, and examine how they are changed and altered by the figure of the conduit. But experiment teaches us that when a pipe has a different direction from the vertical one, the different horizontal sections of the fluid preserve their parallelism, the sections following taking the place of the preceding ones, and so on; from which it follows (on account of the incompressibility of the fluid) that the velocity of each horizontal section or plate, taken vertically, ought to be in the inverse ratio of the diameter of the section. It suffices, therefore, to determine the motion of a single section, and the problem then becomes analogous to the vibration of a compound pendulum, by which, according to the theory of James Bernoulli, the motions acquired and lost at each instant of time form an equilibrium, as may be supposed to take place with the different sections of a fluid in a pipe, each section being animated with velocities acquired and lost at every instant of time.

The theory of Bernoulli had not been proposed by him until long after the discovery of the indirect principle of *vis viva* by Huygens. The same was the case with the problem of the motions of fluids issuing from vessels, and it is surprising that no advantage had been taken of it earlier. Michelotti, in his experimental researches *de Separatione Fluidorum in Corpore Animali*, in rejecting the theory of the Newtonian cataract, (which had been advanced in Newton's *Mathematical Principles*, in the year 1687, but afterwards corrected in the year 1714,) supposes the water to escape from an orifice in the bottom of a vessel kept constantly full, with a velocity produced by the height of the superior surface; and that if, immediately above the lowest plate of water escaping from the orifice, the column of water be frozen, the weight of the column will have no effect on the velocity of the water issuing from the orifice; and that if this solid column be at once changed to its liquid state, the effect will remain the same. The Marquis Poleni, in his work *De Castellis per quæ derivantur Fluviorum Aquæ*, published at Padua in the year 1718, shows, from many experiments, that if A be the orifice, and H the height of the column above it, the quantity of water which issues in a given time is represented by $2\,A\,H \times \frac{0\cdot571}{1\cdot000}$, whereas if it spouted out from the orifice with a velocity acquired by falling from the height H, it ought to be exactly 2 A H, so that experiment only gives a little more than half the quantity promised by the theory; hence, if we were to calculate from these experiments the velocity that the water ought to have to furnish the necessary quantity, we should find that it would hardly make it reascend $\frac{1}{3}$rd of its height. These experiments would have been quite contrary to expectation, had not Sir Isaac Newton observed that water issuing from an orifice $\frac{5}{8}$ths of an inch in diameter, was contracted $\frac{2\frac{1}{2}}{}$ths of the diameter of the orifice, so that the cylinder of water which actually issued was less than it ought to have been, according to the theory, in the ratio of 441 to 625; and augmenting it in this proportion, the opening should have been $2\,A\,H\,\frac{0\cdot805}{1\cdot000}$, or $\frac{4}{5}$ths of the quantity which ought to have issued on the supposition that the velocity was in the ratio of the square root of the height; from which it was inferred that the theory was correct, but that the discrepancy was owing to certain resistances, which experiment could alone determine. The accuracy of the general conclusion was affected by several assumptions, namely, the perfect fluidity and sensibility of the mass, which was neither affected by friction nor cohesion, and an infinitely small thickness in the edge of the aperture.

Daniel Bernoulli, in his great work, *Hydrodynamica, seu de Viribus et Motibus Fluidorum Commentaria*, published at Strasburgh in the year 1738, in considering the efflux of water from an orifice in the bottom of a vessel, conceives the fluid to be divided into an infinite number of horizontal strata, on the following suppositions, namely, that the upper surface of the fluid always preserves its horizontality; that the fluid forms a continuous mass; that the velocities vary by insensible gradations, like those of heavy bodies; and that every point of the same stratum descends vertically with the same velocity, which is inversely proportional to the area of the base of the stratum; that all sections thus retaining their parallelism are contiguous, and change their velocities imperceptibly; and that there is always an equality between the vertical descent and ascent, or *vis viva*: hence he arrives, by a very simple and elegant process, to the equations of the problem, and applies its general formulæ to several cases of practical utility. When the figure of the vessel is not subject to the law of continuity, or when sudden and finite changes take place in the velocities of the sections, there is a loss of *vis viva*, and the equations require to be modified. John Bernoulli and Maclaurin arrived at the same conclusions by different steps, somewhat analogous to the cataract of Newton. The investigations of D'Alembert had been directed principally to the dynamics of solid bodies, until it occurred to him to apply them to fluids; but in following the steps of Bernoulli he discovered a formula applicable to the motions of fluid, and reducible to the ordinary laws of hydrostatics. The application of his theory to elastic and non-elastic bodies, and the determination of the motions of fluids in flexible pipes, together with his investigations relative to the resistance of pipes, place him high in the ranks of those who have contributed to the perfection of the science.

The celebrated Euler, to whom every branch of science owes such deep obligations, seems to have paid particular attention to the subject of hydrodynamics; and in attempting to reduce the whole of it to uniform and general formulæ, he exhibited a beautiful example of the application of analytical investigation to the solution of a great variety of problems for which he was so famous. The *Memoirs of the Academy of Berlin*, from the year 1768 to 1771, contain numerous papers relative to fluids flowing from orifices in vessels, and through pipes of constant or variable diameters. "But it is greatly to be regretted," says M. Prony, "that Euler had not treated of friction and cohesion, as his theory of the linear motion of air would have applied to the motions of fluids through pipes and conduits, had he not always reasoned on the hypotheses of mathematical fluidity, independently of the resistances which modify it."

In the year 1765 a very complete work was published at Milan by Paul Lecchi, a celebrated Milanese engineer, entitled *Idrostatica esaminata ne' suoi Principj e Stabilite nelle suoi Regole della Mensura della Acque correnti*, containing a complete examination of all the different theories which had been proposed to explain the phænomena of effluent water, and the doctrine of the resistance of fluids. The author treats of the velocity and quantity of water, whether absolutely or relatively, which issues from orifices in vessels and reservoirs, according to their different altitudes, and inquires how far the law applies to masses of water flowing in canals and rivers, the velocities and quantities of which he gives the methods of measuring. The *extensive and successful practice* of Lecchi as an engineer added much to the reputation of his work*.

In the year 1764 Professor Michelotti of Turin undertook, at the expense of the King of Sardinia, a very extensive series of experiments on running water issuing through orifices and additional tubes placed at different heights in a tower of the finest masonry, twenty feet in height and three feet square inside. The water was supplied by a channel two feet in width, and under pressures of from five to twenty-two feet. The effluent waters were conveyed into a reservoir of ample area, by canals of brick-work lined with stucco, and having various forms and declivities; and the experiments, particularly on the efflux of water through differently shaped orifices, and additional tubes of different lengths, were most numerous and accurate, and Michelotti was the first who gave representations of the changes which take place in the figure of the fluid vein, after it has issued from the orifice. His experiments on the velocities of rivers, by means of the bent tube of Pitot, and by an instrument resembling a water-wheel, called the *stadera idraulica*, are numerous and interesting; but, unfortunately, their reduction is complicated with such various circumstances that it is difficult to derive from them any satisfactory conclusions. But Michelotti is justly entitled to the merit of having made the greatest revolution in the science by experimental

* See also *Memorie Idrostatico-storiche*, 1773.

investigation*. The example of Michelotti gave a fresh stimulus to the exertions of the French philosophers, to whom, after the Italians, the science owes the greatest obligations. Accordingly, the Abbé Bossut, a most zealous and enlightened cultivator of hydrodynamics, undertook, at the expense of the French Government, a most extensive and accurate series of experiments, which he published in the year 1771, and a more enlarged edition, in two volumes, in the year 1786, entitled *Traité Théorique et Expérimental d'Hydronamique*. The first volume treats of the general principles of hydrostatics and hydraulics, including the pressure and equilibrium of non-elastic and elastic fluids against inflexible and flexible vessels; the thickness of pipes to resist the pressure of stagnant fluids; the rise of water in barometers and pumps, and the pressure and equilibrium of floating bodies; the general principles of the motions of fluids through orifices of different shapes, and their friction and resistance against the orifices; the oscillations of water in siphons; the percussion and resistance of fluids against solids; and machines moved by the action and reaction of water. The second volume gives a great variety of experiments on the motions of water through orifices and pipes and fountains; their resistances in rectangular or curvilinear channels, and against solids moving through them; and lastly, of the fire- or steam-engine. In the course of these experiments he found that when the water flowed through an orifice in a thin plate, the contraction of the fluid vein diminished the discharge in the ratio of 16 to 10; and when the fluid was discharged through an additional tube, two or three inches in length, the theoretical discharge was diminished only in the ratio of 16 to 13. In examining the effects of friction, Bossut found that small orifices discharged less water in proportion than large ones, on account of friction, and that, as the height of the reservoir augmented, the fluid vein contracted likewise; and by combining these two circumstances together, he has furnished the means of measuring with precision the quantity of water discharged either from simple orifices or additional tubes, whether the vessels be constantly full, or be allowed to empty themselves. He endeavoured to point out the law by which the diminution of expenditure takes place, according to the increase in the length of the pipe or the number of its bends; he examined the effect of friction in diminishing the velocity of a stream in rectangular and curvilinear channels; and showed that in an open canal, with the same

* *Sperimenti Idraulici,* 1767 and 1771.

height of reservoir, the same quantity of water is always discharged, whatever be the declivity and length; that the velocities of the waters in the canal are not as the square roots of the declivities, and that in equal declivities and depth of the canal the velocities are not exactly as the quantities of water discharged; and he considers the variations which take place in the velocity and level of the waters when two rivers unite, and the manner in which they establish their beds.

His experiments, in conjunction with D'Alembert and Condorcet, on the resistance of fluids, in the year 1777, and his subsequent application of them to all kinds of surfaces, including the shock and resistance of water-wheels, have justly entitled him to the gratitude of posterity. The Abbé Bossut had opened out a new career of experiments; but the most difficult and important problem remaining to be solved related to rivers. It was easy to perform experiments with water running through pipes and conduits on a small scale, under given and determined circumstances: but when the mass of fluid rolled in channels of unequal capacities, and which were composed of every kind of material, from the rocks amongst which it accumulated to the gravel and sand through which it forced a passage,—at first a rapid and impetuous torrent, but latterly holding a calm and majestic course,—sometimes forming sand-banks and islands, at other times destroying them, at all times capricious, and subject to variation in its force and direction by the slightest obstacles,—it appeared impossible to submit them to any general law.

Unappalled, however, by these difficulties, the Chevalier Buat, after perusing attentively M. Bossut's work, undertook to solve them by means of a theorem which appeared to him to be the key of the whole science of hydraulics. He considered that if water was in a perfect state of fluidity, and ran in a bed from which it experienced no resistance whatever, its motion would be constantly accelerated, like the motion of a heavy body descending an inclined plane; but as the velocity of a river is not accelerated *ad infinitum*, but arrives at a state of uniformity, it follows that there exists some obstacle which destroys the accelerating force, and prevents it from impressing upon the water a new degree of velocity. This obstacle must therefore be owing either to the viscidity of the water, or to the resistance it experiences against the bed of the river; from which Dubuat derives the following principle:—That when water runs uniformly in any channel, the accelerating force which obliges it to run is equal to the sum of all the resistances which it experiences, whether arising from the viscidity of the water or the friction of its bed. Encouraged by this discovery, and by the application of its principles to the solution of a great many cases in practice, Dubuat was convinced that the motion of water in a conduit pipe was analogous to the uniform motion of a river, since in both cases gravity was the cause of motion, and the resistance of the channel or perimeter of the pipes the modifiers. He then availed himself of the experiments of Bossut on conduit pipes and artificial channels to explain his theory: the results of which investigations were published in the year 1779. M. Dubuat was, however, sensible that a theory of so much novelty, and at variance with the then received theory, required to be supported by experiments more numerous and direct than those formerly undertaken, as he was constrained to suppose that the friction of the water did not depend upon the pressure, but on the surface and square of the velocity. Accordingly, he devoted three years to making fresh experiments, and, with ample funds and assistance provided by the French Government, was enabled to publish his great work, entitled *Principes d'Hydraulique vérifiés par un grand nombre d'Expériences, faites par Ordre du Gouvernement*, 2 vols. 1786, (a third volume, entitled *Principes d'Hydraulique et Hydronamique*, appeared in 1816);—in the first instance, by repeating and enlarging the scale of Bossut's experiments on pipes (with water running in them) of different inclinations or angles, of from 90° to $\frac{1}{40,000}$th part of a right angle, and in channels of from $1\frac{1}{2}$ line in diameter to 7 and 8 square toises of surface, and subsequently to water running in open channels, in which he experienced great difficulties in rendering the motion uniform: but he was amply recompensed by the results he obtained on the diminution of the velocity of the different parts of a uniform current, and of the relation of the velocities at the surface and bottom, by which the water works its own channel, and by the knowledge of the resistances which different kinds of beds produce, such as clay, sand and gravel; and varying the experiments on the effect of sluices, and the piers of bridges, &c., he was enabled to obtain a formula applicable to most cases in practice*.

Thus, let V = mean velocity per second, in inches.

d = hydraulic mean depth, or quotient which arises from dividing the area or section of the canal, in square inches, by the perimeter of the part in contact with the water, in linear inches.

s = the slope or declivity of the pipe, or the surface of the water.

g = 16·087, the velocity in inches which a body acquires in falling one second of time.

* *Edinburgh Encyclopedia*, Art. HYDRODYNAMICS, by Brewster.

n = an abstract number, which was found by experiment to be equal to 243·7.

$$\text{then } v = \frac{\sqrt{n\,g}\,(\sqrt{d} - 0{\cdot}1)}{\sqrt{s} - \log.\ \sqrt{s} + 1{\cdot}6} - 0{\cdot}3\,(\sqrt{d} - 0{\cdot}1).$$

Such are some of the objects of M. Dubuat's work. But his hypotheses are unfortunately founded upon assumptions which render the applications of his theory of little use. It is evident that the supposition of a constant and uniform velocity in rivers cannot hold: nevertheless he has rendered great services to the science by the solution of many important questions relating to it; and although he has left on some points a vast field open to research, he is justly entitled to the merit of originality and accuracy.

Contemporary with Dubuat was M. Chezy, one of the most skilful engineers of his time: he was director of the Ecole des Ponts et Chaussées, and reported, conjointly with M. Perronet, on the Canal Yvette. He endeavoured to assign, by experiment, the relation existing between the inclination, length, transversal section, and velocity of a canal. In the course of this investigation he obtained a very simple expression of the velocity, involving three different variable quantities, and capable, by means of a single experiment, of being applied to all currents whatever. He assimilates the resistance of the sides and bottom of the canal to known resistances, which follow the law of the square of the velocity, and he obtains the following simple formula:

$v = \sqrt{\frac{g\,d}{z\,s}}$, where g is = 16·087 feet, the velocity acquired by a heavy body after falling one second.

d = hydraulic mean depth, equal to the area of the section divided by the perimeter of the part of the canal in contact with the water.

s = the slope or declivity of the pipe.

z = an abstract number, to be determined by experiment.

In the year 1784, M. Lespinasse published in the *Memoirs of the Academy of Sciences at Toulouse* two papers, containing some interesting observations on the expenditure of water through large orifices, and on the junction and separation of rivers. The author had performed the experiments contained in his last paper on the rivers Fresquel and Aude, and on that part of the canal of Languedoc below the Fresquel lock, towards its junction with that river.

As we before stated, M. Dubuat had classified with much sagacity his observations on the different kinds of resistance experienced in the motion of fluids, and which might have led him to express the sum of the resistances by a rational function of the velocity composed of two or three terms only. Yet the merit of this determination was reserved to M. Coulomb, who, in a beautiful paper, entitled "Expériences destinées à déterminer la Cohérence des Fluides et les Lois de leurs Résistances dans les Mouvemens très lents," proves, by reasoning and facts,

1st, That in extremely slow motions the part of the resistance is proportional to the square of the velocity.

2ndly, That the resistance is not sensibly increased by increasing the height of the fluid above the resisting body.

3rdly, That the resistance arises solely from the mutual cohesion of the fluid particles, and not from their adhesion to the body upon which they act.

4thly, That the resistance in clarified oil, at the temperature of 69° Fahrenheit, is to that of water as 17·5 : 1; a proportion which expresses the ratio of the mutual cohesion of the particles of oil to the mutual cohesion of the particles of water.

M. Coulomb concludes his experiments by ascertaining the resistance experienced by cylinders that move very slowly and perpendicularly to their axes, &c.

This eminent philosopher, who had applied the doctrine of torsion with such distinguished success in investigating the phænomena of electricity and magnetism, entertained the idea of examining in a similar manner the resistance of fluids, contrary to the doctrines of resistance previously laid down. M. Coulomb proved, that in the resistance of fluids against solids, there was no constant quantity of sufficient magnitude to be detected; and that the pressure sustained by a moving body is represented by two terms, one which varies as the simple velocity, and the other with its square.

The apparatus with which these results were obtained consisted of discs of various sizes, which were fixed to the lower extremity of a brass wire, and were made to oscillate under a fluid by the force of torsion of the wire. By observing the successive diminution of the oscillations, the law of resistance was easily found. The oscillations which were best suited to these experiments continued for twenty or thirty seconds, and the amplitude of the oscillation (that gave the most regular results) was between 480 the entire division of the disc, and 8 or 10 divisions from zero.

The first who had the happy idea of applying the law of Coulomb to the case of the velocities of water running in natural or artificial channels was M. Girard, Ingénieur en chef des Ponts et Chaussées, and Director of the Works of the Canal l'Ourcq at Paris *.

He is the author of several papers on the theory of running waters, and of a valuable series of experiments on the motions of fluids in capillary tubes.

M. Coulomb had given a common coefficient to the two terms of his formula representing the resistance of a fluid,—one proportional to the simple velocity, the other to the square of the velocity. M. Girard found that this identity of the coefficients was applicable only to particular fluids under certain circumstances; and his conclusions were confirmed by the researches of M. Prony, derived from a great many experiments, which make the coefficients not only different, but very inferior to the value of the motion of the filaments of the water contiguous to the side of the pipe.

The object of M. Girard's experiments was to determine this velocity; and this he has effected in a very satisfactory manner, by means of twelve hundred experiments, performed with a series of copper tubes, from 1·83 to 2·96 millimetres in diameter, and from 20 to 222 centimetres in length; from which it appeared, that when the velocity was expressed by 10, and the temperature was 0, centigrade, the velocity was increased four times when the temperature amounted to 85°. When the length of the capillary tube was below that limit, a variation of temperature exercised very little influence upon the velocity of the issuing fluid, &c.

It was in this state of the science that M. Prony (then having under his direction different projects for canals,) undertook to reduce the solutions of many important problems on running water to the most strict and rigorous principles, at the same time capable of being applied with facility to practice.

For this purpose he selected fifty-one experiments which corresponded best on conduit pipes, and thirty-one on open conduits. Proceeding, therefore, on M. Girard's theory of the analogy between fluids and a system of corpuscular solids or material bodies, gravitating in a curvilinear channel of indefinite length, and occupying and abandoning successively the different parts of the length of channel, he was enabled to express the velocity of the water, whether it flows in pipes or in open conduits, by a simple formula, free of logarithms, and requiring merely the extraction of the square root †.

* *Essai sur le Mouvement des Eaux courantes:* Paris 1804. *Recherches sur les Eaux publiques, &c. Devis général du Canal l'Ourcq, &c.*

† *Mémoires des Savans Etrangers, &c.* 1815.

Thus $v = -0{\cdot}0469734 + \sqrt{0{\cdot}0022065 + 3041{\cdot}47 \times G}$, which gives the velocity in metres: or, in English feet,

$$v = -0{\cdot}1541131 + \sqrt{0{\cdot}023751 + 32806{\cdot}6 \times G}.$$

When this formula is applied to pipes, we must take $G = \frac{1}{4} D K$, which is deduced from the equation $K = \frac{H + Z = H}{L}$. When it is applied to canals, we must take $G = R I$, which is deduced from the equation $I = \frac{Z}{L}$, R being equal to the mean radius of Dubuat on the hydraulic mean depth, and I equal to the sine of inclination in the pipe or canal. M. Prony has drawn up extensive Tables, in which he has compared the observed velocities with those which are calculated from the preceding formulæ, and from those of Dubuat and Girard. In both cases the coincidence of the observed results with the formulæ are very remarkable, but particularly with the formulæ of M. Prony. But the great work of M. Prony is his *Nouvelle Architecture Hydraulique*, published in the year 1790. This able production is divided into five sections, viz. Statics, Dynamics, Hydrostatics, Hydrodynamics, and on the physical circumstances that influence the motions of Machines. The chapter on hydrodynamics is particularly copious and explanatory of the motions of compressible and incompressible fluids in pipes and vessels, on the principle of the parallelism of the fluid filaments, and the efflux of water through different kinds of orifices made in vessels kept constantly full, or permitted to empty themselves; he details the theory of the clepsydra, and the curves described by spouting fluids; and having noticed the different phænomena of the contraction of the fluid vein, and given an account of the experiments of Bossut, M. Prony deduces formulæ by which the results may be expressed with all the accuracy required in practice.

In treating of the impulse and resistance of fluids, M. Prony explains the theory of Don George Juan, which he finds conformable to the experiments of Smeaton, but to differ very materially from the previously received law of the product of the surfaces by the squares of the velocities, as established by the joint experiments of D'Alembert, Condorcet and Bossut, in the year 1775. The concluding part of the fourth section is devoted to an examination of the theory of the equilibrium and motion of fluids according to Euler and D'Alembert; and by a rigorous investigation of the nature of the questions to be determined, the whole theory is reduced to two equations only, in narrow pipes, according to the theory of Euler, showing its approximation to the hypothesis of the parallelism of filaments.

The fifth and last section investigates the different circumstances (such as friction, adhesion and rigidity,) which influence the motions of machines.

A second volume, published in the year 1796, is devoted to the theory and practice of the steam-engine. Previously to the memoir of M. Prony, *Sur le Jaugeage des Eaux courantes*, in the year 1802, no attempt had been made to establish with certainty the correction to be applied to the theoretical expenditures of fluids through orifices and additional tubes. The phænomenon had been long noticed by Sir Isaac Newton, and illustrated by Michelotti by a magnificent series of experiments, which, although involving some intricacies, have certainly formed the groundwork of all the subsequent experiments upon this particular subject.

By the method of interpolation, M. Prony has succeeded in discovering a series of formulæ applicable to the expenditures of currents out of vertical and horizontal orifices, and to the contraction of the fluid vein; and in a subsequent work, entitled *Recherches sur le Mouvemens des Eaux courantes*, he establishes the following formulæ for the mean velocities of rivers.

When $V =$ velocity at the surface,
and $U =$ mean velocity,
$U = 0{\cdot}816458\ V$,
which is about $\frac{4}{5}\ V$.

These velocities are determined by two methods. 1st, By a small water-wheel for the velocity at the surface, and the improved tube of Pitot for the velocities at different depths below the surface.

If $h =$ the height of the water in the vertical tube above the level of the current, the velocity due to this height will be determined by the formula $V = \sqrt{2gh} = \sqrt{\overset{\text{metres}}{19{\cdot}606}\,h} = 4{\cdot}429\sqrt{h}$.

When water runs in channels, the inclination usually given amounts to between $\frac{1}{300}$th and $\frac{1}{800}$th part of the length, which will give a velocity of nearly $1\frac{1}{4}$ mile per hour, sufficient to allow the water to run freely in earth. We have seen the inclination very conveniently applied in cases of drainage at $\frac{1}{1200}$th and $\frac{1}{1300}$th, and some rivers are said to have $\frac{1}{8000}$th only.

M. Prony gives the following formulæ, from a great number of observations:

If $U =$ mean velocity of the water in the canal,
$I =$ the inclination of the canal per metre,
$R =$ the relation of the area to the profile of its perimeter,
we shall have

$$U = -\,0{\cdot}07 + \sqrt{0{\cdot}005 + 3233\,.\,R\,.\,I};$$

and for conduit pipes,

calling U = the mean velocity,
Z = the head of water in the inferior orifice of the pipe,
L = the length of the pipe in metres,
D = the diameter of the pipe,

we shall have

$$U = -0{\cdot}0248829 + \sqrt{0{\cdot}000619159 + 717{\cdot}857 \frac{D Z}{L}}$$

or, where the velocity is small,

$$U = 26{\cdot}79 \sqrt{\frac{D Z}{L}};$$

that is, the mean velocities approximate to a direct ratio compounded of the squares of the diameters and heads of water, and inversely as the square root of the length of the pipes: and by experiments made with great care, M. Prony has found that the formula

$$U = -0{\cdot}0248829 + \sqrt{0{\cdot}000619159 + 717{\cdot}857 \frac{D Z}{L}}$$

scarcely differs more or less from experiments than $\frac{1}{40}$ or $\frac{1}{25}$. The preceding formulæ suppose that the horizontal sections, both of the reservoir and the recipient, are great in relation to the transverse section of the pipe, and that the pipe is kept constantly full *.

In comparing the formulæ given for open and close canals, M. Prony has remarked that these formulæ are not only similar, but the constants which enter into their composition are nearly the same; so that either of them may represent the two series of phænomena with sufficient exactness.

The following formula applies equally to open or close canals:

$$U = -0{\cdot}0469734 + \sqrt{\left(0{\cdot}0022065 + 3041{\cdot}47 \frac{D Z}{L}\right)}.$$

But the most useful of the numerous formulæ given by M. Prony for open canals is the following:

* According to Mr. Jardine's experiments on the quantity of water delivered by the Coniston Main from Coniston to Edinburgh, the following is a comparison:

	Scots Pints.
Actual delivery of Coniston Main	189·4
Ditto by Eytelwein's formula	189·77
Ditto by Girard's formula	188·26
Ditto by Dubuat's formula	188·13
Ditto by Prony's simple formula	192·32
Ditto by Prony's tables	180·7

Let g = the velocity of a body falling in one second,
w = the area of the transverse section,
p = the perimeter of that section,
I = the inclination of the canal,
Q = the constant volume of water through the section,
U = the mean velocity of the water,
R = the relation of the area to the perimeter of the section;

then 1st, $0{\cdot}000436\,\mathrm{U} + 0{\cdot}003034\,\mathrm{U}^2 = g\,\mathrm{I}\,\mathrm{R} = g\,\mathrm{I}\,\frac{w}{p}$;

2ndly, $\mathrm{U} = \frac{\mathrm{Q}}{w}$;

3rdly, $\mathrm{R}\,w^2 - 0{\cdot}0000444499\,.\,w\,\frac{\mathrm{Q}}{\mathrm{I}} - 0{\cdot}000309314\,\frac{\mathrm{Q}^2}{\mathrm{I}} = 0.$

This last equation, containing the quantities

$$\mathrm{Q}\,\mathrm{I}\,w \text{ and } \mathrm{R} = \frac{w}{p},$$

shows how to determine one of them, and, knowing the three others, we shall have the following equations:

4thly, $p = \dfrac{g\,\mathrm{I}\,w^3}{0{\cdot}000436\,\mathrm{Q}\,w + 0{\cdot}003034\,\mathrm{Q}^2}$.

5thly, $\mathrm{I} = \dfrac{p\,(0{\cdot}0000444499\,\mathrm{Q}\,w + 0{\cdot}000309314\,\mathrm{Q}^2)}{w^3}$.

6thly, $w = 0{\cdot}000436 \pm \dfrac{\sqrt{[(0{\cdot}000436)^2 + 4(0{\cdot}003034)\,g\,\mathrm{R}\,\mathrm{I}]}\,\mathrm{Q}}{2\,g\,\mathrm{R}\,\mathrm{I}}$.

These formulæ are, however, modified in rivers by circumstances, such as weeds, vessels and other obstacles in the rivers; in which case M. Girard has conceived it necessary to introduce into the formulæ the coefficient of correction = 1·7 as a multiplier of the perimeter, by which the equations will be,

$$p - 1{\cdot}7\,(0{\cdot}000436\,\mathrm{U} + 0{\cdot}003034\,\mathrm{U}^2) = g\,\mathrm{I}\,w.$$

The preceding are among the principal researches of this distinguished philosopher *.

In the year 1798, Professor Venturi of Modena published a very interesting memoir, entitled *Sur la Communication latérale du Mouvement des Fluides.* Sir Isaac Newton was well acquainted with this communication, having deduced from it the propagation of rotary motion from the interior to the exterior of a whirlpool; and had affirmed that when motion is propagated in a fluid, and has passed beyond the aperture, the

* *Recherches Physico-mathématiques sur la Théorie des Eaux courantes,* par M. Prony.

motion diverges from that opening, as from a centre, and is propagated in right lines towards the lateral parts. The simple and immediate application of this theorem cannot be made to a jet or aperture at the surface of still water. Circumstances enter into this case which transform the results of the principle into particular motions. It is nevertheless true that the jet communicates its motion to the lateral parts without the orifice, but does not repel it in a radial divergency. M. Venturi illustrates his theory by experiments on the form and expenditure of fluid veins issuing from orifices, and shows how the velocity and expenditure are increased by the application of additional tubes; and that in descending cylindrical tubes, the upper ends of which possess the form of the contracted vein, the expense is such as corresponds with the height of the fluid above the inferior extremity of the tube. The ancients remarked that a descending tube applied to a reservoir increased the expenditure*. D'Alembert, Euler and Bernoulli attributed it to the pressure of the atmosphere. Gravesend, Guglielmini and others sought for the cause of this augmentation in the weight of the atmosphere, and determined the velocity at the bottom of the tube to be the same as would arise from the whole height of the column, including the height of the reservoir. Guglielmini supposed that the pressure at the orifice below is the same for a state of motion as for that of rest, which is not true. In the experiments he made for that purpose, he paid no regard either to the diminution of expenditure produced by the irregularity of the inner surface of the tubes, or the augmentation occasioned by the form of the tubes themselves. But Venturi established the proposition upon the principle of vertical ascension combined with the pressure of the atmosphere, as follows:

1st, That in additional conical tubes the pressure of the atmosphere increases the expenditure in the proportion of the exterior section of the tube to the section of the contracted vein, whatever be the position of the tube.

2ndly, That in cylindrical pipes the expenditure is less than through conical pipes, which diverge from the contracted vein, and have the same exterior diameter. This is illustrated by experiments with differently formed tubes, as compared with a plate orifice and a cylindrical tube, by which the ratios in point of time were found to be 41″, 31″ and 27″, showing the advantage of the conical tube.

3rdly, That the expenditure may be still further increased,

* "Calix devexus ampliùs rapit."—*Frontinus de Aqueductibus*. See also *Pneumatics* of Hero.

in the ratio of 24 to 10, by a certain form of tube,—a circumstance of which he supposes the Romans were well aware, as appears from their restricting the length of the pipes of conveyance from the public reservoirs to fifty feet; but it was not perceived that the law might be equally evaded by applying a conical frustrum to the extremity of the tube.

M. Venturi then examines the causes of eddies in rivers; whence he deduces from his experiments on tubes with enlarged parts, that every eddy destroys part of the moving force of the current of the river, of which the course is permanent and the sections of the bed unequal, the water continues more elevated than it would have done if the whole river had been equally contracted to the dimensions of its smallest section,—a consequence extremely important in the theory of rivers, as the retardation experienced by the water in rivers is not only due to the friction over the beds, but to eddies produced from the irregularities in the bed, and the flexures or windings of its course: a part of the current is thus employed to restore an equilibrium of motion, which the current itself continually deranges. As respects the contracted vein, it had been pretended by the Marquis de Lorgna* that the contracted vein was nothing else but a continuation of the Newtonian cataract; and that the celerity of the fluid issuing from an orifice in a thin plate is much less than that of a body which falls from the height of the charge. But Venturi proved that the contraction of the vein is incomparably greater than can be produced by the acceleration of gravity, even in descending streams, the contraction of the stream being 0·64, and the velocity nearly the same as that of a heavy body which may have fallen through the height of the charge. These experimental principles, which are in accordance with the results of Bossut, Michelotti and Poleni, are strictly true in all cases where the orifice is small in proportion to the section of the reservoir, and when that orifice is made in a thin plate, and the internal afflux of the filaments is made in an uniform manner round the orifice itself. Venturi then shows the form and contraction of the fluid vein by increased charges. His experiments with the cone are curious; and it would have been greatly to be regretted that he had stopped short in his investigations, but for the more extensive researches of Bidone and Lesbros. M. Hachette, in opposition to the theory of Venturi, assigns, as a cause of the increase by additional tubes, the adhesion of the fluid to the sides of the tubes arising from capillary attraction.

* *Memorie della Società Italiana*, vol. iv.

In the year 1801, M. Eytelwein, a gentleman well known to the public by his translation of M. Dubuat's work into German, (with important additions of his own,) published a valuable compendium of hydraulics, entitled *Handbuch der Mechanik und der Hydraulik,* in which he lays down the following rules.

1. That when water flows from a notch made in the side of a dam, its velocity is as the square of the height of the head of the water; that is, that the pressure and consequent height are as the square of the velocity, the proportional velocities being nearly the same as those of Bossut.
2. That the contraction of the fluid vein from a simple orifice in a thin plate is reduced to 0·64.
3. For additional pipes the coefficient is 0·65.
4. For a conical tube similar to the curve of contraction 0·98.
5. For the whole velocity due to the height, the coefficient by its square must be multiplied by 8·0458.
6. For an orifice the coefficient must be multiplied by 7·8.
7. For wide openings in bridges, sluices, &c., by 6·9.
8. For short pipes 6·6.
9. For openings in sluices without side walls 5·1.

Of the twenty-four chapters into which M. Eytelwein's * work is divided, the seventh is the most important. The late Dr. Thomas Young, in commenting upon this chapter, says:

"The simple theorem by which the velocity of a river is determined, appears to be the most valuable of M. Eytelwein's improvements, although the reasoning from which it is deduced is somewhat exceptionable. The friction is nearly as the square of the velocity, not because a number of particles proportional to the velocity is torn asunder in a time proportionally short,—for, according to the analogy of solid bodies, no more force is destroyed by friction when the motion is rapid than when slow,—but because when a body is moving in lines of a given curvature, the deflecting forces are as the squares of the velocities; and the particles of water in contact with the sides and bottom must be deflected, in consequence of the minute irregularities of the surfaces on which they slide, nearly in the same curvilinear path, whatever their velocity may be. At any rate (he continues) we may safely set out with this hypothesis, that the principal part of the friction is as the square of the velocity, and the friction is nearly the same at all depths †; for Professor Robison found that the time of oscillation of the fluid in a bent

* See Nicholson's translation of Eytelwein's work.

† See my "Experiments on the Friction and Resistance of Fluids," *Philosophical Transactions* for 1831.

tube was not increased by increasing the pressure against the sides, being nearly the same when the principal part was situated horizontally, as when vertically. The friction will, however, vary, according to the surface of the fluid which is in contact with the solid, in proportion to the whole quantity of fluid; that is, the friction for any given quantity of water will be as the surface of the bottom and sides of a river directly, and as the whole quantity in the river inversely; or, supposing the whole quantity of water to be spread on a horizontal surface equal to the bottom and sides, the friction is inversely as the height at which the river would then stand, which is called the hydraulic mean depth*." It is, therefore, calculated that the velocities will be a mean proportional between the hydraulic mean depth and the fall, or $\frac{1}{10}$ths of the velocity per second.

Professor Robison informs us, that by the experiments of Mr. Watt on a canal eighteen feet wide at the top, seven feet at the bottom, and four feet deep, having a fall of four inches per mile, the velocities were seventeen inches per second at the surface, fourteen inches per second in the middle, and ten inches per second at the bottom, making a mean velocity of fourteen inches per second; then finding the hydraulic mean depth, and dividing the area of the section by the perimeter, we have $\frac{50}{20\cdot6}$, or 29·13 inches; and the fall in two miles being eight inches, we have $\sqrt{(8 \times 29\cdot13)} = 15\cdot26$ for the mean proportional of $\frac{1}{10}$ths, or 13·9 inches, which agrees very nearly with Mr. Watt's velocity.

The Professor has, however, deduced from Dubuat's elaborate theories 12·568 inches. But this simple theorem applies only to the straight and equable channels of a river. In a curved channel the theorem becomes more complicated; and, from observations made in the Po, Arno, Rhine, and other rivers, there appears to be no general rule for the decrease of velocity going downwards. M. Eytelwein directs us to deduct from the superficial velocity $\frac{1}{130}$ for every foot of the whole depth. Dr. Young thinks $\frac{9}{10}$ths of the superficial velocity sufficient. According to Major Rennell, the windings of the river Ganges in a length of sixty miles are so numerous as to reduce the declivity of the bed to four inches per mile, the medium rate of motion being about three miles per hour, so that a mean hydraulic depth of thirty feet, as stated to be $\frac{2}{3}$rds of the velocity per second, will be 4·47 feet, or three miles per hour. Again, the river when full has thrice the volume of water in it, and its motion is also accelerated in the proportion of 5 to 3;

* See *Nicholson's Journal* for 1802, vol. iii. p. 31.

and, assuming the hydraulicmean depth to be doubled at the time of the inundation, the velocity will be increased in the ratio of 7 to 5; but the inclination of the surface is probably increased also, and consequently produces a further velocity of from 1·4 to 1·7. M. Eytelwein agrees with Genneté*, that a river may absorb the whole of the water of another river equal in magnitude to itself, without producing any sensible elevation in its surface. This apparent paradox Genneté pretends to prove by experiments, from observing that the Danube absorbs the Inn, and the Rhine the Mayne rivers; but the author evidently has not attended to the fact, as may be witnessed in the junction of rivers in marshes and fenny countries,—the various rivers which run through the Pontine and other marshes in Italy, and in Cambridgeshire and Lincolnshire in this country: hence the familiar expression of the waters being overridden is founded in facts continually observed in these districts. We have also the experiments of Brunings in the *Architecture Hydraulique Générale de Wiebeking*, Wattmann's *Mémoires sur l'Art de construire les Canaux*, and Funk *Sur l'Architecture Hydraulique générale*, which are sufficient to determine the coefficients under different circumstances, from velocities of $\frac{2}{3}$ths to $7\frac{1}{2}$ feet, and of transverse sections from $\frac{1}{}$ to 19135 square feet. The experiments of Dubuat were made on the canal of Jard and the river Hayne; those of Brunings in the Rhine, the Waal and Ifrel; and those of Wattmann in the drains near Cuxhaven.

M. Eytelwein's paper contains formulæ for the contraction of fluid veins through orifices †, and the resistances of fluids passing through pipes and beds of canals and rivers, according to the experiments of Couplet, Michelotti, Bossut, Venturi, Dubuat, Wattmann, Brunings, Funk and Bidone.

In the ninth chapter of the *Handbuch*, the author has endeavoured to simplify, nearly in the same manner as the motion of rivers, the theory of the motion of water in pipes, observing that the head of water may be divided into two parts, one to produce velocity, the other to overcome the friction; and that the height must be as the length and circumference of the section of the pipe directly, or as the diameter,—and inversely as the area of the section, or as the square of the diameter.

* *Expériences sur le Cours des Fleuves, ou Lettre à un Magistrat Hollandais*, par M. Genneté. Paris 1760.

† "Recherches sur le Mouvement de l'Eau, en ayant egard à la Contraction qui a lieu au Passage par divers Orifices, et à la Resistance qui retard le Mouvement, le long des Parois des Vases; par M. Eytelwein,"—*Mémoires de l'Académie de Berlin*, 1814 and 1815.

In the allowance for flexure, the product of its square, multiplied by the sum of the sines of the several angles of inflection, and then by ·0038, will give the degree of pressure employed in overcoming the resistance occasioned by the angles; and deducting this height from the height corresponding to the velocity, will give the corrected velocity*.

M. Eytelwein investigates, both theoretically and experimentally, the discharge of water by compound pipes,—the motions of jets, and their impulses against plane and oblique surfaces, as in water-wheels, in which it is shown that the hydraulic pressure must be twice the weight of the generating column, as deduced from the experiments of Bossut and Langsdorft; and in the case of oblique surfaces, the effect is stated to vary as the square of the sine of the angle of incidence; but for motions in open water about $\frac{2}{5}$ths of the difference of the sine from the radius must be added to this square.

The author is evidently wrong in calculating upon impulse as forming part of the motion of overshot wheels; but his theory, that the perimeter of a water-wheel should move with half the velocity of a given stream to produce a maximum effect, agrees perfectly with the experiments of Smeaton and others†.

The author concludes his highly interesting work by examining the effects of air as far as they relate to hydraulic machines, including its impulse against plane surfaces on siphons

* Hence, if f denote the height due to the friction,
$d =$ the diameter of the pipe,
$a =$ a constant quantity,

we shall have, $$f = V^2 \frac{a\,l}{d} \text{ and } V^2 = \frac{f\,d}{a\,l}.$$

But the height employed in overcoming the friction corresponds to the difference between the actual velocity and the actual height, that is, $f = h - \frac{V^2}{b^2}$, where b is the coefficient for finding the velocity from the height.

Hence we have, $$V^2 = \frac{b^2\,d\,h - d\,V^2}{a\,b^2\,l} \text{ and } V = \sqrt{\frac{b^2\,d\,h}{a\,b^2\,l + d}}.$$

Now Dubuat found b to be 6·6, and $a\,b^2$ was found to be 0·0211, particularly when the velocity is between six and twenty-four inches per second. Hence we have, $$V^2 = \frac{43{\cdot}6\,d\,h}{0{\cdot}0211\,l + d}, \text{ or } V = 45{\cdot}5 \sqrt{\left(\frac{d\,h}{l + 47\,d}\right)},$$

or more accurately, $$V = 50 \sqrt{\left(\frac{d\,h}{l + 50\,d}\right)}.$$

† The author of this paper has made a great many experiments on the maximum effect of water-wheels; but the recent experiments of the Franklin Institution, made on a more magnificent scale, and now in the course of trial, eclipse everything that has yet been effected on this subject. See also Poncelet, *Mémoire sur les Roues Hydrauliques*, and *Aubes Courbes par dessous, &c.* 1827.

and pumps of different descriptions, horizontal and inclined helices, bucket-wheels, throwing-wheels, and lastly, on instruments for measuring the velocity of streams of water. A very detailed account of the work was given in the *Journal of the Royal Institution*, by the late Dr. Young. But it is due to MM. Dubuat and Prony to state, that M. Eytelwein has exactly followed the steps of these gentlemen in his *Theory of the Motion of Water in open Channels.*

In the year 1809 a valuable series of experiments upon the motions of waters through pipes, was made by MM. Mallet and Vici at Rome, and afterwards by M. Prony *.

It had been proved, by experiments made with great care, that the diminution of velocity, and consequent expenditure in pipes, was not in the ratio of the capacity of the pipes, as Frontinus had supposed in his valuation of the product of the ancient module or calice; and as it was desirable to ascertain the actual product of the three fountains now used at Rome, a series of experiments was undertaken by these gentlemen; the principal result of which was, that a pipe, of which the gauge was five onces †, furnished $\frac{1}{7}$th more water than five pipes of one once, on account of the diminution of the velocity by friction in the ratio of the perimeters of the orifices as compared with their areas.

M. Mallet also made a great many researches relative to the distribution of water in the different cities and towns of England and France, with a view to their application at Paris; of all of which he has published an account.

The researches that had been made hitherto on the expenditure of water through orifices, had for their object the determination of the velocity and magnitude of the section, by which it is necessary to multiply the velocity to obtain the expense. But although these be the first elements for consideration, they are not sufficient; for the fluid vein presents other phænomena equally important, both in the theory and its application, namely, the form and direction of the vein after it has issued from the orifice. The former phænomena, as we before stated, had been long noticed by Michelotti and others, but nothing precise had been established on the forms and remarkable phænomena of the fluid vein itself. Venturi had given three examples.

M. Hachette, in two memoirs presented to the Académie Royale des Sciences in 1815 and 1816, also considered the

* *Notices Historiques,* par M. Mallet. Paris 1830.
† French measure, or 0·03059 French kilolitres.

form of veins; and in his *Traité des Machines*, he states that he had already given a description of veins issuing from circular, elliptical, triangular and square orifices, without having entered into any detail respecting them, so that that part of the subject was in a great measure involved in doubt. In 1829 a paper, entitled "Expériences sur la Forme et sur la Direction des Veines et des Courans d'Eau, lancés par diverses Ouvertures," was read to the Academy of Sciences at Turin by M. Bidone, giving an account of a series of experiments made in the years 1826 and 1827, in the Hydraulic Establishment of the Royal University. The results of these experiments are divided into five articles. The first gives a description of the apparatus and mode of proceeding, and the figures obtained from veins expended from rectilinear and curvilinear orifices, with salient angles pierced in vertical plates, and whose perimeters are formed by straight and curved lines, varying upwards of fifty different ways, with variable and invariable changes, from zero to twenty-two French feet: the area of water was equal to one square inch. The sections of the veins were taken at different distances from the aperture. The results are extremely curious, as illustrating the influence of pressure and divergence on part of a fluid mass not *in equilibrio*, and may be assimilated to the phænomena presented by the undulation of streams of light. The author contents himself with stating the results, which are further illustrated by diagrams.

In a second paper, read to the Accademia delle Scienze in April following of the same year (1829), M. Bidone enters into a theoretical consideration of his experiments, in which he represents the greatest contraction of the fluid vein to take place at a distance not exceeding the greatest diameter of the orifice, whatever be the shape; from which it results that the expression for the expense of the orifice is equal to the sum of the product of each superficial element multiplied by the velocity of the fluid vein; and as it was determined by experiment that the area of the vena contracta was from 0·60 to 0·62 of the area of the orifice, it follows that this coefficient of contraction, multiplied by the velocity due to the charge, represents the expenditure.

M. Bidone considers the case of a fluid vein reduced to a state of permanence, and expended from a very small orifice, as compared with the sections of the containing vessel, according to the theory of Euler; and finds that the magnitude of the section of the contracted vein does not depend upon the velocity of the component filaments, but solely on their direction, a result conformable to experiment.

He then determines, from the results of M. Venturoli*, the absolute magnitude of the contracted section of the vein (issuing from a circular orifice) to be exactly ⅝rds of the orifice, the correction due to the contraction depending upon the adhesion and friction of the fluid against the perimeter of the orifice, and the ratio of the area of the vein to the area of the orifice: the same for all orifices. Hitherto the magnitude of fluid veins, as determined by direct measurements, had given greater coefficients than the effective expenditure allowed.

Michelotti, with a pressure of twenty feet, with orifices of one and two inches in diameter, found the coefficient 0·649

Bossut	0·660
Borda	0·646
Venturi	0·640
Eytelwein	0·640
Hachette	0·690
Newton	0·707
Helsham	0·705
Brindley and Smeaton	0·631
Banks	0·750
Rennie†	0·621

In several experiments the ratio rarely exceeded 0·620; so that the discrepancy must have arisen from inaccuracies in the measurement of the fluid vein and orifice.

In the year 1827, it having been considered desirable to repeat the experiments of Bossut and Dubuat, application was made to the French Government by General Sabatier, Commander-in-chief of the Military School at Metz, for permission to undertake a series of experiments on a scale of magnitude sufficient to establish the principles laid down by those authors, and serve as valuable practical rules for future calculations. The apparatus consisted, 1st, of an immense basin, having an area of 25,000 square metres; 2nd, of a smaller reservoir, having a superficial area of 1500 square metres, and a depth of 3·70 metres, so contrived, by means of sluices, as to have a complete command of the level of the water during the experiment; 3rd, of a basin directly communicating with the second basin, 3·68 metres in length, and 3 metres in width, to receive the product of the orifices; 4th, a basin or gauge capable of containing 24,000 litres.

* *Elementi di Meccanica e d'Idraulica:* Milano 1818. *Recherche Geometriche fatte nella Scuola degli Ingegneri pontifici d'Acque e Strade, l'anno* 1821. Milano.

† "On the Friction and Resistance of Fluids," *Philosophical Transactions* of 1831.

The time was constantly noticed by an excellent stop-watch, made by Breguet; and the opening of the orifices, the charges of the fluid in the reservoir, as well as the level of the water in the gauge basin relative to each expense of fluid, were always measured to the tenth of a millimetre, so that, even under the most unfavourable circumstances, the approximation was at least to $\frac{1}{200}$dth part of the total result. The total disposable fall or height, counting from the ordinary surface of the Moselle river, was four metres, from which two metres were deducted for the gauge basin, leaving only a fall of two metres under the most favourable circumstances; and in the subsequent experiments of 1828 the height never exceeded 1·60 metre, sufficiently high for all practical purposes. An apparatus was provided for regulating the height of the orifice and the surface of the water in the reservoirs, and for tracing with the greatest accuracy the forms and sections of the fluid veins before and after issuing from the orifices, and the depressions experienced by the surface of the water previously to its issuing from an opening of twenty centimetres square, the upper side of which was on a level with the surface of the water in the reservoir. These depressions are recorded in the Tables,

1st, On the expenditure of water through rectangular vertical orifices, twenty centimetres square, and varying in height from one to twenty centimetres, under charges of from ·0174 of a metre to 1·6901 metre:

2ndly, On the expenditures of water from the similar-sized orifices, open at the top, but under charges of from two to twenty-two centimetres.

The whole is comprised in eleven Tables of 241 experiments, to which is added a twelfth Table, showing the value of the coefficients of contraction for complete orifices, from twenty centimetres square to one centimetre, calculated according to the following formula:

D for the height of the orifices, where*

$$D = l\,o\sqrt{2gh} = l\,(h-h')\sqrt{2g\,\frac{(h+h')}{2}}$$ being the theoretical expense relative to the velocity;

$$D' = \tfrac{2}{3}\,l\sqrt{2g}\,(h^{\frac{3}{2}} - h'^{\frac{3}{2}}) = \tfrac{2}{3}\,l\,(h\sqrt{2gh} - h'\sqrt{2gh'})$$

or the theoretical expense, having regard to the influence of the orifice.

* That is, where $l = 0·20$ metre, being the horizontal breadth of all the orifices;
$h =$ the charge of the fluid on the lower part of the orifice;
$h' =$ the charge in the upper or variable side of the orifice;
$o = h - h'$ the thickness of the vein of water.

The conclusions to be derived from these Tables are,

1st, That for complete orifices of twenty centimetres square and high charges, the coefficient is 0·600; with the charge equal to four or five times the opening of the orifice, the coefficient augments to 0·605; but beyond that charge the coefficient diminishes to 0·593.

2ndly, That the same law maintains for orifices of ten and five centimetres in height, the coefficients being for ten centimetres 0·611, 0·618, 0·611 respectively, and for five centimetres in height 0·618, 0·631, 0·623.

Lastly, That with orifices of three, two and one centimetres in height, the law changes very rapidly, and the coefficients increase as the opening of the orifice becomes less, being for one centimetre 0·698, the smallest height of the orifice, to 0·640 for three centimetres.

These remarkable discrepancies from the results of Bidone and others are attributed by MM. Lesbros and Poncelet to differences in the construction of the apparatus or in the mode of measurement adopted by the latter gentlemen; but in general the coincidences are sufficiently satisfactory, and they are the more accurately confirmed by the subsequent investigations of MM. D'Aubuisson and Castel at Toulouse *. As respects water issuing from the openings or notches made in the sides of dams, or what we should term *incomplete* orifices, it appears that the coefficient obtained by the ordinary formula of Dubuat, or $l\, h \sqrt{2\, g\, h}$, augments from the total charge of twenty-two centimetres when it is from 0·389 to two centimetres when it becomes 0·415; hence we may safely adopt M. Bidone's coefficient of 0·405, or, according to MM. Poncelet and Lesbros' theory 0·400, for calculating expenditures through notches in dams. From these and other experiments the authors are led to conclude, that the law of continuity maintains for indefinite heights both with complete and *incomplete* orifices, and that the same coefficient can be obtained by adopting in both cases the same formula. The authors observe that the area of the section of the greatest contraction of the vein, considered as a true square, is exactly two thirds of the area of the orifice; a fact which goes to prove that there is no certain comparison between the mean theoretical or calculated velocities, by means of the formula now used, and the mean effective velocities derived from the expenditure.

The authors conclude their memoir by recommending their experiments for adoption in all cases of plate orifices situated

* *Annales de Chimie et de Physique* for 1830, tom. xliv. p. 225.

at a distance from the sides and bottom of the reservoir, promising to investigate with similar accuracy in a future memoir the cases which may occur to the contrary.

A note is appended to the memoir by M. Lesbros, containing formulæ for calculating the effective expenditure of complete orifices; and also a Table of constants, which gives the effective expenditure of each orifice as compared with experiment. We have been thus particular in detailing the results of MM. Lesbros and Poncelet's work, because they have comprehended all the cases upon which there remained any doubts, and with very few exceptions are in accordance with the experiments of Brunacci, Navier, Christian, Gueymard, D'Aubuisson, and by the author of this paper*. So that in point of accuracy and laborious investigation, the authors of these valuable accessions to our knowledge, not only merit our gratitude, but have very amply replied to the liberality of the French Government.

Having thus endeavoured to elucidate the labours of the foreign philosophers who have contributed so greatly to the progress of hydraulics, it only remains for us to notice the scanty contributions of our countrymen to the science. While France and Germany were rapidly advancing upon the traces of Italy, England remained an inactive spectator of their progress, contented with the splendour of her own Newton, to receive from foreigners whatever was original or valuable in the science. The *Philosophical Transactions*, rich as they are in other respects, scarcely contain a single paper on this subject founded on any experimental investigations. Some erroneous and inconclusive inferences from Newton, by Dr. Jurin; a paper on the Measure of Force, by Mr. Eames; a paper on Wiers, by Mr. Roberts; another on the Motion and Resistance of Fluids, by Dr. Vince; and a summary of Bossut and Dubuat's Experiments on the Motions of Fluids through Tubes, by Dr. Thomas Young, comprise nearly the whole of the papers on hydraulics in the *Philosophical Transactions*. The various treatises on the subject published by Maclaurin, Emerson, Dr. Matthew Young, Desaguliers, Clare and Switzer, with the exception of the theoretical investigations, are compiled principally from the works of foreigners; and it was not until the subject was taken up by Brindley, Smeaton, Robison, Banks and Dr. Thomas Young, that we were at all aware of our deficiency. Practical men were either necessitated to follow the uncertain rules derived from their predecessors, or their own experience and sagacity, for the little knowledge they possessed.

* *Philosophical Transactions* for 1831.

On the subject of hydrometry we were equally ignorant; and although the Italian collection had been published several years previously, and was well known on the Continent, it was not until Mr. Mann published an abstract of that collection that we were at all aware of the state of the science abroad.

Under these circumstances the author of this paper was induced, in the year 1830, to undertake a series of experiments to ascertain, 1st, The friction of water against the surface of a cylinder, and discs revolving in it, at different depths and velocities: from which it appeared, that with slow velocities the friction approximated the ratio of the surfaces, but that an increase of surface did not materially affect it with increased velocities; and that with equal surfaces the resistances approximated to the squares of the velocities.

2ndly, To ascertain the direct resistances against globes and discs revolving in air and water alternately: from which it resulted, that the resistances in both cases were as the squares of the velocities; and that the mean resistances of circular discs, square plates, and globes of equal area, in atmospherical air, were as under:

Circular discs . .	25·180	. . . . . .	1·18
Square plates . .	22·010	in air, . .	1·36 in water.
Round globes . .	10·627	. . . . . .	0·75

3rdly, That with circular orifices made in brass plates of $\frac{1}{60}$th of an inch in thickness, and having apertures of $\frac{1}{4}$, $\frac{1}{2}$, $\frac{3}{4}$, $\frac{1}{1}$ of an inch respectively, under pressures varying from one to four feet, the average coefficients of contraction were,

for altitudes of 1 foot . . . . 0·619
4 feet . . . . 0·621

For additional tubes of glass the coefficient was,

for 1 foot . . . . 0·817
4 feet . . . . 0·806

4thly, That the expenditures through orifices, additional tubes, and pipes of different lengths, of equal areas and under the same altitude as compared with the expenditure through a pipe of 30 feet in length, are as

1 : 3 for orifices,
1 : 4 for additional tubes,
1 : 3·7 for a pipe 1 foot in length,
1 : 2·6 ——— 8 feet ———,
1 : 2·0 ——— 4 ———,
1 : 1·4 ——— 2 ———.

5thly, That with bent rectangular pipes $\frac{1}{2}$ an inch in diameter, and 15 feet in length, the expenditures were diminished with fourteen bends two thirds, as compared with a straight pipe, and with twenty-four right angles, one third; but did not seem to observe any decided law.

In several experiments tried on a great scale, the results gave from one fifth to one sixth of the altitude for the friction. In the case of the Coniston main, which conducts the water from the reservoir at Coniston to the castle of Edinburgh, the diameter of which is $4\frac{1}{2}$ inches, the length 14,930 feet, and the altitude 51 feet, it was proved by Mr. Jardine that the formulæ of Dubuat and Eytelwein approximated to the real results very nearly; and in some experiments made on a great scale by the author of this paper, these formulæ were found equally applicable. In several experiments made in the year 1828, on the water-works at Grenoble, by M. Gueymard, it was found that pipes of six and eight French inches in diameter furnished only two thirds of the water indicated by the formulæ of M. Prony; but when of nine inches diameter, the formula approximated very nearly. In M. Gueymard's experiment the altitude of the reservoir above the point of delivery was 8·453 metres, or 27·73 English feet. The height to which the water was required to be elevated was 5·514 metres, or 18 feet; the volume of water required was 954 litres, or 33·6 cubic feet; the length of the pipe was 3200 metres, or 10498 feet. There were eight gentle curves in the system, but enlarged beyond the average diameter of the parts of the pipe; from which it resulted that the height to which the water was delivered was only two thirds of the height of the reservoir*.

In the preceding short but imperfect history of the science of hydraulics we have confined our attention to the experimental researches that have been made on spouting fluids only. In a future communication I hope to examine the state of our knowledge of the natural phænomena of rivers, and the causes by which they are influenced; at present it is extremely limited, and although we have many works upon the subject, very little seems to be known either of their properties or of the laws by which they are governed.

* According to M. Prony's theory, the height raised would only have been 5·514 metres instead of 5·671 metres. The difficulty, however, of making experiments on a great scale will always prove an obstacle to the right solution of the question, in as much as it exacts that the pipe be of the same diameter throughout, that is, perfectly straight, and free from bends, and the charge of water invariable. For this purpose M. Prony has calculated Tables showing the relation subsisting between the expenditure, diameter, length, the total inclination of the pipes, and the difference of pressure at its extremities.

Appendix.

Since the foregoing Report was read to the British Association a paper, entitled "Mémoire sur la Constitution des Veines Liquides lancées par des Orifices Circulaires en mince paroi," has been communicated to the Academy of Sciences at Paris, by M. Félix Savart, 26 Août 1833. The author, after detailing very minutely the different phænomena presented by liquid veins issuing from circular orifices perforated in thin plates, attached to the bottom and sides of vessels, illustrates his positions by a series of curious experiments on the vibrations and sounds of the drops which issue from the annular rings or pipes formed by the troubled part of the liquid. The results of these experiments are best given in his own words.

"1°. Toute veine liquide lancée verticalement de haut en bas par un orifice circulaire pratiqué dans une paroi plane et horizontale est toujours composée de deux parties bien distinctes par l'aspect et la constitution. La partie qui touche à l'orifice est un solide de révolution dont toutes les sections horizontales vont en décroissant graduellement de diamètre. Cette première partie de la veine est calme et transparente, et ressemble à un tige de cristal. La seconde partie, au contraire, est toujours agitée, et paraît dénuée de transparence, quoiqu'elle soit cependant d'une forme assez régulière pour qu'on puisse facilement voir qu'elle est divisée en un certain nombre de renflemens allongés dont le diamètre maximum est toujours plus grand que celui de l'orifice.

"2°. Cette seconde partie de la veine est composée de gouttes bien distinctes les unes des autres, qui subissent pendant leur chute, des changemens périodiques de forme, auxquels sont dues les apparences de ventres ou renflemens régulièrement espacés que l'inspection directe fait reconnaître dans cette partie de la veine, dont la continuité apparente dépend de ce que les gouttes se succèdent à des intervalles de temps qui sont moindres que la durée de la sensation produite sur la rétine par chaque goutte en particulier.

"3°. Les gouttes qui forment la partie trouble de la veine sont produites par des renflemens annulaires qui prennent naissance très près de l'orifice, et qui se propagent à des intervalles de temps égaux, le long de la partie limpide de la veine, en augmentant de volume à mesure qu'ils descendent, et qui enfin se séparent de l'extrémité inférieure de la partie limpide et continue à des intervalles de temps égaux à ceux de leur production et de leur propagation.

"4°. Ces renflemens annulaires sont engendrés par une succession périodique de pulsations qui ont lieu à l'orifice même; de sorte que la vitesse de l'écoulement, au lieu d'être uniforme, est périodiquement variable.

"5°. Le nombre de ces pulsations, même pour des charges foibles, est toujours assez grand, dans un temps donné, pour qu'elles soient de l'ordre de celles qui, par la fréquence de leur retour, peuvent donner lieu à des sons perceptibles et comparables. Ce nombre ne dépend que de la vitesse de l'écoulement, à laquelle il est directement proportionnel, et du diamètre des orifices, auquel il est inversement proportionnel. Il ne paraît altéré ni par la nature du liquide, ni par la température.

"6°. L'amplitude de ces pulsations peut être considérablement augmentée par des vibrations de même période communiquées à la masse entière du liquide et aux parois du réservoir qui le contient. Sous cette influence étrangère, les dimensions et l'état de la veine peuvent subir des changemens remarquables: la longueur de la partie limpide et continue peut se réduire presqu'à rien, tandis que les ventres de la partie trouble acquièrent une régularité de forme et une transparence qu'ils ne possèdent pas ordinairement. Lorsque le nombre des vibrations communiquées est différent de celui des pulsations qui ont lieu à l'orifice, leur influence peut même aller jusqu'à changer le nombre de ces pulsations, mais seulement entre de certaines limites.

"7°. La dépense ne paraît pas altérée par l'amplitude des pulsations, ni même par leur nombre.

"8°. La resistance de l'air n'influe pas sensiblement sur la forme et les dimensions des veines, non plus que sur le nombre des pulsations.

"9°. La constitution des veines lancées horizontalement ou même obliquement de bas en haut ne diffère pas essentiellement de celle des veines lancées verticalement de haut en bas; seulement le nombre des pulsations à l'orifice paraît devenir d'autant moindre que le jet approche plus d'être lancé verticalement de bas en haut.

"10°. Quelle que soit la direction de la veine, son diamètre décroît toujours très rapidement jusqu'à une petite distance de l'orifice; mais quand la veine tombe verticalement, le décroissement continue jusqu'à ce que la partie limpide se perde dans la partie trouble: il en est encore de même quand la veine est lancée horizontalement, quoiqu'alors le décroissement suive une loi moins rapide. Lorsque le jet est lancé obliquement de bas en haut, et qu'il forme avec l'horizon un angle de 25° à 45°, toutes les sections normales à la courbe qu'il décrit deviennent sensiblement égales entre elles, à partir de la partie contractée que touche à l'orifice. Enfin, pour des angles plus grands que 45°, le diamètre de la veine va en augmentant depuis la partie contractée jusqu'à la naissance de la portion trouble ; de sorte que c'est seulement alors qu'il existe une section qu'on peut à juste titre appeler section contractée."

Report on the Recent Progress and Present State of certain Branches of Analysis. By George Peacock, *M.A., F.R.S., F.G.S., F.Z.S., F.R.A.S., F.C.P.S., Fellow and Tutor of Trinity College, Cambridge.*

The present Report was intended in the first instance to have comprehended some notice of the recent progress and present state of analytical science in general, including algebra, the application of algebra to geometry, the differential and integral calculus, and the theory of series: a very little progress, however, in the inquiries which were required for the execution of this undertaking convinced me of the necessity of confining them within much narrower limits, unless I should have ventured to occupy a much larger space in the annual publication of the Proceedings and Reports of the British Association than could be properly or conveniently allotted to one department of science, when so many others were required to be noticed. It is for these reasons that I shall restrict my observations, in the following Report, to Algebra, Trigonometry, and the Arithmetic of Sines; at the same time I venture to indulge a hope of being allowed, upon some future occasion, to bring before the Members of the Association some notice of those higher branches of analysis which at present I feel myself compelled, though reluctantly, to omit.

Algebra.—The science of algebra may be considered under two points of view, the one having reference to its principles, and the other to its applications: the first regards its completeness as an independent science; the second its usefulness and power as an instrument of investigation and discovery, whether as respects the merely symbolical results which are deducible from the systematic developement of its principles, or the application of those results, by interpretation, to the physical sciences.

Algebra, considered with reference to its principles, has received very little attention, and consequently very little improvement, during the last century; whilst its applications, using that term in its largest sense, have been in a state of continued advancement. Many causes have contributed to this comparative neglect of the accurate and logical examination of the first principles of algebra: in the first place, the proper assumption and establishment of those principles involve metaphysical difficulties of a very serious kind, which present themselves to a learner at a period of his studies when his mind has not been subjected to such a system of mathematical discipline as may enable it to cope with them: in the second place, we are commonly taught to approach those difficulties under the cover of a much more simple and much less general science, by steps which are studiously smoothed down, in order to render the transition from one science to the other as gentle and as little startling as possible; and lastly, from the peculiar relation which the first principles of algebra, in common with those of other sciences of strict demonstration, bear to the great mass of facts and reasonings of which those sciences are composed.

It is this last circumstance which constitutes a marked distinction between those sciences which, like algebra and geometry, are founded upon assumed principles and definitions, and the physical sciences: in one case we consider those principles and definitions as *ultimate* facts, from which our investigations proceed in one direction only, giving rise to a series of conclusions which have reference to those facts alone, and whose correctness or truth involves no other condition than the existence of a necessary connexion between them, in whatever manner the evidence of that existence may be made manifest; whilst in the physical sciences there are no such *ultimate* facts which can be considered as the natural or the assumable limits of our investigations. It is true, indeed, that in the application of algebra or geometry to such sciences, we assume certain facts or principles as possessing a necessary existence or truth, investing them, as it were, with a strictly mathematical character, and making them the foundation of a system of propositions, whose connexion involves the same species of evidence with that of the succession of propositions in the abstract sciences; but in assigning to such propositions their proper interpretation in the physical world, our conclusions are only true to an extent which is commensurate with the truth and universality of application of our fundamental assumptions, and of the various conditions by which the investigation of those propositions has been supposed to be limited; in other words, such conclusions can be considered as approximations only to physical truth; for such assumed first principles, however vast may be the superstructure which is raised upon them, form only one or more links in the great chain of propositions, the termination and foundation of which must be for ever veiled in the mystery of the first cause.

It is not my intention to enter upon the examination of the general relations which exist between the speculative and physical sciences, but merely to point out the distinction between the ultimate objects of our reasonings in the one class and in the other: in the first, we merely regard the results of the science itself, and the logical accuracy of the reasoning by which they are deduced from assumed first principles; and all our conclusions possess a necessary existence, without seeking either for their strict or for their approximate interpretation in the nature of things: in the second, we found our reasonings equally upon assumed first principles, and we equally seek for logical accuracy in the deduction of our conclusions from them; but both in the principles themselves and in the conclusions from them, we look to the external world as furnishing by interpretation corresponding principles and corresponding conclusions; and the physical sciences become more or less adapted to the application of mathematics, in proportion to the extent to which our assumed first principles can be made to approach to the most simple and general facts or principles which are discoverable in those sciences by observation or experiment, when divested of all incidental and foreign causes of variation; and still more so, when the causes of such variation can be distinctly pointed out, and when their extent and influence are reducible to approximate at least, if not to accurate estimation.

The first principles, therefore, which form the foundation of our mathematical reasonings in the physical sciences being neither arbitrary assumptions nor necessary truths, but really forming part of the series of propositions of which those sciences are composed, can never cease to be more or less the subject of examination and inquiry at any point of our researches: they form the basis of those interpretations which are perpetually required to connect our mathematical with the corresponding physical conclusions; and even supposing the immediate appeal to them to be superseded, as will frequently be the case, by other propositions which are deducible from them, they still continue to claim our attention as the propositions which terminate those physical and logical inquiries at which our mathematical reasonings begin. But in the abstract sciences of geometry and algebra, those principles which are the foundation of those sciences are also the proper limits of our inquiries; for if they are in any way connected with the physical sciences, the connexion is arbitrary, and in no respect affects the truth of our conclusions, which respects the evidence of their connexion with the first principles only, and does not require, though it may allow, the aid of physical interpretation.

It is true that there exists a connexion between physical and speculative geometry, as well as between physical and speculative mechanics; and if in speculative geometry we regarded the actual construction and mensuration of the figures and solids in physical geometry alone, the transition from one science to the other being made by interpretation, then speculative geometry and speculative mechanics must be regarded as sciences which were similar in their character, though different in their objects: but we cultivate speculative geometry without any such exclusive reference to physical geometry, as an instrument of investigation more or less applicable, by means of interpretation, to all sciences which are reducible to measure, and whose abstract conclusions, in whatever manner *suggested* or derived, possess a great practical value altogether apart from their applications to practical geometry; whilst the conclusions in speculative mechanics are valuable from their applications to physical mechanics only, and are not otherwise separable from the conclusions of those abstract sciences which are employed as instruments in their investigation.

This separation of speculative and physical geometry was perfectly understood by the ancients, though their views of its application to the physical sciences were extremely limited; and it is to the complete abstraction of the principles of speculative geometry that we must in a great measure attribute the vast discoveries which were made by its aid in the hands of Newton and his predecessors, when a more enlarged and philosophical knowledge of the laws of nature supplied those physical axioms or truths which were required as the medium of its applications; and though it was destined to be superseded, at least in a great degree, by another abstract science of much greater extent and applicability, yet it was enabled to maintain its ground for a considerable time against its more powerful rival, in consequence of the superior precision of its principles and the superior evidence of its conclusions, when considered with reference to the form under which the principles and conclusions of algebra were known or exhibited at that period.

Algebra was denominated in the time of Newton *specious* or *universal arithmetic*, and the view of its principles which gave rise to this *synonym* (if such a term may be used) has more or less prevailed in almost every treatise upon this subject which has appeared since his time. In a similar sense, algebra has been said to be a science which arises from that generalization of the processes of arithmetic which results from the use of symbolical language: but though in the exposition of the principles of algebra, arithmetic has always been taken for its foundation, and the names of the fundamental operations in one science have been transferred to the other without any immediate change of their meaning, yet it has generally been found necessary subsequently to enlarge this very narrow basis of so very general a science, though the reason of the necessity of doing so, and the precise point at which, or the extent to which, it was done, has usually been passed over without notice. The science which was thus formed was perfectly abstract, in whatever manner we arrived at its fundamental conclusions; and those conclusions were the same whatever view was taken of their origin, or in whatever manner they were deduced; but a serious error was committed in considering it as a science which admitted of strict and rigorous demonstration, when it certainly possessed no adequate principles of its own, whether assumed or demonstrated, which could properly justify the character which was thus given to it.

There are, in fact, two distinct sciences, *arithmetical* and *symbolical algebra*, which are closely connected with each other, though the existence of one does not necessarily determine the existence of the other. The first of these sciences would be, properly speaking, *universal arithmetic*: its general symbols would represent numbers only; its fundamental operations, and the signs used to denote them, would have the same meaning as in common arithmetic; it would reject the *independent* use of the signs + and −, though it would recognise the common rules for their incorporation, when they were preceded by other quantities or symbols: the operation of subtraction would be *impossible* when the subtrahend was greater than the quantity from which it was required to be taken, and therefore the proper *impossible* quantities of such a science would be the *negative* quantities of *symbolical* algebra; it would reject also the consideration of the multiple values of simple roots, as well as of the negative and impossible roots of equations of the second and higher degree: it is this species of algebra which alone can be legitimately founded upon arithmetic as its basis.

Mr. Frend *, Baron Maseres, and others, about the latter end of the last century, attempted to introduce *arithmetical*

* *The Principles of Algebra*, by William Frend, 1796; and *The true Theory of Equations, established on Mathematical Demonstration*, 1799. The following extracts from his prefaces to these works will explain the nature of his views:

"The ideas of number are the clearest and most distinct of the human mind: the acts of the mind upon them are equally simple and clear. There cannot be confusion in them, unless numbers too great for the comprehension of the to the exclusion of *symbolical* algebra, as the only form of it which was capable of strict demonstration, and which alone, therefore, was entitled to be considered as a science of strict and logical reasoning. The arguments which they made use of were unanswerable, when advanced against the form under which the principles of algebra were exhibited in the elementary and all other works of that period, and which they have continued to retain ever since, with very trifling and unimportant alterations; and the system of algebra which was formed by the first of these authors was perfectly logical and complete, the connexion of all its parts being capable of strict demonstration; but there were a great multitude of algebraical results and propositions, of unquestionable value and of unquestionable consistency with each other, which were irreconcilable with such a system, or, at all events, not deducible from it; and amongst them, the theory of the composition of equations, which Harriot had left in so complete a form, and which made it necessary to consider negative and even impossible quan-

learner are employed, or some arts are used which are not justifiable. The first error in teaching the first principles of algebra is obvious on perusing a few pages only of the first part of Maclaurin's Algebra. Numbers are there divided into two sorts, positive and negative: and an attempt is made to explain the nature of negative numbers, by allusions to book debts and other arts. Now when a person cannot explain the principles of a science, without reference to a metaphor, the probability is, that he has never thought accurately upon the subject. A number may be greater or less than another number: it may be added to, taken from, multiplied into, or divided by, another number; but in other respects it is very intractable; though the whole world should be destroyed, one will be one, and three will be three, and no art whatever can change their nature. You may put a mark before one, which it will obey; it submits to be taken away from another number greater than itself, but to attempt to take it away from a number less than itself is ridiculous. Yet this is attempted by algebraists, who talk of a number less than nothing, of multiplying a negative number into a negative number, and thus producing a positive number, of a number being imaginary. Hence they talk of two roots to every equation of the second order, and the learner is to try which will succeed in a given equation: they talk of solving an equation which requires two impossible roots to make it soluble: they can find out some impossible numbers, which being multiplied together produce unity. This is all jargon, at which common sense recoils; but from its having been once adopted, like many other figments, it finds the most strenuous supporters among those who love to take things upon trust and hate the colour of a serious thought."

"From the age of Vieta, the father, to this of Maseres, the restorer of algebra, many men of the greatest abilities have employed themselves in the pursuit of an idle hypothesis, and have laid down rules not founded in truth, nor of any sort of use in a science admitting in every step of the plainest principles of reasoning. If the name of Sir Isaac Newton appears in this list, the number of advocates for errour must be considerable. It is, however, to be recollected, that for a much longer period, men scarcely inferiour to Newton in genius, and his equals, probably, in industry, maintained a variety of positions in philosotities as having a real existence in algebra, however vain might be the attempt to interpret their meaning.

Both Mr. Frend and Baron Maseres were sensible of the consequences of admitting the truth of this theory of the composition of equations as far as their system was concerned, and it must be allowed that they have struggled against it with considerable ingenuity: they admitted the possibility of multiple real, that is, positive roots, and which are all equally *congruous* to the problem whose solution was required through the medium of the equation, indicating an *indetermination* in the problem proposed: but it would be easy to propose problems leading to equations whose roots were real and positive, and yet not *congruous* to the problem proposed, whose existence must be admitted upon their own principles; and if so, why not admit the existence of other roots, whether negative or impossible, to which the algebraical solution of the problem might lead, though they might admit of no very direct interpretation, in conformity with the expressed conditions of the problem*?

phy, which were overthrown by a more accurate investigation of nature; and if the name Ptolemy can no longer support his epicycles, nor that of Des Cartes his vortices, Newton's dereliction of the principles of reasoning cannot establish the fallacious notion, that every equation has as many roots as it has dimensions."

"This notion of Newton and others is founded on precipitation. Instead of a patient examination of the subject, an hypothesis which accounts for many appearances is formed; where it fails, unintelligible terms are used; in those terms indolence acquiesces: much time is wasted on a jargon which has the appearance of science, and real knowledge is retarded. Thus volumes upon volumes have been written on the stupid dreams of Athanasius, and on the impossible roots of an equation of n dimensions."

This work of Mr. Frend, though containing many assertions which show great distrust of the results of algebraical science which were in existence at the time it was written, presents a very clear and logical view of the principles of arithmetical algebra.

The voluminous labours of Baron Maseres are contained in his *Scriptores Logarithmici*, and in a thick volume of Tracts on the Resolution of Cubic and Biquadratic Equations. He seems generally to have forgotten that any change had taken place in the science of algebra between the age of Ferrari, Cardan, Des Cartes, and Harriot, and the end of the 18th century; and by considering all algebraical formulæ as essentially arithmetical, he is speedily overwhelmed by the same multiplicity of cases (which are all included in the same really algebraical formula) which embarrassed and confounded the first authors of the science.

* Thus, in the solution of the following problem: "Sold a horse for 24*l.*, and by so doing lost as much per cent. as the horse cost me: required the prime cost of the horse?" we arrive at the equation

$$100\,x - x^2 = 2400;$$

if we subtract both sides of this equation from 2500, we get

$$2500 - 100\,x + x^2 = 100,$$

$$\text{or } x^2 - 100\,x + 2500 = 100,$$

inasmuch as the quantities upon each side of the sign = are in both cases

If the authors of this attempt at algebraical reform had been better acquainted with the more modern results of the science, they would have felt the total inadequacy of the very limited science of arithmetical algebra to replace it; and they would probably have directed their attention to discover whether any principles were necessary to be assumed, which were not necessarily deducible as propositions from arithmetic or arithmetical algebra, though they might be suggested by them. As it was, however, these speculations did not receive the consideration which they really merited; and it is very possible that the attempt which was made by one of their authors to connect the errors in reasoning, which he attacked, as forming part only of a much more extensive class to which the human mind is liable from the influence of prejudice or fashion, had a tendency to divert men of an enlarged acquaintance with the results of algebra from such a cautious and sustained examination of them as was required for their refutation, or rather for such a correction of them as was really necessary to establish the science of algebra upon its proper basis.

I know that it is the opinion of many persons, even amongst the masters* of algebraical science, that arithmetic does supply

identical with each other: if we extract the square root on both sides, rejecting the negative value of the square root, we get in the first case

$$50 - x = 10,$$

and in the second,

$$x - 50 = 10.$$

The first of these simple equations gives us $x = 40$, and the second $x = 60$, both of which satisfy the conditions of the problem proposed: the two roots which are thus obtained, strictly by means of arithmetical algebra, show that the problem proposed is to a certain extent indeterminate. Mr. Frend and Baron Maseres contended that multiple real roots, which are always the indication of a similar indetermination in the problems which lead to such equations, might be obtained by arithmetical algebra alone, and that all other roots were useless fictions, which could lead to no practical conclusions. But it is very easy to show, that incongruous and real, as well as negative and impossible roots, may equally indicate the impossibility of the problem proposed: thus, if it was proposed "to find a number the double of whose square exceeds three times the number itself by 5," we shall find $\frac{5}{2}$ and -1 for the roots of the resulting equation, both of which equally indicate the impossibility of the problem proposed, if by *number* be meant a *whole positive number*.

* Cauchy, who has enriched analysis with many important discoveries, and who is justly celebrated for his almost unequalled command over its language, has made it the principal object of his admirable work, entitled *Cours d'Analyse de l'Ecole Royale Polytechnique*, to meet the difficulties which present themselves in the transition from arithmetical to symbolical algebra: and though he admits to the fullest extent the essential distinction between them, in the ultimate form which the latter science assumes, yet he considers the principles of one as deducible from those of the other, and presents the rules for the concurrence and incorporation of signs; for the inverse relation of the operations called addition and subtraction, multiplication and division; for

a sufficient basis for symbolical algebra considered under its most general form; that symbols, considered as representing numbers, may represent every kind of concrete magnitude; the indifference of the order of succession of different algebraical operations, as so many *theorems* founded upon the ordinary principles and reasonings of arithmetic. In order to show, however, the extraordinary vagueness of the reasoning which is employed to establish these theorems, we will notice some of them in detail: *On représente,* says he, *les grandeurs qui doivent servir d'accroissements, par des nombres précédés du signe* $+$, *et les grandeurs qui doivent servir de diminutions par des nombres précédés du signe* $-$. *Cela posé, les signes* $+$ *et* $-$ *placés devant les nombres peuvent être comparés, suivant la remarque qui en a été faite*[1], *à des adjectifs placés auprès de leurs substantifs.* It is unquestionable, however, that in the most common cases of the interpretation of specific magnitudes affected with the signs $+$ and $-$, there is no direct reference either to increase or diminution, to addition or to subtraction. He subsequently gives those signs a conventional interpretation, as denoting quantities which are *opposed* to each other; and assuming the existence of quantities affected by independent signs, and denoting $+\mathrm{A}$ by a, and $-\mathrm{A}$ by b, he says that

$$+a = +\mathrm{A} \qquad +b = -\mathrm{A}$$
$$-a = -\mathrm{A} \qquad -b = +\mathrm{A};$$

and therefore,

$$+(+\mathrm{A}) = +\mathrm{A} \qquad +(-\mathrm{A}) = -\mathrm{A}$$
$$-(+\mathrm{A}) = -\mathrm{A} \qquad -(-\mathrm{A}) = +\mathrm{A};$$

which he considers as a sufficient *proof* of the rule of the concurrence of signs in whatever operations they may occur; though it requires a very slight examination of this process of reasoning to show that it involves several arbitrary assumptions and interpretations which may or may not be consistent with each other. In the proofs which he has given of the other fundamental theorems which we have mentioned above, we shall find many other instances of similar confusion both in language and in reasoning: thus, "subtraction is the inverse of addition in arithmetic; then therefore, also, subtraction is the inverse of addition in algebra, even when applied to quantities affected with the signs $+$ and $-$, and whatever those quantities may be." But is this a conclusion or an assumption? or in what manner can we explain in words the process which the mind follows in effecting such a deduction? "If a and b be whole numbers, it may be proved that $a\,b$ is identical with $b\,a$: *therefore,* $a\,b$ is identical with $b\,a$, whatever a and b may denote, and whatever may be the interpretation of the operation which connects them." But any attempt to establish this conclusion, without a previous definition of the meaning of the operation of multiplication when applied to such quantities, will show it to be altogether impracticable. The system which he has followed, not merely in the establishment of the fundamental operations, but likewise in the interpretation of what he terms *symbolical expressions* and *symbolical equations,* requires the introduction of new *conventions,* which are not the less arbitrary because they are rendered necessary for the purpose of making the results of the science consistent with each other: some of those conventions I believe to be necessary, and others not; but in almost every instance I should consider them introduced at the wrong place, and more or less inconsistently with the professed grounds upon which the science is founded.

[1] By Buée in the *Philosophical Transactions,* 1806.

that the operations of addition, subtraction, multiplication and division are used in one science and in the other in no sense which the mind may not comprehend by a practicable, though it may not be by a very simple, process of generalization; that we may be enabled by similar means to conceive both the use and the meaning of the signs + and −, when used independently; and that though we may be startled and somewhat embarrassed by the occurrence of impossible quantities, yet that investigations in which they present themselves may generally be conducted by other means, and those difficulties may be evaded which it may not be very easy or very prudent to encounter directly and openly.

In reply, however, to such opinions, it ought to be remarked that arithmetic and algebra, under no view of their relation to each other, can be considered as one science, whatever may be the nature of their connexion with each other; that there is nothing in the nature of the symbols of algebra which can essentially confine or limit their signification or value; that it is an abuse of the term *generalization** to apply it to designate the process of mind by which we pass from the meaning of $a-b$, when a is greater than b, to its meaning when a is less than b, or from that of the product $a\,b$, when a and b are abstract numbers, to its meaning when a and b are concrete numbers of the same or of a different kind; and similarly in every case where a result is either to be obtained or explained, where no previous definition or explanation can be given of the operation upon which it depends: and even if we should grant the legitimacy of such generalizations, we do necessarily arrive at a new science much more general than arithmetic, whose principles, however derived, may be considered as the immediate, though not the ultimate foundation of that system of combinations of symbols which constitutes the science of algebra. It is more natural and philosophical, therefore, to assume such principles as independent and *ultimate*, as far as the science itself is concerned, in whatever manner they may have been suggested, so that it may thus become essentially a science of symbols and their combinations, constructed upon its own rules, which may

* The operations in arithmetical algebra can be previously defined, whilst those in symbolical algebra, though bearing the same name, cannot: their meaning, however, when the nature of the symbols is known, can be generally, but by no means necessarily, interpreted. The process, therefore, by which we pass from one science to the other is not an ascent from particulars to generals, which is properly called *generalization*, but one which is essentially arbitrary, though restricted with a specific view to its operations and their results admitting of such interpretations as may make its applications most generally useful.

be applied to arithmetic and to all other sciences by interpretation: by this means, interpretation will *follow*, and not *precede*, the operations of algebra and their results; an order of succession which a very slight examination of their necessary changes of meaning, corresponding to the changes in the specific values and applications of the symbols involved, will very speedily make manifest.

But though the science of arithmetic, or of arithmetical algebra, does not furnish an adequate foundation for the science of symbolical algebra, it necessarily *suggests* its principles, or rather its laws of combination; for in as much as symbolical algebra, though arbitrary in the authority of its principles, is not arbitrary in their application, being required to include arithmetical algebra as well as other sciences, it is evident that their rules must be identical with each other, as far as those sciences proceed together in common: the real distinction between them will arise from the *supposition or assumption that the symbols in symbolical algebra are perfectly general and unlimited both in value and representation, and that the operations to which they are subject are equally general likewise.* Let us now consider some of the consequences of such an assumption.

A system of symbolical algebra will require the assumption of the independent use of the signs $+$ and $-$.

For the general rule in arithmetical algebra* informs us, that the result of the subtraction of $b + c$ from a is denoted by $a - b - c$, or that $a - (b + c) = a - b - c$, its application being limited by the necessity of supposing that $b + c$ is less than a. The general hypothesis made in symbolical algebra, namely, *that symbols are unlimited in value, and that operations are equally applicable in all cases,* would necessarily lead us to the conclusion that $a - (b + c) = a - b - c$ for *all values of the symbols,* and therefore, also, when $b = a$, in which case we have

$$a - (a + c) = a - a - c = - c.$$

In a similar manner, also, we find

$$a - (a - c) = a - a + c = + c = c\dagger.$$

We are thus necessarily led to the *assumption* of the existence of such quantities as $- c$ and $+ c$, or of symbols preceded

* Whatever general symbolical conclusions are true in arithmetical algebra must be true likewise in symbolical algebra, otherwise one science could not include the other. This is a most important principle, and will be the subject of particular notice hereafter.

† For it appears from arithmetical algebra that $a - a = 0$, and that $a - a + b = b$.

by the independent signs * $+$ and $-$, which no longer denote operations, though they *may* denote affections of quantity. It appears likewise that $+c$ is identical with c, but that $-c$ is a quantity of a different nature from c: the interpretation of its meaning must depend upon the joint consideration of the specific nature of the magnitude denoted by a, and of the symbolical conditions which the sign $-$, thus used, is required to satisfy †.

In a similar manner, the result of the operation, or rather the operation itself, of extracting the square root of such a quantity as $a - b$ is *impossible*, unless a is greater than b. To remove the limitation in such cases, (an essential condition in symbolical algebra,) we assume the existence of a sign such as $\sqrt{-1}$; so that if we should suppose $b = a + c$, we should get $\sqrt{(a-b)} = \sqrt{\{a-(a+c)\}} = \sqrt{(a-a-c)} = \sqrt{(-c)} = \sqrt{-1}\, c$ ‡. In a similar manner, in order to make the operation universally applicable, when the n^{th} root of $a - b$ is required, we assume the existence of a sign $\sqrt[n]{-1}$, for which, as will afterwards appear, equivalent symbolical forms can always be found, involving $\sqrt{-1}$ and numerical quantities.

By assuming, therefore, the independent existence of the signs $+$, $-$, $\sqrt[n]{1}$, and $\sqrt[n]{-1}$, $(1)^n$, and $(-1)^n$ §, we shall obtain a symbolical result in all those operations, which we call addition, subtraction, multiplication, division, extraction of roots, and raising of powers, though their meaning may or may *not* be identical with that which they possess in arithmetic. Let us now inquire a little further into the assumptions which determine the symbolical character and relation of these fundamental operations.

The operations called *addition* and *subtraction* are denoted by the signs $+$ and $-$.

They are the inverse of each other.

* That is, not preceded by other symbols as in the expressions $a - c$ and $a + c$.

† Amongst these conditions, the principal is, that if $-c$ be subjected to the operation denoted by the sign $-$, it will become identical with $+c$: thus, $a - (-c) = a + c$. It does not follow, however, that the sign $-$ thus used, must necessarily admit of interpretation.

‡ The symbolical form, however, of this and of similar signs is not arbitrary, but dependent upon the general laws of symbolical combination.

§ I do not assert the necessity of considering such signs as $\sqrt{-1}$, $(1)^n$, $(-1)^n$, as forming essentially a part of the earliest and most fundamental assumptions of algebra: the necessity for their introduction will arise when those operations with which they are connected are first required to be considered, and will in all cases be governed by the general principle above mentioned.

In the *concurrence* of the signs + and −, in whatever manner used, if two like signs come together, whether + and +, or − and −, they are replaced by the single sign +; and when two unlike signs come together, whether + and −, or − and +, they are replaced by the single sign −.

When different operations are performed or indicated, it is indifferent in what order they succeed each other.

The operations called *multiplication* and *division* are denoted by the signs × and ÷, or more frequently by a conventional position of the quantities or symbols with respect to each other: thus, the product of a and b is denoted by $a \times b$, $a \,.\, b$, or $a\,b$; the quotient of a divided by b is denoted by $a \div b$, or by $\frac{a}{b}$.

The operations of multiplication and division are the inverse of each other.

In the *concurrence* of the signs + and − in multiplication or division, if two like signs come together, whether + and +, or − and −, they are replaced by the single sign +; and if two unlike signs come together, whether + and −, or − and +, they are replaced by the single sign −.

When different operations succeed each other, it is not indifrent in what order they are taken.

We arrive at all these rules, when the operations are defined and when the symbols are numbers, by deductions, not from each other, but from the definitions themselves: in other words, these conclusions are not dependent upon each other, but upon the definitions only. In the absence, therefore, of such definitions of the meaning of the operations which these signs or forms of notation indicate, they become *assumptions*, which are independent of each other, and which serve to define, or rather to *interpret** the operations, when the specific nature of the symbols is known; and *which also identify the results of those operations with the corresponding results in arithmetical algebra, when the symbols are numbers and when the operations are arithmetical operations.*

The rules of symbolical combination which are thus assumed

* To *define*, is to assign beforehand the meaning or conditions of a term or operation; to *interpret*, is to determine the meaning of a term or operation conformably to definitions or to conditions previously given or assigned. It is for this reason, that we *define* operations in arithmetic and arithmetical algebra conformably to their popular meaning, and we *interpret* them in symbolical algebra conformably to the symbolical conditions to which they are subject.

have been *suggested only* by the corresponding rules in arithmetical algebra. They cannot be said to be *founded* upon them, for they are not *deducible* from them; for though the operations of addition and subtraction, in their arithmetical sense, are applicable to all quantities of the same kind, yet they necessarily require a different meaning when applied to quantities which are different in their nature, whether that difference consists in the kind of quantity expressed by the unaffected symbols, or in the different signs of affection of symbols denoting the same quantity; neither does it necessarily follow that in such cases there *exists* any interpretation which can be given of the operations, which is competent to satisfy the required symbolical conditions. It is for such reasons that the investigation of such interpretations, when they are discoverable, becomes one of the most important and most essential of the deductive processes which are required in algebra and its applications.

Supposing that all the operations which are required to be performed in algebra are capable of being symbolically denoted, the results of those operations will constitute what are called *equivalent* forms, the discovery and determination of which form the principal business of algebra. The greatest part of such equivalent forms result from the direct application of the rules for the fundamental operations of algebra, when these rules regard symbolical combinations only: but in other cases, the operations which produce them being neither previously defined nor reduced to symbolical rules, unless for some specific values of the symbols, we are compelled to resort, as we have already done in the discovery and assumption of the fundamental rules of algebra themselves, to the results obtained for such specific values, for the purpose of discovering the rules which determine the symbolical nature of the operation for *all* values of the symbols. As this principle, which may be termed *the principle of the permanence of equivalent forms,* constitutes the real foundation of all the rules of symbolical algebra, when viewed in connexion with arithmetical algebra considered as a science of suggestion, it may be proper to express it in its most general form, so that its authority may be distinctly appealed to, and some of the most important of its consequences may be pointed out.

Direct proposition:

Whatever form is algebraically equivalent to another when expressed in general symbols, must continue to be equivalent, whatever those symbols denote.

Converse proposition:

Whatever equivalent form is discoverable in arithmetical algebra considered as the science of suggestion, when the symbols are general in their form, though specific in their value, will continue to be an equivalent form when the symbols are general in their nature as well as in their form *.

The direct proposition must be true, since the laws of combination of symbols by which such equivalent forms are deduced, have no reference to the specific values of the symbols.

The converse proposition must be true, for the following reasons:

If there be an equivalent form when the symbols are general in their nature as well as in their form, it must coincide with the form discovered and proved in arithmetical algebra, where the symbols are general in their form but specific in their nature; for in passing from the first to the second, no change in its form can take place by the first proposition.

Secondly, we may assume the existence of such an equivalent form in symbols which are general both in their form and in their nature, since it will satisfy the only condition to which all such forms are subject, which is, that of perfect coincidence with the results of arithmetical algebra, as far as such results are common to both sciences.

Equivalent forms may be said to have a necessary existence when the operation which produces them admits of being defined, or the rules for performing it of being expressly laid down: in all other cases their existence may be said to be conventional or assumed. Such conventional results, however, are as much real results as those which have a necessary existence, in as much as they satisfy the only condition of their existence, which the principle of the permanence of equivalent forms imposes upon them: thus, the series for $(1 + x)^n$ has a necessary existence whenever the nature of the operation upon $1 + x$ which it indicates can be defined; that is, when n is a whole or a fractional, a positive or negative, number †; but for all other values of n, where no previous definition or interpretation of the nature of the operation which connects it with its equivalent series can be given, then its existence is *conventional* only, though, symbolically speaking, it is equally entitled to be considered as an equivalent form in one case as in the other.

It is evident that a system of symbolical algebra might be

* Peacock's *Algebra*, Art. 132.

† The meaning of $(1 + x)^n$ cannot properly be said to be defined when n is a fractional number, whether positive or negative, or a negative whole number, but to be ascertained by interpretation conformably to the principle of the permanence of equivalent forms.

formed, in which the symbols and the *conventional* operations to which they were required to be subjected would be perfectly general both in value and application. If, however, in the construction of such a system, we looked to the assumption of such rules of operation or of combination only, as would be sufficient, and not more than sufficient, for deducing equivalent forms, without any reference to any subordinate science, we should be altogether without any means of interpreting either our operations or their results, and the science thus formed would be one of symbols only, admitting of no applications whatever. It is for this reason that we adopt a subordinate science as a science of suggestion, and we frame our assumptions so that our results shall be the same as those of that science, when the symbols and the operations upon them become identical likewise : and in as much as arithmetic is the science of calculation, comprehending all sciences which are reducible to measure and to number ; and in as much as arithmetical algebra is the immediate form which arithmetic takes when its digits are replaced by symbols and when the fundamental operations of arithmetic are applied to them, *those symbols being general in form, though specific in value,* it is most convenient to assume it as the subordinate science, which our system of symbolical algebra *must be required to comprehend in all its parts.* The principle of the permanence of equivalent forms is the most general expression of this law, in as much as its truth is absolutely necessary to the identity of the results of the two sciences, when the symbols in both denote the same things and are subject to the same conditions. It was with reference to this principle that the fundamental assumptions respecting the operations of addition, subtraction, multiplication and division were said to be *suggested* by the ascertained rules of the operations bearing the same names in arithmetical algebra. The independent use of the signs + and −, and of other signs of affection, was an assumption requisite to satisfy the still more general principle of symbolical algebra, *that its symbols should be unlimited in value and representation, and the operations to which they are subject unlimited in their application.*

In arithmetical algebra, the definitions of the operations determine the rules; in symbolical algebra, the rules determine the meaning of the operations, or more properly speaking, they furnish the means of interpreting them : but the rules of the former science are invariably the same as those of the latter, in as much as the rules of the latter are assumed with this view, and merely differ from the former in the universality of their applications: and in order to secure this universality of their applications, such additional signs* are assumed, and of such a symbolical form, as those applications may render necessary. We call those rules, or their equivalent symbolical consequences, *assumptions*, in as much as they are not deducible as conclusions from any previous knowledge of those operations which have corresponding names: and we might call them *arbitrary* assumptions, in as much as they are *arbitrarily* imposed upon a science of symbols and their combinations, which might be adapted to any other assumed system of consistent rules. In the assumption, therefore, of a system of rules such as will make its symbolical conclusions necessarily coincident with those of arithmetical algebra, as far as they can exist in common, we in no respect derogate from the authority or completeness of symbolical algebra, considered with reference to its own conclusions and to their connexion with each other, at the same time that we give to them a meaning and an application which they would not otherwise possess.

It follows from this view of the relation of arithmetical and symbolical algebra, that all the results of arithmetical algebra which are general in form are true likewise in symbolical algebra, whatever the symbols may denote. This conclusion may be said to be true in virtue of the principle of the permanence of equivalent forms, or rather it may be said to be the proper expression of that principle. Its consequences are most important, as far as the investigation of the fundamental propositions of the science are concerned, in as much as it enables us to investigate them in the most simple cases, when the operations which produce them are perfectly defined and understood, and when the general symbols denote positive whole numbers. If *the conclusions thus obtained do not involve in their expression any condition which is essentially connected with the specific values of the symbols, they may be at once transferred to symbolical algebra, and considered as true for all values of the symbols whatsoever*†.

Thus, coefficients in arithmetical algebra, such as m in $m\,a$, which are general in form, lead to the interpretation of such

* There is no necessary limit to the multiplication of such signs : the signs $+$, $-$, $(1)^n$ and $(-1)^n$ and their equivalents (for the symbolical form of such signs is not arbitrary), comprehend all those signs of affection which are required by those operations with which we are at present acquainted.

† Some formulæ are essentially arithmetical: of this kind is $1 . 2 . 3 \dots r$, in which r must be a whole number. The formula $\dfrac{m(m-1)\dots(m-r+1)}{1 . 2 \quad . \quad . \quad . \quad r}$ is symbolical with respect to m, but arithmetical with respect to r. Such cases, and their extension to general values of r, will be more particularly considered hereafter.

expressions as $m\,a$ in symbolical algebra, when m is a number whole or fractional, and a any symbol whatsoever. When m, n and a are whole numbers, it very readily appears that $m\,a + n\,a = (m + n)\,a$, and that $m\,a - n\,a = (m - n)\,a$: the same conclusions are true likewise for all values of m, n and a. In arithmetical algebra we assume a^2, a^3, a^4, &c., to represent $a\,a$, $a\,a\,a$, $a\,a\,a\,a$, &c., and we readily arrive at the conclusion that $a^m \times a^n = a^{m+n}$, when m and n are whole numbers: the same conclusion must be true also when m and n are any quantities whatsoever. In a similar manner we pass from the result $(a^m)^n = a^{mn}$, when n is a whole number, to the same conclusion for all values of the symbols*.

The preceding conclusions are extremely simple and elementary, but they are not obtainable for *all* values of the symbols by the aid of any other principle than that of the permanence of equivalent forms: they are assumptions which are made in conformity with that principle, or rather for *the purpose of rendering that principle universal*; and it will of course follow that all interpretations of those expressions where m and n are not whole numbers must be subordinate to such assumptions. Thus, $\frac{a}{2} + \frac{a}{2} = \left(\frac{1}{2} + \frac{1}{2}\right)\,a = a$, and therefore $\frac{a}{2}$ must mean *one half* of a, whatever a may be; $a^{\frac{1}{2}} \times a^{\frac{1}{2}} = a^{\frac{1}{2}+\frac{1}{2}} = a^1 = a$, and therefore $a^{\frac{1}{2}}$ must mean the square root of a, whatever a may be, whenever such an operation admits of interpretation. In a similar manner $\frac{a}{3}$ must mean *one third part*, and $a^{\frac{1}{3}}$ the *cube root* of a, whatever a may be, and similarly in other cases: it follows, therefore, that the interpretation of the meaning of $a^{\frac{1}{2}}$, $a^{\frac{1}{3}}$, &c., is determined by the *general*

* The general theorems $m\,a + n\,a = (m + n)\,a$ and $m\,a - n\,a = (m - n)\,a$, $a^m \times a^n = a^{m+n}$ and $\frac{a^m}{a^n} = a^{m-n}$, $(a^m)^n = a^{mn}$ and $(a^m)^{\frac{1}{n}} = a^{\frac{m}{n}}$, which are deduced by the principle of the permanence of equivalent forms, and which are supplementary to the fundamental rules of algebra, are of the most essential importance in the simplification and abridgement of the results of those operations, though not necessary for the formation of the equivalent results themselves. It also appears from the four last of the above-mentioned theorems that the operations of multiplication and division, involution and evolution, are performed by the addition and subtraction, multiplication and division, of the indices, when adapted to the same symbol or base. If such indices or logarithms be calculated and registered with reference to a scale of their corresponding numbers, they will enable us to reduce the order of arithmetical operations by two unities, if their orders be regulated by the following scale; addition (1), subtraction (2), multiplication (3), division (4), involution (5), and evolution (6).

principle of *indices,* and also that we ought not to say that we assume $a^{\frac{1}{2}}$ to denote $\sqrt{a}$, and $a^{\frac{1}{3}}$ to denote $\sqrt[3]{a}$, as is commonly done*, in as much as such phrases would seem to indicate that such assumptions are independent, and not subject to the same common principle in all cases.

In all cases of indices which involve or designate the inverse processes of evolution, we must have regard likewise to the other great principle of symbolical algebra, which authorizes the existence of signs of affection. The square root of a may be either affected with the sign $+$ or with the sign $-$; for $+ a^{\frac{1}{2}} \times + a^{\frac{1}{2}}$, and $- a^{\frac{1}{2}} \times - a^{\frac{1}{2}}$, will equally have for their result $+ a$ or a, by the general rule for the concurrence of similar signs and the general principle of indices: in a similar manner $a^{\frac{1}{3}}$ *may* be affected with the multiple sign of affection $(1)^{\frac{1}{3}}$, if there are any symbolical values of $(1)^{\frac{1}{3}}$ different from $+ 1$ (equivalent to the sign $+$), which will satisfy the requisite symbolical conditions†. It is the *possible* existence of such signs of affection, which is consequent upon the universality of algebraical operations, which makes it expedient to distinguish between the results which are not affected by such signs, and the same results when affected by them. The first class of results or values are such as are alone considered in arithmetical algebra, and we shall therefore term them *arithmetical* values, though the quantities themselves may not be arithmetical: the second class may be termed algebraical values, in as much as they are altogether, as far as they are different from the arithmetical values, the results of the generality of the operations of symbolical algebra.

This distinction may generally be most conveniently expressed by considering such a sign as a factor, or a symbolical quantity multiplied according to the rule for that operation into the arithmetical value: in this sense $+ 1$ and $- 1$ may be considered as factors which are equivalent to the signs $+$ and $-$, that is, equivalent to affecting the quantities into which they are multiplied with the signs $+$ and $-$, according to the

* Wood's *Algebra*, DEFINITIONS.

† That is, if there is any symbolical expression different from 1, such as $\frac{-1 + \sqrt{-3}}{2}$, and $\frac{-1 - \sqrt{-3}}{2}$, the cubes of which are identical with 1. In a similar manner we may consider the existence of multiple values of 1^n or $(-1)^n$, and, therefore, of multiple signs of affection corresponding to them, as consequent upon the general laws of combination of symbolical algebra, and as results to be determined from those laws, and whose existence, also, is dependent upon them.

general rule for the concurrence of signs. In a similar manner we may consider $(1)^{\frac{1}{2}}(a)^{\frac{1}{2}}$ as equivalent to $(a)^{\frac{1}{2}}$; $(1)^{\frac{1}{3}}(a)^{\frac{1}{3}}$ as equivalent to $(a)^{\frac{1}{3}}$; $(1)^n a^n$ as equivalent to a^n; $(-1)^n (a)^n$ as equivalent to $(-a)^n$, and similarly in other cases: in all such cases the algebraical quantity into which the equivalent sign or its equivalent factor is multiplied, is supposed to possess its arithmetical value only*.

The series for $(1+x)^n$, when n is a whole number, may be exhibited under a general form, which is independent of the specific value of the index; for such a series may be continued indefinitely in form, though all its terms after the $(n+1)$th must become equal to zero. Thus, the series

$$(1+x)^n = (1)^n \left(1 + n\,x + \frac{n\,(n-1)}{1\,.\,2}\,x^2 + \right.$$

$$\left. + \frac{n\,(n-1)\,.\,.\,(n-r+1)}{1\,.\,2\,.\,.\,.\,r}\,x^r + \&c.\right)$$

indefinitely continued, in which n is particular in value (a whole number) though general in form, must be true also, in virtue of the principle of the permanence of equivalent forms, when n is general in value as well as in form†.

This theorem, which, singly considered, is, of all others, the most important in analysis, has been the subject of an almost unlimited variety of demonstrations. Like all other theorems whose consequences present themselves very extensively in algebraical results, it is more or less easy to pass from some one of those consequences to the theorem itself: but all the demonstrations which have been given of it, with the exception of the principle of one given by Euler‡, have been confined to such values of the index, namely, whole or fractional numbers, whether positive or negative, as made not only the development depend upon definable operations, but likewise assumed the existence of the series itself, leaving the form of its coefficients *alone* undetermined. It is evident, however, that if there existed a general form of this series, its form could

* This separation of the symbolical sign of affection from its arithmetical subject, or rather the expression of the signs of affection *explicitly*, and not *implicitly*, is frequently important, and affords the only means of explaining many paradoxes (such as the question of the existence of real logarithms of negative numbers), by which the greatest analysts have been more or less embarrassed.

† If such a series should, for any assigned value of n, have more symbolical values than one, one of them will be the arithmetical value, inasmuch as one symbolical value of 1^n is always 1.

‡ In the *Nov. Comm. Petropol.* for 1774.

be detected for any value of the index whatever, which was general in form, and therefore, also, when that index was a whole number; a case in which the interpretation of the operation designated by the index, as well as the performance of the operation itself, was the most simple and immediate.

That such a series, likewise, would satisfy the only symbolical conditions which the general principles of indices imposes upon the binomial, might be very easily shown; for if m and n be whole numbers, then if the two series

$$(1 + x)^m = 1^m \left(1 + m\,x + \frac{m\,(m-1)}{1\,.\,2}\,x^2 + \&c.\right)$$

$$(1 + x)^n = 1^n \left(1 + n\,x + \frac{n\,(n-1)}{1\,.\,2}\,x^2 + \&c.\right)$$

be multiplied together, according to the rule for that purpose, we must obtain

$$(1 + x)^{m+n} = 1^{m+n} \left(1 + (m+n)\,x + \frac{(m+n)\,(m+n-1)}{1\,.\,2}\ \&c.\right)$$

a series in which $m + n$ has replaced m or n in its component factors: and in as much as we must obtain the same symbolical result of this multiplication, whatever be the specific values of m and n, it follows, that if the same form of these series represents the development of $(1 + x)^n$ and $(1 + x)^n$, whatever m and n may be, then, likewise, the series for the product of $(1 + x)^m$ and $(1 + x)^n$, or $(1 + x)^{m+n}$, would be that which arose from putting $m + n$ in the place of m or n in each of the component factors. If, therefore, we assumed S (m) and S (n) to represent the series for $(1 + x)^m$ and $(1 + x)^n$, when m and n are any quantities whatsoever, then $(1 + x)^m \times (1 + x)^n = (1 + x)^{m+n} = \text{S}\,(m + n) = \text{S}\,(m) \times \text{S}\,(n)$; or, in other words, the series will possess precisely the same symbolical properties with the binomial to which they are required to be equivalent.

It is the equation $a^m \times a^n = a^{m+n}$, for all values of m and n, which determines the interpretation of a^m or a^n, when such an interpretation is possible; in other words, such quantities possess no properties which are independent of that equation. The same remark of course extends to $(1 + x)^n$, for all values of n, and similarly, likewise, to those series which are equivalent to it. That all such series must possess the same form would be evident from considering that the symbolical properties of $(1 + x)^n$ undergo no change for a change in the value of n, and that no series could be permanently equivalent to it whose form was not equally permanent likewise. In assuming, therefore, the existence of such a permanent series, our symbolical conclusions are necessarily consistent with each other, and it is the interpretation of the operations which produce them, which must be made in conformity with them. It is true that we can extract the square or the cube root of $1 + x$, and we can also determine the corresponding series by the processes of arithmetical algebra; and we likewise interpret $(1 + x)^{\frac{1}{2}}$ and $(1 + x)^{\frac{1}{3}}$ to mean the square and the cube root of $1 + x$, in conformity with the general principle of indices. The coincidence of the series for $(1 + x)^{\frac{1}{2}}$ and $(1 + x)^{\frac{1}{3}}$, whether produced by the processes of arithmetical algebra, or deduced by the principle of the permanence of equivalent forms from the series for $(1 + x)^n$, would be a proof of the correctness of our interpretation, not a condition of the truth of the general principle itself.

In order to distinguish more accurately the precise limits of hypothesis and of proof in the establishment of the fundamental propositions of symbolical algebra, it may be expedient to restate, at this point in the progress of our inquiry, the order in which the hypotheses and the demonstrations succeed each other.

We are supposed to be in possession of a science of arithmetical algebra whose symbols denote numbers or arithmetical quantities only, and whose laws of combination are capable of strict demonstration, without the aid of any principle which is not furnished by our knowledge of common arithmetic.

The symbols in arithmetical algebra, though general in form, are not general in value, being subject to limitations, which are necessary in many cases, in order to secure the practicability or possibility of the operations to be performed. In order to effect the transition from arithmetical to symbolical algebra, we now make the following hypotheses:

(1.) The symbols are unlimited, both in value and in representation.

(2.) The operations upon them, whatever they may be, are possible in all cases.

(3.) The laws of combination of the symbols are of such a kind as to coincide *universally* with those in arithmetical algebra when the symbols are arithmetical quantities, and when the operations to which they are subject are called by the same names as in arithmetical algebra.

The most general expression of this last condition, and of its connexion with the first hypothesis, is the *law of the perma-*

nence of equivalent forms, which is our proper guide in the establishment of the fundamental propositions of symbolical algebra, in the invention of the requisite signs, and in the determination of their symbolical form: but in the absence of the complete enunciation of that law, we may proceed with the investigation of the fundamental rules for addition, subtraction, multiplication and division, and of the theorems for the collection of multiples, and for the multiplication and involution of powers of the same symbol, which will, in fact, form a series of *assumptions* which are not arbitrary, but subordinate to the conditions which are imposed by our hypotheses: but if we suppose those conditions to be incorporated into one general law, whose truth and universality are admitted, then those assumptions become necessary consequences of this law, and must be considered in the same light with other propositions which follow, directly or indirectly, from the first principles of a demonstrative science. In the same manner, if we *assume* the existence of such signs as are requisite to secure the universality of the operations, the symbolical form of those signs, and the laws which regulate their use, will be determined by the same principles upon which the ordinary results of symbolical algebra are founded.

The natural and necessary dependence of these two methods of proceeding upon each other being once established, we may adopt either one or the other, as may best suit the form of the investigation which is under consideration: the great and important conclusion to which we arrive in both cases being, the transfer of all the conclusions of arithmetical algebra which are general in form (that is, which do not involve in their expression some restriction which limits the symbols to *discontinuous* values,) to symbolical algebra, accompanied by the invention or use of such signs (with determinate symbolical forms) as may be necessary to satisfy so general an hypothesis.

There are many expressions which involve symbols which are necessarily *discontinuous* in their value, either from the form in which they present themselves in such expressions or from some very obvious conventions in their use: thus, when we say that

$$\cos x = \cos (2 r \pi + x),$$
$$\text{and} - \cos x = \cos \{ (2r + 1) \pi + x \}$$

propositions which are only true when r is a whole number, the limitation is conveyed (though imperfectly) by the conventional use of $2 r$ and $2 r + 1$ to express *even* and *odd* numbers; for otherwise there would be no sufficient reason for not using the simple symbol r both in one case and the other. In a similar manner, in the expression of Demoivre's theorem

$$(\cos \theta + \sqrt{-1} \sin \theta)^n$$
$$= \cos (2 r n \pi + n \theta) + \sqrt{-1} \sin (2 r n \pi + n \theta),$$

we may suppose n to be any quantity whatsoever*, but r is necessarily a whole number.

In some cases, however, the construction of the formula itself will sufficiently express the necessary restriction of the values of one or more of its symbols, without the necessity of resorting to any convention connected with their introduction: thus, the formula $1 \times 2 \times 3 \ldots\ldots r$, commencing from 1, is essentially arithmetical, and limited by its form to whole and positive values of r. The same is the case with the formula $r(r-1) \ldots . 3 . 2 . 1$, where some of the successive and strictly arithmetical values of the terms of the series r, $r-1$, &c., are put down; but the formula $r(r-1)(r-2) \ldots .$ is subject to no such restriction, in as much as any number of such factors may be formed and multiplied together, whatever be the value of r. In a similar manner, the formula

$$\frac{n(n-1) \ldots (n-r+1)}{1 . 2 \ldots r},$$

which is so extensively used in analysis, is unlimited with respect to the symbol n, and essentially limited with respect to the symbol r: it is under such circumstances that it presents itself in the development of $(1+x)^n$.

In the differential calculus we readily find

$$\frac{d^r x^n}{d x^r} = n(n-1) \ldots (n-r+1) x^{n-r},$$

and in a similar manner also

$$\frac{d^r}{d x^r}(x^n + C_1 x^{r-1} + C_2 x^{r-2} + \ldots C_n) = n(n-1) \ldots (n-r+1) x^{n-r}:$$

in both these cases the value of n is unlimited, whilst the value of r is essentially a positive whole number; in other words,

* The investigation of this formula (like the equivalent series for $(1+x)^n$ when n is a general symbol,) requires the aid of the principle of the permanence of equivalent forms, in common with all other theorems connected with the general theory of indices. The formula above given involves also *implicitly* any sign of affection which the general value of n may introduce: for

$$(\cos \theta + \sqrt{-1} \sin \theta)^n = (1)^n (\cos n \theta + \sqrt{-1} \sin n \theta)$$
$$= (\cos 2 r n \pi + \sqrt{-1} \sin 2 r n \pi)(\cos n \theta + \sqrt{-1} \sin n \theta)$$
$$= \cos (2 r n \pi + n \theta) + \sqrt{-1} \sin (2 r n \pi + n \theta)$$

the principle of equivalent forms might be extended to this formula (supposing it to be investigated for integral values of n and r,) as far as the symbol n is concerned only. This arithmetical coefficient of *differentiation* (if such a term may be applied to it,) will present itself in the expression of the rth differential coefficient (r being a whole positive number,) of all algebraical functions *; and it is for this reason that we are *apparently* debarred from considering fractional or *general* indices of differentiation when applied to such functions, and that we are consequently prevented from treating the differential and integral calculus as the same branch of analysis whose general laws of derivation are expressed by common formulæ.

But is it not possible to exhibit the coefficient of differentiation under some equivalent form which may include general values of the index of differentiation? It is well known that the definite integral

$$\int_0^1 dx \left(\log \frac{1}{x}\right)^n \dagger$$

(adopting Fourier's notation,) is equal to $1 \,.\, 2 \ldots . n$, when n is a whole number; and that consequently, under the same circumstances, the coefficient of differentiation or

$$n(n-1)\ldots(n-r+1) = \frac{\int_0^1 dx \left(\log \frac{1}{x}\right)^n}{\int_0^1 dx \left(\log \frac{1}{x}\right)^{n-r}}:$$

and in as much as the *form* of this equivalent expression is not restricted to integral and positive values of r, we may assume

* Thus if $u = \frac{1}{1+x^2}$, we have $\frac{d^r u}{dx^2} = \frac{(-1)^r \,.\, 2 \,.\, 3 \ldots (r+1)\, x^r}{(1+x^2)^{r+1}}$

$$\times \left\{1 - \frac{r(r-1)}{2 \,.\, 3\, x^2} + \frac{r(r-1)(r-2)(r-3)}{2 \,.\, 3 \,.\, 4 \,.\, 5\, x^4} - \&c.\right\}:$$

and if $u = \frac{1}{\sqrt{(1+x^2)}}$, we have $\frac{d^r u}{dx^r} = \frac{1 \,.\, 2 \ldots . r}{(1-x^r)^{r+\frac{1}{2}}}$

$$\times \left\{1 + \frac{1}{2^2} \cdot \frac{r(r-1)}{x^2} + \frac{1}{2^2 \,.\, 4^2} \frac{r(r-1)(r-2)(r-3)}{x^4} + \&c.\right\}$$

† This definite integral, the second of that class of *transcendents* to which Legendre has given the name of *Integrales Euleriennes,* was first considered by Euler in the fifth volume of the *Commentarii Petropolitani,* in a memoir on the interpolation of the terms of the series

$$1 + 1 \times 2 + 1 \times 2 \times 3 + 1 \times 2 \times 3 \times 4 + \&c.,$$

which is full of remarkable views upon the generalization of formulæ and their interpretation. The same memoir contains the first solution of a problem involving fractional indices of differentiation.

it to be *permanent*, so long as we do not at the same time assume the necessary existence and interpretation of equivalent results. If, however, such results can be found, either generally, or for particular values (not integral and positive) of r, apart from the sign of integration, the consideration of the values of the corresponding differential coefficients will involve no other theoretical difficulty than that which attends the transition from integral to fractional and other values of common indices.

Euler, in his Differential Calculus*, has given the name of *inexplicable functions* to those functions which are apparently restricted by their form to integral and positive values of one or more of the general symbols which they involve: of this kind are the functions

$$1 \times 2 \times 3 \times \quad \ldots\ldots \quad x,$$

$$\frac{1}{1} + \frac{1}{2} + \frac{1}{3} + \quad \ldots\ldots \quad \frac{1}{x},$$

$$\frac{1}{1^n} + \frac{1}{2^n} + \frac{1}{3^n} + \quad \ldots\ldots \quad \frac{1}{x^n},$$

$$1 + \frac{a-b}{a+b} + \frac{a-2b}{a+3b} + \ldots \frac{a-(x-1)\,b}{a+(x+1)\,b},$$

and innumerable others which present themselves in the theory of series. The attempts which he has made to interpolate the series of which such functions form the general terms, are properly founded upon the hypothesis of the existence of permanent equivalent forms, though it may not be possible to exhibit the explicit forms themselves by means of the existing signs and symbols of algebra. In the cases which we have hitherto considered, the forms which were assumed to be permanent had a real previous existence, which necessarily resulted from operations which were capable of being defined. In the case of inexplicable functions, the corresponding permanent forms which hypothetically include them, may be considered as having an hypothetical existence only, whose form degenerates into that of the inexplicable function in the case of integral and positive values of the independent variable or variables. It is for the expression of such cases that definite integrals find their most indispensable usage.

* *Institutiones Calculi Differentialis*, Capp. xvi. et xvii. See also an admirable posthumous memoir of the same author amongst the additions to the Edition of that work printed at Pavia in 1787. He had been preceded in such researches by Stirling, an author of great genius and originality, whose labours upon the interpolation of series and other subjects have not received the attention to which they are justly entitled.

It is easy to construct formulæ which may exhibit the possibility of their thus degenerating into others of a much more simple form, when one or more of the independent variables become whole numbers: of this kind is the formula

$$\frac{\alpha + \beta \sin(2 r \pi + \theta) + \gamma \sin(2 r \pi + \theta') + \&c.}{\alpha + \beta \sin\theta + \gamma \sin\theta' +, \&c.} \times \phi(r),$$

which is, or is not, identical with $\phi(r)$, according as r is a whole or a fractional number: such functions are termed *undulating* functions by Legendre *. We can conceive also the possible existence of many other transcendents amongst the unknown and undiscovered results of algebra, which may possess a similar property.

The transcendent

$$\int_0^1 d x \left(\log \frac{1}{x}\right)^n$$

mentioned above, possesses many properties which give it an uncommon importance in analysis, and most of all from its furnishing the connecting link in the transition from integral and positive to *general* indices of differentiation in algebraical functions. If we designate, as Legendre has done,

$$\int_0^1 d x \left(\log \frac{1}{x}\right)^r \text{by } \Gamma(1 + r),$$

we shall readily derive the fundamental equation

$$\Gamma(1 + r) = r\, \Gamma(r) \dagger \quad \ldots \ldots \quad (1)$$

which is in a form which admits of all values of r. It appears

* *Traité des Fonctions Elliptiques*, tom. xi. p. 476.

† In as much as

$$\frac{d^{n-r}}{d x^{n-r}} x^n = \frac{\Gamma(1+n)}{\Gamma(1+r)} x^r = \mathrm{A}\, x^r,$$

and

$$\frac{d^{n-r+1}}{d x^{n-r+1}} x^n = \frac{\Gamma(1+n)}{\Gamma(r)} x^{r-1} = \mathrm{B}\, x^{r-1} = r\, \mathrm{A}\, x^{r-1},$$

it follows that $r\,\mathrm{A} = \mathrm{B}$, and therefore also that

$$\frac{r}{\Gamma(1+r)} = \frac{1}{\Gamma(r)} \text{ or } r\,\Gamma(r) = \Gamma(1+r),$$

which is the equation (1): and it is obvious that the transition from

$$\frac{d^{n-r}}{d x^{n-r}} x^n \text{ to } \frac{d^{n-r+1}}{d x^{n-r+1}} x^n$$

(which is equivalent to the simple differentiation of $\mathrm{A}\, x^r$, when A is a constant coefficient), will lead to the same relation between $\Gamma(1 + r)$ and $\Gamma(r)$, *whatever* be the value of r, whether positive or negative, whole or fractional. Legendre has apparently limited this equation to positive values of r, also from this equation that if the values of the transcendent $\Gamma(r)$ can be determined for all values of r which are included

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(*Fonctions Elliptiques*, tom. ii. p. 415,) a restriction which is obviously unnecessary.

There are two cases in which the coefficient of x^{n-r} in the equation

$$\frac{d^r x^n}{d x^r} = \frac{\Gamma(1+n)}{\Gamma(1+n-r)} x^{n-r}$$

requires to be particularly considered: the first is that in which this coefficient becomes *infinite*; the second, that in which it becomes equal to *zero*.

The numerator $\Gamma(1+n)$ will be *infinite* when n is any negative whole number; the denominator $\Gamma(1+n-r)$ will become infinite when $n-r$ is any negative whole number, and in no other case: if n be a negative whole number, and if r be a whole number, either positive or negative, such that $n-r$ is negative, then the coefficient $\frac{\Gamma(1+n)}{\Gamma(1+n-r)}$ becomes finite, in as much as $\Gamma(-t)$ (if t be a whole number) $= \frac{\Gamma(0)}{1.2\,..\,t.(-1)^t}$, and $\Gamma(0)$ disappears, therefore, by division: thus all the coefficients of $\frac{d^{-r}}{d x^{-r}} \cdot \frac{1}{x}$ are infinite, unless r be a negative whole number, such as $-m$, in which case it becomes $1.2\,..\,m.(-1)^m$, a result which is easily verified. In a similar manner it would appear that the coefficients of $\frac{d^r}{d x^{-r}} \cdot \frac{1}{x^n}$ are infinite, when n is a positive number, unless r be a negative whole number equal to, or greater than, n.

The coefficient $\frac{\Gamma(1+n)}{\Gamma(1+n-r)}$ will become equal to zero, when $1+n-r$ *is*, and when $1+n$ is *not*, equal to zero or to any negative whole number; for, under such circumstances, the denominator is *infinite* and the numerator is *finite*.

As the most important consequences will be found to result from these *critical* values of the coefficient of differentiation, we shall proceed to examine them somewhat in detail.

(1.) The simple differentials or differential coefficients of *constant* quantities are equal to zero, whilst the differentials or differential coefficients to *general* indices (positive whole numbers being excepted,) are variable.

Thus

$$\frac{d^{\frac{1}{2}} a}{d x^{\frac{1}{2}}} = \frac{d^{\frac{1}{2}}.a x^0}{d x^{\frac{1}{2}}} = \frac{a\,\Gamma(1)}{\Gamma(\frac{1}{2})} . x^{0-\frac{1}{2}} = \frac{a}{\sqrt{\pi x}} : \frac{d^{-\frac{1}{2}} a}{d x^{-\frac{1}{2}}}$$

$$= \frac{a(\Gamma 1)}{\Gamma(\frac{3}{2})} . x^{0+\frac{1}{2}} = \frac{2\sqrt{x}}{\sqrt{\pi}} : \frac{d^{-1} a}{d x^{-1}} = \frac{\Gamma(1)}{\Gamma(2)} . a x^{0+1} = a x;$$

and similarly in other cases.

(2.) The differentials of *zero* to general indices (positive whole numbers being excepted) are not *necessarily* equal to *zero*.

Thus, if we suppose

$$a = 0 = \frac{C}{\Gamma(0)} x^{-n}, \text{ we get } \frac{d^r 0}{d x^r} = \frac{C}{\Gamma(0)} \cdot \frac{\Gamma(1-n)}{\Gamma(1-n-r)} . x^{-n-r}:$$

if n be a positive whole number, $\Gamma(1-n) = \infty$, and this expression is *finite* unless $\Gamma(1-n-r) = \infty$, in which case it is *zero*: if r be also a positive whole between any two successive whole numbers, they can be determined for all other values of r. Euler * first assigned the

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number, it is always *zero*: if $r = -1$, it is finite when $n = 1$: if $r = -2$, it is finite when $n = 1$ or $n = 2$: if $r = -3$, it is finite when $n = 1$, or $n = 2$, or $n = 3$: and generally if r be any negative whole number, there will be finite values corresponding to every value of n from 1 to $-r$; we thus get

$$\frac{d^{-1}\,0}{d\,x^{-1}} = \mathrm{C}$$

$$\frac{d^{-2}\,0}{d\,x^{-2}} = \mathrm{C}\,x + \mathrm{C}_1$$

$$\frac{d^{-2}\,0}{d\,x^{-2}} = \frac{\mathrm{C}\,x^2}{1\,.\,2} + \mathrm{C}_1\,x + \mathrm{C}_2$$

$$\frac{d^{-n}\,0}{d\,x^{-n}} = \frac{\mathrm{C}\,x^{n-1}}{1\,.\,2\,.\,.\,(n-1)} + \frac{\mathrm{C}_1\,.\,x^{n-2}}{1\,.\,2\,.\,.\,(n-2)} + \mathrm{C}_{n-2}\,.\,x + \mathrm{C}_{n-1}.$$

This is the true theory of the introduction of complementary arbitrary functions in the ordinary processes of integration.

More generally, if r be not a whole number,

$$\frac{d^r\,0}{d\,x^r} = \frac{\mathrm{C}}{\Gamma\,(0)}\,.\,\frac{\Gamma\,(1-n)}{\Gamma\,(1-n-r)}\,x^{-n-r},$$

which will be finite when n is a positive whole number and when $1 - n - r$ is not a negative whole number: thus if n be any number in the series 1, 2, 3 . . ., and if $r = \frac{1}{2}$, then

$$\frac{d^{\frac{1}{2}}\,0}{d\,x^{\frac{1}{2}}} = \frac{\mathrm{C}}{\Gamma\,(-\frac{1}{2})}\,.\,x^{-\frac{3}{2}} \text{ or } \frac{\mathrm{C}}{\Gamma\,(-\frac{3}{2})}\,x^{-\frac{5}{2}} \text{ or } \frac{\mathrm{C}}{\Gamma\,(-\frac{5}{2})}\,x^{-\frac{7}{2}}$$

and so on for ever: consequently,

$$\frac{d^{\frac{1}{2}}\,0}{d\,x^{\frac{1}{2}}} = \frac{\mathrm{C}}{x^{\frac{3}{2}}} + \frac{\mathrm{C}_1}{x^{\frac{5}{2}}} + \frac{\mathrm{C}_2}{x^{\frac{7}{2}}} + \&\text{c. } \textit{in infinitum.}$$

In a similar manner, we shall find

$$\frac{d^{-\frac{5}{2}}\,0}{d\,x^{-\frac{5}{2}}} = \mathrm{C}\,x^{\frac{3}{2}} + \mathrm{C}_1\,x^{\frac{1}{2}} + \frac{\mathrm{C}_2}{x^{\frac{1}{2}}} + \frac{\mathrm{C}_3}{x^{\frac{3}{2}}} + \&\text{c. } \textit{in infinitum.}$$

The knowledge of these complementary arbitrary functions will be found of great importance for the purpose of explaining some results of the general differentiation of the same function under different forms which would otherwise be irreconcileable with each other.

(3.) The differential coefficient will be *zero*, when n is *not*, and when $n - r$ *is*, a negative whole number.

Thus,

$$\frac{d^2\,x}{d\,x^2} = 0,\quad \frac{d^3\,x^2}{d\,x^3} = 0,\quad \frac{d^{\frac{3}{2}}\,x^{\frac{1}{2}}}{d\,x^{\frac{3}{2}}} = 0,\quad \frac{d^{\frac{3}{2}}\,x^{-\frac{1}{2}}}{d\,x^{\frac{3}{2}}} = 0,\quad \frac{d^{-\frac{5}{2}}\,x^{-\frac{7}{2}}}{d\,x^{-\frac{5}{2}}} = 0:$$

and similarly in other cases.

(4.) The differentials of ∞ are not *necessarily* equal to ∞, but may be *finite.* If we represent ∞ by $\mathrm{C}\,\Gamma\,(0)$, we shall find

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* *Commentarii Petrop.*, vol. v. 1731.

value of $\Gamma\left(\frac{1}{2}\right) = \sqrt{\pi}$, by the aid of the very remarkable expression for π, which Wallis derived from his theory of inter-

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$$\frac{d^r\,(\infty)\,x^0}{d\,x^r} = \mathrm{C}\,\Gamma(0)\,.\,\frac{\Gamma(1)\,x^{-r}}{\Gamma(1-r)} = (-1)^{r-1}\,.\,1\,.\,2\,.\,.\,(r-1)\,.\,x^{-r},$$

whenever r is a positive whole number.

Conversely also,

$$\frac{d^{-1}\,.\,x^{-1}}{d\,x^{-1}} = \frac{\Gamma(0)}{\Gamma(1)}\,.\,x^0, \text{ where } \frac{\Gamma(0)}{\Gamma(1)} = \infty.$$

$$\frac{d^{-2}\,.\,x^{-1}}{d\,x^{-2}} = \frac{\Gamma(0)}{\Gamma(2)}\,.\,x^1,$$

$$\frac{d^{-3}\,.\,x^{-1}}{d\,x^{-3}} = \frac{\Gamma(0)}{\Gamma(3)}\,.\,x^2,$$

$$\frac{d^{-r}\,.\,x^{-1}}{d\,x^{-r}} = \frac{\Gamma(0)}{\Gamma(r)}\,.\,x^{r-1},$$

the arbitrary complementary functions being omitted.

(5.) The occurrence of infinite values of the coefficient of differentiation will generally be the indication of some essential change of form in the transition from the primitive function to its corresponding differential coefficients.

Thus,

$$\frac{d^{-1}}{d\,x^{-1}}\,.\,\frac{1}{x} = \frac{\Gamma(0)}{\Gamma(1)}\,.\,x^0 = \log x + \mathrm{C};$$

this last result or value of $\frac{\Gamma(0)}{\Gamma(1)}\,.\,x^0$ being obtained by the ordinary process of integration: and generally,

$$\frac{d^{-r}\,.\,x^{-1}}{d\,x^{-r}} = \frac{\Gamma(0)}{\Gamma(r)}\,x^{r-1} + \frac{\mathrm{C}\,x^{r-1}}{\Gamma(r)} + \frac{\mathrm{C}_1\,x^{r-2}}{\Gamma(r-1)} + \&\text{c.}\ldots,$$

the first term of which is *infinite*, in all cases in which r is not a *negative* whole number, in which case it becomes equal to $(-1)^{-r}\,1\,.\,2\ldots(-r)\,x^{r-1}$, the complementary arbitrary functions also disappearing. If we suppose, however, r to be a positive whole number, and if we replace $\frac{\Gamma(0)}{\Gamma(1)}\,.\,x^0$ by its transcendental value already determined, we shall get

$$\frac{d^{-r}\,x^{-1}}{d\,x^{-r}} = \frac{x^{r-1}}{\Gamma(r)}\left\{\log x + \mathrm{C}\right\} + \frac{\mathrm{C}_1\,x^{r-2}}{\Gamma(r-1)} + \&\text{c.},$$

which may be replaced by

$$\frac{d^{-r}\,x^{-1}}{d\,x^{-r}} = \frac{x^{r-1}}{\Gamma(r)}\left\{\log x + (-1)^r\,\Gamma(r)\,.\,\Gamma(1-r) + \mathrm{C}\right\} + \frac{\mathrm{C}_1\,x^{r-2}}{\Gamma(r-1)} + \ldots,$$

which is in a form which is true for all values of r whatsoever, and which coincides, for integral values of r, with the form determined by the ordinary process of integration.

More generally,

$$\frac{d^{-r}\,.\,x^{-n}}{d\,x^{-r}} = \frac{\Gamma(1-r)}{\Gamma(1-n+r)}\,.\,x^{-n+r} + \frac{\mathrm{C}\,.\,x^{r-n}}{\Gamma(r-n+1)} + \frac{\mathrm{C}_1\,.\,x^{r-n-1}}{\Gamma(r-n)} + \ldots,$$

which is finite, whilst r is less than n; and when r and n are whole numbers, becomes $= (-1)^r\,.\,\frac{\Gamma(n-r)}{\Gamma(n)}\,x^{r-n}$, omitting complementary functions.

polations; and subsequently, by a much more direct process, which lead to the equation,

$$\Gamma(r)\,\Gamma(1 \quad r) = \frac{\pi}{\sin r\,\pi} \text{ (when } r > 0 < 1):$$

If, under the same circumstances, r be greater than n, the coefficient of differentiation becomes infinite, and its value, determined as above, becomes

$$= \frac{x^{r-n}}{\Gamma(n)\,\Gamma(r-n+1)}\{\log x + \mathrm{C}\} + \frac{\mathrm{C}_1\, x^{r-n-1}}{\Gamma(r-n)} + \&c.$$

$$= \frac{x^{r-n}}{\Gamma(n)\,\Gamma(r-n+1)}\{\log x + (-1)^r\,\Gamma(r-n+1)\,\Gamma(n-r) + \mathrm{C}\}$$
$$+ \frac{\mathrm{C}_1\, x^{r-n-1}}{\Gamma(r-n)} + \ldots,$$

which is in a form adapted to all values of r.

The cases which we have considered above are the only ones in which the coefficient of differentiation will become *infinite*, in consequence of the introduction of $\log x$ in the expression of its value. We shall have occasion hereafter to notice more particularly the meaning of infinite values of coefficients as indications of a change in the constitution of the function into which they are multiplied.

(6.) If $u = (a\,x + b)^n$, then

$$\frac{d^r u}{d x^r} = \frac{\Gamma(1+n)\,a^r}{\Gamma(1+n-r)} \cdot (a\,x+b)^{n-r} + \frac{\mathrm{C}\,x^{r-1}}{\Gamma(-r)} + \ldots.$$

For if $v = a\,x + b$, then $\dfrac{d\,v}{d\,x} = a$ and $\dfrac{d^2 v}{d\,x^2} = 0$; and therefore

$$\frac{d^r u}{d x^r} = \frac{\Gamma(1+n)}{\Gamma(1+n-r)}\, v^{n-r}\left(\frac{d\,v}{d\,x}\right)^r + \frac{\mathrm{C}\,x^{r-1}}{\Gamma(-r)} + \&c.$$

Thus if $u = (x+1)^2$, we get

$$\frac{d^{\frac{1}{2}} u}{d x^{\frac{1}{2}}} = \frac{\Gamma(3)}{\Gamma(\frac{5}{2})}\,(x+1)^{\frac{3}{2}} + \frac{\mathrm{C}}{\Gamma(-\frac{1}{2})\,x^{\frac{1}{2}}} + \frac{\mathrm{C}_1}{\Gamma(-\frac{3}{2})\,x^{\frac{3}{2}}} + \&c.$$

$$= \frac{8}{3\sqrt{\pi}} \cdot (x+1)^{\frac{3}{2}} + \frac{\mathrm{C}}{x^{\frac{1}{2}}} + \frac{\mathrm{C}_1}{x^{\frac{3}{2}}} + \frac{\mathrm{C}_2}{x^{\frac{5}{2}}} + \&c.$$

If we replace $(x+1)^2$ by $x^2 + 2 + x\ 1$, we shall get

$$\frac{d^{\frac{1}{2}} u}{d u^{\frac{1}{2}}} = \frac{\Gamma(3)}{\Gamma(\frac{5}{2})}\,x^{\frac{3}{2}} + 2\,\frac{\Gamma(2)}{\Gamma(\frac{3}{2})}\,x^{\frac{1}{2}} + \frac{\Gamma(1)}{\Gamma(\frac{1}{2})} \cdot \frac{1}{x^{\frac{1}{2}}}$$

$$+ \frac{\mathrm{C}}{\Gamma(-\frac{1}{2})\,x^{\frac{3}{2}}} + \frac{\mathrm{C}_1}{\Gamma(-\frac{3}{2})\,x^{\frac{5}{2}}} + \&c.,$$

$$= \frac{8}{3\sqrt{\pi}}\,x^{\frac{3}{2}} + \frac{4}{\sqrt{\pi}}\,x^{\frac{1}{2}} + \frac{2}{\sqrt{\pi\,x}} + \frac{\mathrm{C}}{x^{\frac{3}{2}}} + \&c.$$

It thus appears that the two results may be made to coincide with each other, when $(x+1)^{\frac{3}{2}}$ in the first of them is developed, by the aid of the proper arbitrary functions.

The necessity of this introduction of arbitrary functions to restore the required identity of the expressions deduced for the same differential coefficients, presents itself also in the ordinary processes of the integral calculus: thus, if $u = (x+1)^2$, we find

Legendre, following closely in the footsteps of this illustrious analyst, has succeeded in the investigation of methods by which the values of this transcendent $\Gamma(r)$ may be calculated to any required degree of accuracy for all positive values of r, and has

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$$\frac{d^{-1}u}{dx^{-1}} = \frac{(x+1)^3}{3} + C; \frac{d^{-2}u}{dx^{-2}} = \frac{(x+1)^4}{3.4} + Cx + C_1$$

$$= \frac{x^4}{12} + \frac{x^3}{3} + \frac{x^2}{2} + \frac{x}{3} + \frac{1}{12} + Cx + C_1.$$

If we replace $(x+1)^2$ by $x^2 + 2x + 1$, we find

$$\frac{d^{-2}u}{dx^{-2}} = \frac{x^4}{12} + \frac{x^3}{3} + \frac{x^2}{2} + Cx + C_1.$$

It is obvious that these two values of $\frac{d^{-2}u}{dx^{-2}}$ cannot be made identical, without the aid of the *proper* arbitrary functions.

(7.) Let $u = v^n$ where $v = f(x)$: and let it be required to find $\frac{d^r u}{dx^r}$.

The general expression for $\frac{d^r u}{dx^r}$, when r is a whole number, is generally extremely complicated, though the law of formation of its terms can always be assigned. If the *inexplicable* expressions in the resulting series be replaced by their proper transcendents, the expression may be generalized for any value of r.

If $\frac{dv}{dx} = p$ and if $\frac{d^2v}{dx^2} = c$, a constant quantity, then $\frac{d^r u}{dx^r} = n(n-1) \dots (n-r+1)\, v^{n-r} p^r$

$$\times \left\{1 + \frac{r(r-1)}{1(n-r+1)} \cdot \frac{cv}{p^2} + \frac{r(r-1)(r-2)(r-3)}{1.2.(n-r+1)(n-r+2)} \cdot \frac{c^2v^2}{p^4} + \&c.\right\}$$

$$= \frac{\Gamma(1+n)}{\Gamma(1+n-r)} v^{n-r} p^2 \left\{1 + \frac{r(r-1)}{1.(n-r+1)} \cdot \frac{cv}{p^2} + \&c.\right\}$$

$$+ \frac{C.}{\Gamma(-r)} \cdot x^{r-1} + \frac{C_1}{\Gamma(-r-1)} x^{r-2} + \&c.$$

which is in a form adapted to any value of r.

If $u = \frac{1}{\sqrt{1+x^2}}$ and $r = \frac{1}{2}$, we shall find

$$\frac{d^{\frac{1}{2}} \frac{1}{\sqrt{1-x^2)}}}{dx^{\frac{1}{2}}} = \frac{\sqrt{\pi x}}{(1-x^2)} \left\{1 - \frac{1}{2^4 x^2} - \frac{3.5}{2^{10} x^4} - \frac{3.5.7}{2^{14} x^6} - \&c.\right\}$$

$$+ \frac{C}{x^{\frac{1}{2}}} + \frac{C_1}{x^{\frac{3}{2}}} + \&c.$$

Rational functions of x may be resolved into a series of fractions, whose denominators are of the form $(x+a)^n$, and whose numerators are constant quantities, whose rth differential coefficients may be found by the methods given above. Irrational functions must be treated by general methods similar to that followed in the example just given, which will be more or less complicated according to the greater or less number of successive simple differentials of the function beneath the radical sign, which are not equal to zero.

given tables of its logarithmic values to twelve places of decimals, with columns of three orders of differences for 1000 equal intervals between 1 and 2*; and similar tables have been given by Bessel and by others. We may therefore consider ourselves to be in possession of its numerical values under all circumstances, though we should not be justified in concluding from thence that their explicit *general symbolical* forms are either discoverable or that they are of such a nature as to be expressible by the existing language and signs of algebra.

The equation

$$\Gamma(r) = (r-1)(r-2) \ldots (r-m)\,\Gamma(r-m),$$

or

$$\Gamma(r-m) = \frac{\Gamma(r)}{(r-1)(r-2)\ldots(r-m)},$$

where m is a whole number, will explain the mode of passing from the fundamental transcendents, when included between $r=0$ and 1, or between $r=1$ and 2, to all the other derived transcendents of their respective classes †. The most simple of such classes of transcendents, are those which correspond to

$$\Gamma\left(\frac{1}{2}\right) = \sqrt{\pi},$$

which alone require for their determination the aid of no higher transcendents than circular arcs and logarithms. In all cases, also, if we consider $\Gamma(r)$ as expressing the *arithmetical* value of the corresponding transcendent, its general form would require the introduction of the factor 1^r, considered as the recipient of the multiple signs of affection which are proper for each differential coefficient, if we use that term in its most general sense.

In the note, p. 211, we have noticed the principal properties of these fractional and general differential coefficients, partly for the purpose of establishing upon general principles the basis of a new and very interesting branch of analysis ‡, and

* *Fonctions Elliptiques*, tom. ii. p. 490.

† Thus, $\Gamma\left(\frac{1}{2}\right) = \sqrt{\pi}$, $\Gamma\left(\frac{3}{2}\right) = \frac{1}{2}\sqrt{\pi}$, $\Gamma\left(\frac{5}{2}\right) = \frac{3 \cdot 1}{2^2}\sqrt{\pi}$, $\Gamma\left(\frac{-1}{2}\right) = \frac{-2}{1}\sqrt{\pi}$, $\Gamma\left(\frac{-3}{2}\right) = \frac{2^2}{1 \cdot 3}\sqrt{\pi}$, $\Gamma\left(\frac{-5}{2}\right) = \frac{-2^3}{1 \cdot 3 \cdot 5} \times \sqrt{\pi}$, &c.

‡ The consideration of fractional and general indices of differentiation was first suggested by Leibnitz, in many passages of his *Commercium Epistolicum* with John Bernouilli, and elsewhere; but the first definite notice of their theory was given by Euler in the *Petersburgh Commentaries* for 1731: they have also been considered by Laplace and other writers, and particularly by Fourier, in his great work, *La Théorie de la Propagation de la Chaleur*. The last of these illustrious authors has considered the general differential coeffi-

partly for the purpose of illustrating the principle of the permanence of equivalent forms in one of the most remarkable examples of its application. The investigations which we have given have been confined to the case of algebraical functions,

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cients of algebraical functions, through the medium of their conversion into transcendental functions by means of the very remarkable formula,

$$\phi x = \frac{2}{\pi} \int_{-\infty}^{+\infty} \phi(\alpha)\, d\alpha \int_{-\infty}^{+\infty} \phi(\alpha)\, dq \cos q(x-\alpha),$$

which immediately gives us,

$$\frac{d^r \phi x}{d x^r} = \frac{2}{\pi} \int_{-\infty}^{+\infty} \phi(\alpha)\, d\alpha \int_{-\infty}^{+\infty} \phi(\alpha)\, dq \frac{d^r}{d x^r} \cos q(x-\alpha);$$

which can be determined, therefore, if $\frac{d^r}{d x^r} \cos q(x-\alpha)$ can be determined, and the requisite definite integrations effected. If, indeed, we grant the practicability of such a conversion of $\phi(x)$ in all cases, and if we suppose the difficulties attending the consideration of the resulting series, which arise from the peculiar signs, whether of discontinuity or otherwise, which they may implicitly involve, to be removed, then we shall experience no embarrassment or difficulty whatever in the transition from integral to general indices of differentiation.

In the thirteenth volume of the *Journal de l'Ecole Polytechnique* for 1832, there are three memoirs by M. Joseph Liouville, all relating to general indices of differentiation, and one of them expressly devoted to the discussion of their algebraical theory. The author defines the differential coefficient of the order μ of the exponential function e^{mx} to be $m^\mu e^{mx}$, and consequently the μth differential coefficient of a series of such functions denoted by $\Sigma A_m e^{mx}$ must be represented by $\Sigma A_m m^\mu e^{mx}$. If it be granted that we can properly define a general differential coefficient, antecedently to the exposition of any general principles upon which its existence depends, then such a definition ought to coincide with the necessary conclusions deduced by those principles in their ordinary applications: but the question will at once present itself, whether such a definition is *dependent* or *not* upon the definition of the simple differential coefficient in this and in all other cases. In the first case it will be a proposition, and not a definition, merely requiring the aid of the principle of the permanence of equivalent forms for the purpose of giving at least an hypothetical existence to $\frac{d^\mu e^{mx}}{d x^\mu}$ for general, as well as for integral values of μ. M. Liouville then supposes that all rational functions of x are expressible by means of series of exponentials, and that they are consequently reducible to the form $\Sigma A_m e^{mx}$, and are thus brought under the operation of his definition. Thus, if x be positive, we have,

$$\frac{1}{x} = \int_0^\infty e^{-\alpha x}\, d\alpha,$$

and therefore,

$$\frac{d^\mu \frac{1}{x}}{d x^\mu} = \int_0^\infty e^{-\alpha x} (-\alpha)^\mu\, d\alpha,$$

and have been chiefly directed to meet the difficulties connected with the estimation of the values of the coefficient of differentiation in the case of fractional and general indices. If we should extend those investigations to certain classes of tran-

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which is easily reducible to the form,

$$\frac{d^{\mu}\frac{1}{x}}{d\,x^{\mu}} = \frac{(-1)^{\mu}\,\Gamma\,(1+\mu)}{x^{1+\mu}},$$

an expression which we have analysed in the note on p. 211. This part of M. Liouville's theory is evidently more or less included in M. Fourier's views, which we have noticed above. The difficulties which attend the complete developement of the formula $\frac{(-1)^{\mu}\,\Gamma\,(1+\mu)}{x^{1+\mu}}$ for all values of μ, which the principle of equivalent forms alone can reconcile, will best show how little progress has been made when the μth differential coefficient of $\frac{1}{x}$ is reduced to such a form.

M. Liouville adopts an opinion, which has been unfortunately sanctioned by the authority of the great names of Poisson and Cauchy, that diverging series should be banished altogether from analysis, as generally leading to false results; and he is consequently compelled to modify his formulæ with reference to those values of the symbols involved, upon which the divergency or convergency of the series resulting from his operations depend. In one sense, as we shall hereafter endeavour to show, such a practice may be justified; but if we adopt the principle of the permanence of equivalent forms, we may safely conclude that the limitations of the formulæ will be sufficiently expressed by means of those critical values which will at once suggest and require examination. The extreme multiplication of cases, which so remarkably characterizes M. Liouville's researches, and many of the errors which he has committed, may be principally attributed to his neglect of this important principle.

It is easily shown, if β be an indefinitely small quantity, that

$$x = \frac{e^{\beta x} - e^{-\beta x}}{2\beta}, \text{ or } \frac{e^{m\beta x} - e^{-n\beta x}}{(m+n)\,\beta},$$

and that consequently any integral function $A_0 + A_1\,x + \ldots A_p\,x^p$, involving integral and positive powers of x only, may be expressed by $\Sigma\,A_m\,\epsilon^{mx}$, where m is indefinitely small; and conversely, also, $\Sigma\,A_m\,e^{mx}$ may, under the same circumstances, be always expressed by a similar integral function of x. M. Liouville, by assuming a particular form,

$$C\,\sqrt{\beta}\,\frac{e^{\beta x} - e^{-\beta x}}{2\,\beta},$$

where C is arbitrary, and β indefinitely small, to represent *zero*, and differentiating, according to his definition, gets

$$\frac{d^{\frac{1}{2}}\,0}{d\,x^{\frac{1}{2}}} = C\,\sqrt{\beta}\,\frac{e^{\beta x}\,\sqrt{-1}\,e^{-\beta x}}{2\,\sqrt{\beta}} = C\,\frac{(1-\sqrt{-1})}{2};$$

but it is evident that by altering the form of this expression for zero we might show that $\frac{d^{\frac{1}{2}}\,0}{d\,x^{\frac{1}{2}}}$ was equal either to *zero* or to *infinity*; and that in the latter scendental functions, such as e^{mx}, $\sin m x$, and $\cos m x$, we shall encounter no such difficulties, in as much as the differentials of those functions corresponding to indices which are general in form, though denoting integral numbers, are in a form

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case the *critical* value *infinity* might be merely the indication of the existence of *negative* or *fractional* powers of x in the expression for $\frac{d^{\frac{1}{2}} 0}{d x^{\frac{1}{2}}}$, which were not expressible by any rational function of $e^{\beta x}$ under a finite form and involving indefinitely small indices only. And such, in fact, would be the result of any attempt to differentiate this exponential expression for x or its powers, with respect to fractional or negative indices. It has resulted from this very rash generalization of M. Liouville that he has assigned as the general form of complementary arbitrary functions,

$$C + C_1 x + C_2 x^2 + C_3 x^3 + \&c.,$$

which is only true when the index of differentiation is a negative whole number.

Most of the rules which M. Liouville has given for the differentiation of algebraical functions are erroneous, partly in consequence of his fundamental error in the theory of complementary arbitrary functions, and partly in consequence of his imperfect knowledge of the constitution of the formula $\frac{\Gamma(1+n)}{\Gamma(1+n-r)}$: thus, after deducing the formula

$$\frac{d^r \cdot \frac{1}{(a x + b)^n}}{d x^r} = \frac{(-1)^r \cdot a^r \cdot}{1 \cdot 2 \ldots (n-1)} \frac{\Gamma(n+r)}{(a x + b)^{n+r}},$$

which is only true when n is a whole number, he says that no difficulty presents itself in its treatment, whilst $n + r$ is > 0, but that $\Gamma(n+r)$ becomes infinite, when $n + r < 0$, in which case he says that it must be transformed into an expression containing finite quantities only, by the aid of complementary functions; whilst, in reality, $\Gamma(n+r)$ is only infinite when $n + r$ is *zero* or a *negative* whole number, and the forms of the complementary functions, such as he has assigned to them, are not competent to effect the conversion required. In consequence of this and other mistakes, in connexion with the important case $\frac{d^r \cdot \frac{1}{(a x + b)^n}}{d x^r}$, nearly all his conclusions with respect to the general differentials of rational functions, by means of their resolution into partial fractions, are nearly or altogether erroneous.

The general differential coefficients of sines and cosines follow immediately from those of exponentials, and present few difficulties upon any view of their theory. In looking over, however, M. Liouville's researches upon this subject, I observe one remarkable example of the abuse of the first principles of reasoning in algebra. There are two values of $\frac{d^{\frac{1}{2}} \cos m x}{d x^{\frac{1}{2}}}$, one positive and the other negative, considered apart from the sign of m, whether positive or negative: but if we put $\cos m x = \frac{1}{4} \cos m x + \frac{3}{4} \cos m x$, we get

$$\frac{d^{\frac{1}{2}} \cos m x}{d x^{\frac{1}{2}}} = \frac{1}{4} \frac{d^{\frac{1}{2}} \cos m x}{d x^{\frac{1}{2}}} + \frac{3}{4} \cdot \frac{d^{\frac{1}{2}} \cos m x}{d x^{\frac{1}{2}}};$$

which is adapted to the immediate application of the general principle in question.

Thus, if $u = e^{mx}$, we get

$$\frac{d u}{d x} = m e^{mx}, \frac{d^2 u}{d x^2} = m^2 e^{mx}, \ldots \frac{d^r u}{d x^r} = m^r e^{mx},$$

when r is a whole number, and therefore, also, when r is any quantity whatsoever.

If $u = \sin m x$, $\frac{d u}{d x} = m \sin\left(\frac{\pi}{2} + m x\right)$, $\frac{d^2 u}{d x^2} = m^2 \sin (\pi + m x)$, ... $\frac{d^r u}{d x^r} = m^r \sin\left(\frac{r\pi}{2} + m x\right)$ when r is a whole number, and therefore generally. In a similar manner if $u = \cos m x$, or rather $u = \cos m (1)^{\frac{1}{2}} x$, (introducing $1^{\frac{1}{2}}$ as a factor in order to express the double sign of $m x$, if determined from the value of its cosine,) then we shall find $\frac{d^r u}{d x} = (m \sqrt{} 1)^r \cos \left\{\frac{r\pi}{2} + (m \sqrt{} 1) x\right\}$, whatever be the value of r. If $u = e^{nx} \cos m x$, we get, by very obvious reductions, making $\rho = \frac{n}{\sqrt{n^2 + m^2}}$ and $\theta = \cos^{-1} \frac{n}{\rho}$,

$$\frac{d^r u}{d x^r} = \rho^r e^{nx} \cos (m x + n \theta).$$

It is not necessary to mention the process to be followed in ob-

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and if we combine *arbitrarily* the double values of the two parts of the second member of this equation, we shall get four values of $\frac{d^{\frac{1}{2}} \cos m x}{d x^{\frac{1}{2}}}$, instead of two; and, in a similar manner, if we should resolve $\cos m x$ into any number of parts, we should get double the number of values of $\frac{d^{\frac{1}{2}} \cos m x}{d^{\frac{1}{2}}}$. If this principle of arbitrary combinations of algebraical values *derived from a common operation* was admitted, we must consider $\frac{\sqrt{x}}{1 - x}$ as having two values, and its equivalent series

$$x^{\frac{1}{2}} + x^{\frac{3}{2}} + x^{\frac{5}{2}} + \text{\&c.}$$

as having an infinite number. But it is quite obvious that those expressions which involve implicitly or explicitly a multiple sign must continue to be estimated with respect to the same value of this sign, however often the *recipient* of the multiple sign may be repeated in any *derived* series or expression. The case is different in those cases where the several terms exist independently of any explicit or implied process of derivation.

taining the general differential coefficients of other expressions, such as $(\cos x)^n$, $\cos m x \times \cos n x$, &c., which present no kind of difficulty. In all such cases the complementary arbitrary functions will be supplied precisely in the same manner as for the corresponding differential coefficients of algebraical functions.

The transition from the consideration of integral to that of fractional and general indices of differentiation is somewhat startling when first presented to our view, in consequence of our losing sight altogether of the principles which have been employed in the derivation of differential coefficients whose indices are whole numbers: but a similar difficulty will attend the transition, in every case, from arithmetical to general values of symbols, through the medium of the principle of the permanence of equivalent forms, though habit and in some cases imperfect views of its theory, may have made it familiar to the mind. We can form distinct conceptions of $m \, . \, m$, $m \, . \, m \, . \, m$, $m \, . \, m \, . \, m \ldots . \, (r)$, where m is a whole number repeated twice, thrice, or r times, when r is also a whole number; and we can readily pass from such expressions to their defined or assumed equivalents $m^2, m^3, \ldots m^r$: in a similar manner we can readily pass from the *factorials** $1 \, . \, 2$, $1 \, . \, 2 \, . \, 3$, $\ldots$ $1 \, . \, 2 \ldots r$, to their assumed equivalents $\Gamma(3), \Gamma(4), \ldots \Gamma(1+r)$, as long as r is a whole number. The transition from m^r and $\Gamma(1+r)$ when r is a whole number, to m^r and $\Gamma(1+r)$ when r is a general symbol, is made by the principle of the equivalent forms; but by no effort of mind can we conneet the first conclusion in each case with the last, without the aid of the intermediate formula, involving symbols which are general in form though specific in value; and in no instance can we interpret the ultimate form, for values of the symbols which are not included in the first, by the aid of the definitions or assumptions which are employed in the establishment of the primary form. In all such cases the interpretation of the ultimate form, when such an interpretation is discoverable, must be governed and determined by a reference to those general properties of it which are independent of the specific values of the symbols.

* Legendre has named the function $\Gamma(1+r) = 1 \, . \, 2 \ldots r$, the function *gamma*. Kramp, who has written largely upon its properties, gave it, in his *Analyse des Refractions Astronomiques*, the name of *faculté numérique*; but in his subsequent memoirs upon it in the earlier volumes of the *Annales des Mathématiques* of Gergonne he has adopted the name of *factorial* function, which Arbagost proposed, and which I think it expedient to retain, as recalling to mind the continued product which suggests this creature of algebraical language.

The law of derivation of the terms in Taylor's series,

$$u' = u + \frac{d\,u}{d\,x} \cdot h + \frac{d^2 u}{d x^2} \cdot \frac{h^2}{1 . 2} + \frac{d^3 u}{d x^3} \cdot \frac{h^3}{1 . 2 . 3} + \&c.,$$

is the same as in the more general series

$$\frac{d^r\,u'}{d\,x^r} = \frac{d^r\,u}{d\,x^r} + \frac{d^{r+1}\,u}{d\,x^{r+1}} \cdot \frac{h}{1} + \frac{d^{r+2}\,u}{d\,x^{r+2}} \cdot \frac{h^2}{1 . 2} + \&c.;$$

and if we possess the law of derivation of $\frac{d\,u}{d\,x}$ and of $\frac{d^r\,u}{d\,x^r}$, we can find all the terms of both these series, whatever be the value of r. The first of these terms must be determined through the ordinary definitions of the differential calculus; the second must be determined in form by the same principles, and generalized through the medium of the principle of equivalent forms. Both these processes are indispensably necessary for the determination of $\frac{d^r\,u}{d\,x^r}$: but it is the second of them which altogether separates the interpretation of $\frac{d^r\,u}{d\,x^r}$ from that of $\frac{d\,u}{d\,x}$, or rather of $\frac{d^r\,u}{d\,x^r}$ when r is a whole number, unless in the particular cases in which the symbols in both are identical in value.

There are two distinct processes in algebra, the direct and the inverse, presenting generally very different degrees of difficulty. In the first case, we proceed from defined operations, and by various processes of demonstrative reasoning we arrive at results which are general in form though particular in value, and which are subsequently generalized in value likewise: in the second, we commence from the general result, and we are either required to discover from its form and composition some equivalent result, or, if defined operations have produced it, to discover the primitive quantity from which those operations have commenced. Of all these processes we have already given examples, and nearly the whole business of analysis will consist in their discussion and developement, under the infinitely varied forms in which they will present themselves.

The disappearance of *undulating* and of determinate functions with *arbitrary constants*, upon the introduction of integral or other specific values of certain symbols involved, is one of the chief sources* of error in effecting transitions to equiva-

* The theory of discontinuous functions and of the signs of discontinuity will show many others.

lent forms, whether the process followed be direct or inverse. Many examples of the first kind may be found in the researches of Poinsot respecting certain trigonometrical series, which will be noticed hereafter, and which had been hastily generalized by Euler and Lagrange; and a remarkable example of the latter has already been pointed out, in the disappearance of the functions with arbitrary constants in the transition from u to $\frac{d^r u}{dx^r}$, when r becomes a whole positive number. The general discussion of such cases, however, would lead me to an examination of the theory of the introduction of determinate and arbitrary functions in the most difficult processes of the integral calculus and of the calculus of functions, which would carry me far beyond the proper limits and object of this Report. I have merely thought it necessary to notice them in this place for the purpose of showing the extreme caution which must be used in the generalization of equivalent results by means of the application of the principle of the permanence of equivalent forms*.

The preceding view of the principles of algebra would not only make the use and form of *derivative* signs, of whatever kind they may be, to be the necessary results of the same general principle, but would also show that the interpretation of their meaning would not precede but follow the examination of the circumstances attending their introduction. I consider it to be extremely important to attend to this order of succession between results and their interpretation, when those results belong to symbolical and not to arithmetical algebra, in as much as the neglect of it has been the occasion of much of the confusion and inconsistency which prevail in the various theories which have been given of algebraical signs. I speak of *deri-*

* Euler, in the *Petersburgh Acts* for 1774, has denied the universality of this principle, and has adduced as an example of its failure the very remarkable series

$$\frac{1-a^m}{1-a} + \frac{(1-a^m)(1-a^{m-1})}{1-a^2} + \frac{(1-a^m)(1-a^{m-1})(1-a^{m-2})}{1-a^3} + \&c.,$$

which is equal to m, when m is a whole number, but which is apparently not equal to m, for other values of m, unless at the same time $a = 1$: the occurrence, however, of *zero* as a factor of the $(m+1)^{\text{th}}$ and following terms in the first case, and the reduction of every term to the form $\frac{0}{0}$ in the second, would form the proper indications of a change in the constitution of the equivalent function corresponding to these values of m and a, of which many examples will be given in the text.

vative signs as distinguished from those primitive signs of operation which are used in arithmetical algebra; but such signs, though accurately defined and limited in their use in one science, will cease to be so in the other, their meaning being dependent in symbolical algebra, in common with all other signs which are used in it, upon the symbolical conditions which they are required to satisfy.

I will consider, in the first place, *signs of affection*, which are those symbolical quantities which do not affect the magnitudes, though they do affect the specific nature, of the quantities into which they are incorporated.

Of this kind are the signs $+$ and $-$, when used independently; or their equivalents $+1$ and -1, when considered as symbolical factors; the signs $(+1)^n$ and $(-1)^n$, or their symbolical equivalents

$$\cos 2rn\pi + \sqrt{-1}\sin 2rn\pi \text{ and } \cos(2r+1)n\pi + \sqrt{-1}\sin(2r+1)n\pi;$$

$$\text{or } e^{2rn\pi\sqrt{-1}} \text{ and } e^{(2r+1)n\pi\sqrt{-1}}.$$

The affections symbolized by the signs $+1$ and -1 admit of very general interpretation consistently with the symbolical conditions which they are required to satisfy, and particularly so in geometry: and it has been usual, in consequence of the great facility of such interpretations, to consider all quantities affected by them (which are not abstract) as *possible*, that is, as quantities possessing in all cases relations of existence which are expressible by those signs. It should be kept in mind, however, that such interpretations are in no respect distinguished from those of other algebraical signs, except in the extent and clearness with which their conditions are symbolized in the nature of things.

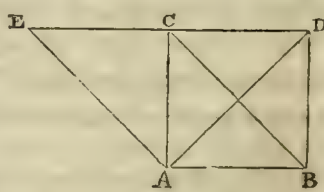
The other signs of affection, different from $+1$ and -1, which are included in $(1)^n$ and $(-1)^n$, are expressible generally by $\cos\theta + \sqrt{-1}\sin\theta$, or by $\alpha + \beta\sqrt{-1}$, where α and β may have any values between 1 and -1, zero included, and where $\alpha^2 + \beta^2 = 1$. To all quantities, whether abstract or concrete, expressed by symbols affected by such signs, the common term *impossible* has been applied, in contradistinction to those *possible* magnitudes which are affected by the signs $+$ and $-$ only.

If, indeed, the affections symbolized by the signs included under the form $\cos\theta + \sqrt{-1}\sin\theta$, admitted in no case of an interpretation which was consistent with their symbolical conditions, then the term *impossible* would be correctly applied to quantities affected by them: but in as much as the signs $+$ and

$-$, when used independently, and the sign $\cos\theta + \sqrt{-1}\sin\theta$, when taken in its most enlarged sense, *equally* originate in the generalization of the operations of algebra, and are *equally* independent of any previous definitions of the meaning and extent of such operations, they are also equally the object of interpretation, and are in this respect no otherwise distinguished from each other than by the greater or less facility with which it can be applied to them.

Many examples* of their consistent interpretation may be pointed out in geometry as well as in other sciences: thus, if $+a$ and $-a$ denote two equal lines whose directions are *opposite* to each other, then $(\cos\theta + \sqrt{-1}\sin\theta)\,a$ may denote an equal line, making an angle θ with the line denoted by $+a$; and consequently $a\sqrt{-1}$ will denote a line which is perpendicular to $+a$. This interpretation admits of very extensive application, and is the foundation of many important consequences in the application of algebra to geometry.

The signs of *operation* $+$ and $-$ may be immediately interpreted by the terms *addition* and *subtraction*, when applied to unaffected symbols denoting magnitudes of the same kind: if they are applied to symbols affected with the sign $-$, these signs, and the terms used to interpret them, become convertible. Thus $a + (-b) = a - b$, and $a - (-b) = a + b$; or the algebraical *sum* and *difference* of a and $-b$, is equivalent to the algebraical *difference* and *sum* of a and b: but if they are applied to lines denoted by symbols affected by the signs $\cos\theta + \sqrt{-1}.\sin\theta$, and $\cos\theta' + \sqrt{-1}\sin\theta'$, the results will no longer denote the arithmetical (or geometrical) *sum* and *difference* of the lines in question, but the *magnitude* and *position* of the diagonals of the parallelogram constructed upon them, or upon lines which are equal and parallel to them. Thus, if we denote the line A B by a, and the line A C at right angles to it by $b\sqrt{-1}$, and if we complete the parallelograms ABDC and ABCE, then $a + b\sqrt{-1}$ will denote the diagonal A D, and $a - b\sqrt{-1}$ will denote the other diagonal B C, or the equal and parallel line A E.



It is easily shown that $a + b\sqrt{-1} = \sqrt{(a^2 + b^2)}\,(\cos\theta + \sqrt{-1}\sin\theta)$, (where $\theta = \cos^{-1}\dfrac{a}{\sqrt{a^2+b^2}}$), and $a - b\sqrt{-1} = \sqrt{(a^2 + b^2)}\,\{\cos\theta - \sqrt{-1}\sin\theta\}$; it follows, therefore, that

* Peacock's *Algebra*, chap. xii. Art. 437, 447, 448, 449.

$a + b\sqrt{-1}$ and $a - b\sqrt{-1}$ may be considered as representing respectively a single line, equal in magnitude to $\sqrt{(a^2 + b^2)}$*, and affected by the sign $\cos\theta + \sqrt{-1}\sin\theta$ in one case, and by the sign $\cos\theta - \sqrt{-1}\sin\theta$ in the other; or as denoting the same lines through the medium of the operations denoted in the one case by $+$, and in the other by $-$, upon the two lines at right angles to each other, which are denoted by a and $b\sqrt{-1}$.

We have spoken of the signs of operation $+$ and $-$, as distinguished from the same signs when used as signs of affection, and we have also denominated $a + b\sqrt{-1}$, and $a - b\sqrt{-1}$, the *sum* and *difference* of a and $b\sqrt{-1}$, though they can no longer be considered to be so in the arithmetical or geometrical sense of those terms; but it is convenient to explain the meaning of the same *sign* by the same *term*, though they may be used in a sense which is not only very remote from, but even totally opposed to†, their primitive signification; and such a licence in the use both of signs and of phrases is a necessary consequence of making their interpretation dependent, not upon previous and rigorous definitions as is the case in arithmetical algebra, but upon a combined consideration of their symbolical conditions, and the specific nature of the quantities represented by the symbols. It is this necessity of considering all the results of symbolical algebra as admitting of interpretation *subsequently* to their formation, and not in consequence of any previous definitions, which places all those results in the same relation to the whole, as being equally the creations of the same general principle: and it is this circumstance which jus-

* The arithmetical quantity $\sqrt{(a^2 + b^2)}$ has been called the *modulus* of $a \pm b\sqrt{-1}$ by Cauchy, in his *Cours d'Analyse*, and elsewhere. It is the single *unaffected* magnitude which is included in the *affected* magnitude $a \pm b\sqrt{-1}$: conversely the affected magnitude $(\cos\theta \pm \sqrt{-1}\sin\theta)\sqrt{a^2 + b^2}$ is reducible to the equivalent quantity $a \pm b\sqrt{-1}$, if $\cos\theta = \frac{a}{\sqrt{a^2 + b^2}}$, and therefore $\sin\theta = \frac{b}{\sqrt{a^2 + b^2}}$.

† The *sum* of a and $-b$, or $a + (-b)$, is identical with the *difference* of a and b, or with $a - b$. The term *operation*, also, which is applied *generally* to the fact of the transition from the component members of an expression to the final symbolical result, will only admit of interpretation when the nature of the process which it designates can be described and conceived. In all other cases we must regard the final result alone. Thus, if a and b denote lines, we can readily conceive the process by which we form the results $a + b$ and $a - b$, at least when a is greater than b. But when we interpret $a + b\sqrt{-1}$ to mean a determinate single line with a determinate position, we are incapable of conceiving any process or operation through the medium of which it is obtained.

tifies the assertion, which we have made above, that quantities or their symbols affected by the signs $+$, $-$, or $\cos\theta + \sqrt{-1}.\sin\theta$, are only distinguished from each other by the greater or less facility of their interpretation.

The geometrical interpretation of the sign $\sqrt{-1}$, when applied to symbols denoting lines, though more than once suggested by other authors, was first formally maintained by M. Buée in a paper in the *Philosophical Transactions* for 1806*, which contains many original, though very imperfectly developed views upon the meaning and application of algebraical signs. In the course of the same year a small pamphlet was published at Paris by M. Argand, entitled *Essai sur une Manière de représenter les Quantités Imaginaires, dans les Constructions Géométriques,* written apparently without any knowledge of M. Buée's paper. In this memoir M. Argand arrives at this proposition, That the algebraical sum † of two lines ‡, estimated both according to magnitude and direction, would be the diagonal of the parallelogram which might be constructed upon them, considered both with respect to direction and magnitude, which is, in fact, the capital conclusion of this theory. This memoir of M. Argand seems, however, to have excited very little attention; and his views, which were chiefly founded upon analogy, were too little connected with, or rather dependent upon, the great fundamental principles of algebra, to entitle his conclusions to be received at once into the great class of admitted or demonstrated truths. It would appear that M. Argand had consulted Legendre upon the subject of his memoir, and that a favourable mention of its contents was made by that great analyst in a letter which he wrote to the brother of M. J. F. Français, a mathematician of no inconsiderable eminence. It was the inspection of this letter, upon the death of his brother, which induced M. Français to consider this subject, and he published, in the fourth volume of Gergonne's *Annales des Mathématiques* for 1813, a very curious memoir upon it, containing views more extensive, and more completely developed than those of M. Argand, though generally agreeing with them in their character, and in the conclusions deduced from them. This publication led to a second memoir upon the same theory from M. Argand, and to several observations upon it, in the same Journal, from MM. Servois, Français, and Gergonne, in which some of the most prominent objections to it were proposed, and partly, though very imperfectly, an-

* This paper was read in 1805.

† *La somme dirigée.*

‡ *Lignes dirigées.*

swered. No further notice appears to have been taken of these researches before the year 1828, when Mr. Warren's treatise on the geometrical representation of the square roots of negative quantities* was published. In this work Mr. Warren proposes to give a geometrical representation to every species of quantity; and after premising definitions of addition, subtraction, multiplication and division, involution and evolution, which are conformable to the more enlarged sense which interpretation would assign to those operations when applied to lines represented in position as well as in magnitude; and after showing in great detail the coincidence of the symbolical results obtained from such definitions with the ordinary results of arithmetical and symbolical algebra, he proceeds to *determine* the meaning of the different symbolical roots of 1 and -1, when applied to symbols denoting lines, under almost every possible circumstance. The course which Mr. Warren has followed leads almost necessarily to very embarrassing details, and perhaps, also, to the neglect of such comprehensive propositions as can only derive their authority from principles which make all the results of algebra which are general in form independent of the specific values and representation of the symbols: but at the same time it must be allowed that his conclusions, when viewed in connexion with his definitions, were demonstrably true; a character which could not be given to similar conclusions when they were attempted to be derived by the mere aid of the arithmetical definitions of the fundamental operations of algebra.

This objection to the course pursued by Mr. Warren will more or less apply to all attempts which are made to make the previous interpretations of algebra govern the symbolical conclusions; for though it is always possible to assign a meaning to algebraical operations, and to pursue the consequences of that meaning to their necessary conclusions, yet if the laws of combination which lead to such conclusions are expressed through the medium of general signs and symbols, they will cease, when once formed, to convey the necessary limitations of meaning which the definitions impose upon them. It is for this reason that we must in all cases consider the laws of combination of general symbols as being arbitrary and independent in whatever manner suggested, and that we must make our interpretations of the results obtained conformable to those laws, and not the laws to the interpretations: it is for the same reason, likewise, that our interpretations will not be necessary, though

* *A Treatise on the Geometrical Representation of the Square Roots of Negative Quantities*, by the Rev. John Warren, M.A., Fellow and Tutor of Jesus College Cambridge. 1828.

governed by necessary laws, except so far as those interpretations are dependent upon each other. Thus, if a be taken to represent a line in *magnitude*, it is *not* necessary that $(\cos\theta + \sqrt{-1}\sin\theta)a$ should represent a line equal in length to the one represented by a, and also making an angle θ with the line represented by a; but if $(\cos\theta + \sqrt{-1}\sin\theta)\,a$, *may*, consistently with the symbolical conditions, represent such a line, without any restriction in the value of θ, then, if it *does* represent such a line for one value of θ, it *must* represent such a line for every value of θ included in the formula. It is only in such a sense that interpretations can be said in any case to have a necessary and inevitable existence.

It is this confusion of necessary and contingent truth which has occasioned much of the difficulty which has attended the theories of the interpretation of algebraical signs. It has been supposed that a meaning could be transmitted through a succession of merely symbolical operations, and that there would exist at the conclusion an equally necessary connexion between the primitive definition and the ultimate interpretation, as between the final symbolical result and the laws which govern it. So long as the definitions both of the meaning of the symbols and of the operations to which they are required to be subject are sufficient to deduce the results, those results will have a necessary interpretation which will be dependent upon a joint consideration of all those conditions; but whenever an operation is required to be performed under circumstances which do not allow it to be strictly defined or interpreted, the chain of connexion is broken, and the interpretation of the result will be no longer traceable through its successive steps. This must take place whenever negative or other affected quantities are introduced, and whenever operations are to be performed, either with them, or upon them, even though such quantities and signs should altogether disappear from the final result.

This principle of interpretation being once established, we must equally consider -1, $\sqrt{-1}$, $\cos\theta + \sqrt{-1}\sin\theta$, as signs of *impossibility*, in those cases in which no consistent meaning can be assigned to the quantities which are affected by them, and in those cases only: and it must be kept in mind that the *impossibility* which may or may not be thus indicated, has reference to the interpretation only, and not to the symbolical result, considered as an equivalent form: for all symbolical results must be considered as equally possible which the signs and symbols of algebra, whether admitting of interpretation or not, are competent to express. But there will be found to be many species of impossibility which will present themselves in considering the relations of formulæ with a view to their equivalence, and also under other circumstances, which will be indicated by such means as will destroy all traces of the equivalence which would otherwise exist.

The capacity, therefore, possessed by the signs of affection involving $\sqrt{-1}$ of admitting geometrical or other interpretations under certain circumstances, though it adds greatly to our power of bringing geometry and other sciences under the dominion of algebra, does not in any respect affect the general theory of their introduction or of their relation to other signs: for, in the first place, it is not an essential or necessary property of such signs; and in the second place, it in no respect affects the form or equivalence of symbolical results, though it does affect both the extent and mode of their application. It would be a serious mistake, therefore, to suppose that such incidental properties of quantities affected by such signs constituted their real essence, though such a mistake has been generally made by those who have proposed this theory of interpretation, and has been made the foundation of a charge against them by others, who have criticised and disputed its correctness*.

* This charge is made by Mr. Davies Gilbert in a very ingenious paper in the *Philosophical Transactions* for 1831, "On the Nature of Negative and Impossible Quantities." He says that those mathematicians take an incorrect view of ideal quantities,—mistaking, in fact, incidental properties for those which constitute their real essence,—who suppose them to be principles of perpendicularity, because they may in some cases indicate extension at right angles to the directions indicated by the correlative signs + and —; for with an equal degree of propriety might the actually existing square root of a quantity be taken as the principle of obliquity, in as much as in certain cases it indicates the hypothenuse of a right-angled triangle. In reply to this last observation, it may be observed, that I am not aware that in any case the sign $\sqrt{-1}$ has had such an interpretation given to it.

It is quite impossible for me to give an abridged, and at the same time a fair view of Mr. Davies Gilbert's theory, within a compass much smaller than the contents of his memoir. But I might venture to say that his proof of the rule of signs rests upon some properties of ratios or proportions which no arithmetical or geometrical view of their theory would enable us to deduce. In considering, also, imaginary quantities as *creations of an arbitrary definition, endowed with properties at the pleasure of him who defines them,* he ascribes to them the same character as to all other symbols and operations of algebra; but in saying "that quantities affected by the sign $\sqrt{-1}$ possess a *potential existence* only, but that they are ready to start into energy whenever that sign is removed," he appears to me to assert nothing more than that symbols are impossible or not, according as they are affected by the sign $\sqrt{-1}$ or not. Again, in examining the relation of the terms of the equation

$$x + \sqrt{x^2 - 1} = (y + \sqrt{y^2 - 1})^n = y^n + n\,y^{n-1}\sqrt{y^2 - 1}$$
$$+ \frac{n\,(n-1)}{1\,.\,2}\,y^{n-2}\,(y^2 - 1) + \frac{n\,(n-1)\,(n-2)}{1\,.\,2\,.\,3}\,y^{n-3}\,(y^2 - 1)^{\frac{3}{2}} + \text{ \&c.},$$

Signs of transition are those signs which indicate a change in the nature or form of a function, when considered in the whole course of its passage through its different states of existence. Such *signs*, if they may be so designated, are generally *zero* and *infinity*.

Zero and *infinity* are negative terms, and if applied to desig-

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he denies the correctness of the reasoning by which it is inferred that the second term of the first, and the even terms of the second members of this equation are equal to one another (when x is less than 1), because they are the only terms which are homogeneous to each other, in as much as we thus ascribe real properties to ideal quantities; and he endeavours to make this equality depend upon an assumed arbitrary relation between x and y, though it is obvious that if $y = \cos \theta$, we shall find $x = \cos n \theta$, and that, therefore, this relation is determinate, and not arbitrary. A little further examination of this conclusion would show that it did not depend upon any assumed homogeneity of the parts of the members of this equation to each other, but upon the double sign of the radical quantity which is involved upon both sides.

In arithmetical algebra, where no signs of affection are employed or recognised, both negative and imaginary quantities become the limits of operations; and when this science is modified by the introduction of the independent signs + and − and the rule for their incorporation, the occurrence of the square roots of negative quantities, by presenting an apparent violation of the rule of the signs, becomes a new limit to the application of this new form of the science. The same algebraists who have acquiesced in the propriety of making the first transition in consequence of the facility of assigning a meaning to negative quantities, at the same time that they retained the definitions and principles of the first science, were startled and embarrassed when they came to the second; for it was very clear that no attempt could be made to reconcile the existence and use of such quantities, consistently with the maintenance of that demonstrative character in our reasonings which exists in geometry and arithmetic, where the mind readily comprehends the nature of the quantities employed, and of the operations performed upon them. The proper conclusion in such a case would be that the operations performed, as well as the quantities employed, were symbolical, and that the results, though they might be suggested by the primitive definitions, were not dependent upon them. If no real conclusions had been obtained by the aid of such merely symbolical quantities, they would probably have continued to be regarded as algebraical monsters, whose reduction under the laws of a regular system was not merely unnecessary, but altogether impracticable. But it was soon found that many useful theories were dependent upon them; that any attempt to guard against their introduction in the course of the progress of our operations with symbols would not merely produce the most embarrassing limitations, when such limitations were discoverable, but that they would present themselves in the expression of real quantities, and would furnish at the same time the only means by which such quantities could be expressed. A memorable example of their occurrence under such circumstances presents itself in what has been called the *irreducible* case of cubic equations.

In the *Philosophical Transactions* for 1778 there is a paper by Mr. Playfair on the arithmetic of impossible quantities, in which the definable nature of algebraical operations is asserted in the most express terms, and in which the truth of conclusions deduced by the aid of imaginary symbols is made to depend upon the analogy which exists between certain geometrical properties nate states of quantity, are equally inconceivable. We are accustomed, however, to speak of quantities as *infinitely* great and *infinitely* small, as distinguished from *finite* quantities, whether great or small, and to represent them by the symbols ∞ and 0. It is this practice of designating such inconceivable states of quantity by symbols, which brings them, in some de-

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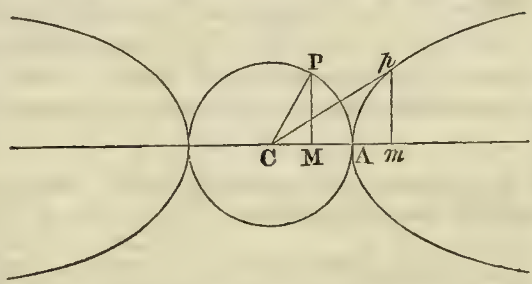
of the circle and the rectangular hyperbola. It is well known that the circle and rectangular hyperbola are included in the same equation $y = \sqrt{(1 - x^2)}$, if we suppose x to have any value between $+\infty$ and $-\infty$: let a circle be described with centre C and radius C A = 1, and upon the production of this radius, let a rectangular hyperbola be described whose semiaxis is 1, in a plane at right angles to that of the circle: if θ denote the angle A C P, then the circular cosine and sine (C M and P M) are expressed by



$$\frac{e^{\theta\sqrt{-1}} - e^{-\theta\sqrt{-1}}}{2} \text{ and } \frac{e^{\theta\sqrt{-1}} - e^{-\theta\sqrt{-1}}}{2\sqrt{-1}}$$

respectively; whilst the *hyperbolic cosine and sine* (to adopt the terms proposed by Lambert) corresponding to the angle $\theta\sqrt{-1}$ (in a plane at right angles to the former) are expressed by

$$\frac{e^{\theta} + e^{-\theta}}{2} \text{ and } \sqrt{-1}\left(\frac{e^{\theta} - e^{-\theta}}{2}\right), \text{ or by } \frac{e^{\theta} + e^{-\theta}}{2} \text{ and } \frac{e^{\theta} - e^{-\theta}}{2},$$

if they be considered as determined by the following conditions; namely, that (hyp. cosine)^2 — (hyp. sine)$^2 = 1$, and that hyp. cos θ = hyp. cos $-\theta$, and hyp. sine $\theta = -$ hyp. sine $-\theta$. A comparison of these processes in the circle and hyperbola would show, says Mr. Playfair, that investigations which are conducted by real symbols, and therefore by real operations, in the hyperbola, would present *analogous* imaginary symbols, and therefore *analogous* imaginary operations in the circle, and conversely; and that the same species of analogy which connects the geometrical properties of the circle and hyperbola, connects the conclusions, of the same symbolical forms, when conducted by real and imaginary symbols.

This attempt to convert an extremely limited into a very general analogy, and to make the conclusions of symbolical algebra dependent upon an insulated case of geometrical interpretation, would certainly not justify us in drawing any general conclusions from processes involving imaginary symbols, unless they could be confirmed by other considerations. The late Professor Woodhouse, who was a very acute and able scrutinizer of the logic of analysis, has criticised this principle of Mr. Playfair with just severity, in a paper in the *Philosophical Transactions* for 1802, "On the *necessary truth of certain conclusions obtained by means of imaginary expressions.*" The view which he has taken of algebraical equivalence, in cases where the connexion between the expressions which were treated as equivalent could not be shown to be the result of a defined operation, makes a very near approach to the principle gree, under the ordinary rules of algebra, and which compels us to consider different orders both of *infinities* and of *zeros*, though when they are considered without reference to their symbolical connexion, they are necessarily denoted by the same simple symbols ∞ and 0: thus there is a necessary symbolical distinction between $(\infty)^{\frac{1}{2}}$, ∞ and $(\infty)^{n}$, and between $(0)^{\frac{1}{2}}$, 0 and (0); though when considered absolutely as denoting *infinity* in one case and *zero* in the other, they are equally designated by the simple symbols ∞ and 0 respectively.

Though the fundamental properties of 0 and ∞, considered as the representatives of zero and infinity, are suggested by the ordinary interpretation of those terms, yet their complete interpretation, like that of other signs, must be founded upon the

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of the permanence of equivalent forms: thus, supposing, when x is a real quantity, we can show that

$$e^{x} = 1 + x + \frac{x^2}{1 \cdot 2} + \frac{x^3}{1 \cdot 2 \cdot 3} + \&c.;$$

but that we cannot show in a similar or any other manner that

$$e^{x\sqrt{-1}} = 1 + x\sqrt{-1} - \frac{x^2}{1 \cdot 2} - \frac{x^3\sqrt{-1}}{1 \cdot 2 \cdot 3} + \&c.,$$

then the equivalence in the latter case is *assumed*, by considering $e^{x\sqrt{-1}}$ as the abridged symbol for the series of terms

$$1 + x\sqrt{-1} - \frac{x^2}{1 \cdot 2} - \frac{x^3\sqrt{-1}}{1 \cdot 2 \cdot 3} + \&c.;$$

in other words, the form which is proved to be true for values of the symbols which are general in form, though particular in value, is assumed to be true in all other cases.

It is true that such a generalization could not be considered as legitimate, without much preparatory theory and without considerable modifications of our views respecting nearly all the fundamental operations and signs of arithmetical algebra; but I refer with pleasure to this incidental testimony to the truth and universality of this important law, from an author whose careful and bold examination of the first principles of analytical calculation entitle his opinion to the greatest consideration.

Mr. Gompertz published, in 1817 and 1818, two tracts on the *Principles and Application of Imaginary Quantities*, containing many ingenious and novel views both upon the correctness of the conclusions obtained by means of imaginary quantities and also upon their geometrical interpretation. The first of these tracts is principally devoted to the establishment of the following *position*: "That wherever the operation by imaginary expressions can be used, the propriety may be explained from the capability of one arbitrary quantity or more being introduced into the expressions which are imaginary previously to the said arbitrary quantity or quantities being introduced, so as to render them real, without altering the truth they are meant to express; and that, in consequence, the operation will proceed on real quantity, the introduced arbitrary quantity or quantities necessary to render the first steps of the reasoning arguments on real quantity, vanishing at the conclusion;

consideration of all the circumstances under which they present themselves in symbolical results. In order, therefore, to determine some of the principles upon which those interpretations must be made, it will be proper to examine some of the more remarkable of their symbolical properties.

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and from whence it will follow that the non-introduction of such can produce nothing wrong." Thus, $x^2 + a\,x + b$, which is equal to

$$\left\{\sqrt{\left(x+\frac{a}{2}\right)^2} + \sqrt{\left(\frac{a^2}{4}-b\right)}\right\}$$
$$\times\left\{\sqrt{\left(x+\frac{a}{2}\right)^2} - \sqrt{\left(\frac{a^2}{4}-b\right)}\right\}$$

is also equal to

$$\left\{\sqrt{\left(x+\frac{a}{2}\right)^2+\beta} + \sqrt{\left(\frac{a^2}{4}-b+\beta\right)}\right\}$$
$$\times\left\{\sqrt{\left(x+\frac{a}{2}\right)^2+\beta} - \sqrt{\left(\frac{a^2}{4}-b+\beta\right)}\right\}$$

whatever be the value of the quantity β; a conclusion which enables us to reason upon real quantities and to make $\beta = 0$, when the primitive factors are required. Similarly, if instead of $\dfrac{e^{z\sqrt{-1}} + e^{-z\sqrt{-1}}}{2} = y$, we suppose $\dfrac{e^{z\sqrt{\beta-1}} + e^{-z\sqrt{\beta-1}}}{2} = y - \mathrm{R}'$, and if instead of $\dfrac{e^{z\sqrt{-1}} - e^{-z\sqrt{-1}}}{2\sqrt{-1}} = x$, we suppose $\dfrac{e^{z\sqrt{\beta-1}} - e^{-z\sqrt{\beta-1}}}{2\sqrt{\beta-1}} = x - \mathrm{R}$, we shall find, whatever β may be, $e^{z\sqrt{\beta-1}} = y - \mathrm{R}' + \sqrt{\beta-1}\,(x - \mathrm{R})$, a result which degenerates into the well known theorem $e^{z\sqrt{-1}} = y + \sqrt{-1}\,x$, if $\beta = 0$. Many other examples are given of this mode of *porismatizing* expressions, (a term derived by Mr. Gompertz from the definition of porisms in geometry,) by which operations are performed upon real quantities which would be otherwise imaginary: and if it was required to satisfy a scrupulous mind respecting the correctness of the *real* conclusions which are derived by the use of imaginary expressions, there are few methods which appear to me better calculated for this purpose than the adoption of this most refined and beautiful expedient.

The second tract of Mr. Gompertz appears to have been suggested by M. Buée's paper in the *Philosophical Transactions*, to which reference has been made in the text: it is devoted to the algebraical representation of lines both in position and in magnitude, as a part of a theory of what he terms *functional projections*, and embraces the most important of the conclusions obtained by Argand and Français, with whose researches, however, he does not appear to have been acquainted. I should by no means consider the process of reasoning which he has followed for obtaining these results to be such as would naturally or necessarily follow from the fundamental assumptions of algebra: but it would be unjust to Mr. Gompertz not to express my admiration of the skill and ingenuity which he has shown in the treatment of a very novel subject and in the application of his principles to the solution of many curious and difficult geometrical problems.

If we assume a to denote a finite quantity, then

(1.) $a \pm 0 = a$, and $a \pm \infty = \pm \infty$.

Consequently 0 does not affect a quantity with which it is connected by the sign $+$ or $-$, whilst ∞, similarly connected with such a quantity, altogether absorbs it.

(2.) $a \times 0 = 0$, $a \times \infty = \infty$; $\dfrac{a}{0} = \infty$ and $\dfrac{a}{\infty} = 0$.

It is this *reciprocal* relation between *zero* and *infinity* which is the foundation of the great analogy which exists between their analytical properties.

(3.) If these symbols be considered *absolutely* by themselves, without any reference to their symbolical origin, then we must consider $\dfrac{0}{0} = 1$ and $\dfrac{\infty}{\infty} = 1$.

But if those symbols be considered as the representatives *equally* of all orders of *zeros* and *infinities* respectively, then $\dfrac{0}{0}$ and $\dfrac{\infty}{\infty}$ may represent either 1 or a or 0 or ∞, its final form and value being determined, when capable of determination, by an examination of the particular circumstances under which those symbols originated. The whole theory of vanishing fractions will depend upon such considerations.

Having ascertained the principal symbolical conditions which 0 and ∞ are required to satisfy, we shall be prepared to consider likewise the principle of their interpretation. The examination of a few cases of their occurrence may serve to throw some light upon this inquiry.

Let us consider, in the first place, the interpretation of the *critical values* 0, ∞ and $\dfrac{0}{0}$ in the formulæ which express the values of x and y in the simultaneous equations,

$$\left.\begin{aligned} a\,x + b\,y &= c \\ a'\,x + b'\,y &= c' \end{aligned}\right\}$$

In this case we find

$$x = \frac{b\,b'\left\{\dfrac{c}{b} - \dfrac{c'}{b'}\right\}}{a\,a'\left\{\dfrac{b'}{a'} - \dfrac{b}{a}\right\}} \text{ and } y = \frac{a\,a'\left\{\dfrac{c}{a} - \dfrac{c'}{a'}\right\}}{b\,b'\left\{\dfrac{a'}{b'} - \dfrac{a}{b}\right\}}.$$

If $\dfrac{c}{b} = \dfrac{c'}{b'}$, $\dfrac{c}{a} = \dfrac{c'}{a'}$, and therefore $\dfrac{a}{b} = \dfrac{a'}{b'}$, then $x = \dfrac{0}{0}$ and $y = \dfrac{0}{0}$.

In this case $a' = m\,a$, $b' = m\,a$, and $c' = m\,c$, and the second equation is deducible from the first, and does not furnish, therefore, a new condition: under such circumstances, therefore, the values of x and y are really *indeterminate*, and the occurrence of $\frac{0}{0}$ in the values of the expressions for x and y is the *sign*, or rather the indication of that indetermination.

If $\frac{c}{b}$ be not equal to $\frac{c'}{b'}$, but if $\frac{b}{a}$ be equal to $\frac{b'}{a'}$, then $x = \infty$ and $y = \infty$. In this case we have $a' = m\,a$, $b' = m\,b$, but c' is not equal to $m\,c$; and the conditions furnished are *inconsistent*, or more properly speaking *impossible*. In this case, the occurrence of the sign ∞ in the expressions for x and y is the sign or indication of this inconsistency or *impossibility*: and it should be observed that no *infinite* values of x and y, if the *infinities* thus introduced were considered as real existences and *identical* in both equations, would satisfy the two equations any more than any two finite values of x and y which would satisfy one of them. We may properly interpret ∞ in this case by the term *impossible*.

If $\frac{c}{b} = \frac{c'}{b'}$, but if $\frac{b'}{a'}$ be not equal to $\frac{b}{a}$, then x is *zero* and y is finite, and therefore *possible*. It is in this sense that we should include *zero* amongst the *possible* values of x or y, a use or rather an *abuse* of language to which we are somewhat familiarized, from speaking of the *zero* of quantity as an existing state of it in the transition from one affection of quantity to another.

If we should take the equations of two ellipses, whose semi-axes are a and b, a' and b' respectively, which are

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} = 1,$$

$$\frac{x^2}{a'^2} + \frac{y^2}{b'^2} = 1,$$

and consider them as simultaneous when expressing the co-ordinates of their points of intersection, then we should find

$$x = \frac{\sqrt{\{b^2 - b'^2\}}}{\sqrt{\left\{\frac{b^2}{a^2} - \frac{b'^2}{a'^2}\right\}}} \text{ and } y = \frac{\sqrt{\{a^2 - a'^2\}}}{\sqrt{\left\{\frac{a^2}{b^2} - \frac{a'^2}{b'^2}\right\}}}.$$

If we suppose $\frac{b}{a} = \frac{b'}{a'}$, or the ellipses to be similar, and at the same time b not equal to b', then $x = \infty$ and $y = \infty$, which would properly be *interpreted* to mean that under no circumstances whatever, whether in the plane of $x\,y$ or in the plane at right angles to it, in which the hyperbolic portions* of curves expressed by those equations are included, would a point of intersection or a simultaneous value of x and y exist: or in other words, the sign or symbol ∞ would in this case mean that such intersection was *impossible.* If we supposed $\frac{b}{a} = \frac{b'}{a'}$ and also $b = b'$, or the ellipses to be coincident in all their parts, then we should find $x = \frac{0}{0}$ and $y = \frac{0}{0}$, indicating that their values were indeterminate, in as much as every part in the identical curves would be also a point of intersection, and would furnish therefore simultaneous values. If we should suppose b greater than b', a greater than a', and $\frac{a}{b}$ not equal to $\frac{a'}{b'}$, then we should find

$$x = \alpha \text{ and } y = \beta\sqrt{-1},$$
$$\text{or } x = \alpha\sqrt{-1} \text{ and } y = \beta,$$

according as $\frac{a}{b}$ is less or greater than $\frac{a'}{b'}$. In this case, one ellipse entirely includes the other, but the hyperbolic portions at right angles to their planes, which are in the direction of the major axis in one case and in that of the minor axis in the other, will intersect each other at points whose coordinates are the values of x and y above given: it would appear, therefore, that the *impossible* intersection of the curves would be indicated by the sign or symbol ∞ alone, and not by $\sqrt{-1}$.

The preceding example is full of instruction with respect to the interpretation of the signs of algebra, when viewed in connexion with the specific values and representations of the symbols; and there are few problems in the application of algebra to the theory of curve lines which would not furnish the materials for similar conclusions respecting them: but it is chiefly with reference to the connexion of those signs with changes in the nature of quantities, and in the form and constitution of expressions, that their interpretations will require the most careful study and examination. We shall proceed to notice a few of such cases.

* If in the equation $\frac{x^2}{a^2} + \frac{y^2}{b^2} = 1$, we suppose y replaced by $y\sqrt{-1}$, and the line which it represents when not affected by $\sqrt{-1}$ to be moved through 90° at right angles to the plane of $x\,y$, we shall find an hyperbola included in the equation of the ellipse.

The second member of the equation

$$\frac{1}{a-b} = \frac{1}{a} + \frac{b}{a^2} + \frac{b^2}{a^3} + \ldots.$$

preserves the same form, whatever be the relation of the values of a and b, and the operation, which produces it, is equally practicable in all cases. As long as a is greater than b, $a-b$ is positive, and there exists, or may be conceived to exist, a perfect arithmetical equality between the two members of the equation. If, however, $a = b$, we have $\frac{1}{0}$ upon one side and the sum of an infinite series of units multiplied into $\frac{1}{a}$ upon the other, and both the members are correctly represented by ∞; but if a be less than b, we have a *negative* and a *finite* value upon one side of the equation, and an infinite series of perpetually increasing terms upon the other, forming one of those quantities to which the older algebraists would have applied the term *plus quam infinitum*, and which we shall represent by the sign or symbol ∞. It remains to interpret the occurrence of such a sign under such circumstances.

The first member of this equation $\frac{1}{a-b}$ is said to *pass through infinity* when its sign changes from + to −, or conversely: its equivalent algebraical form presents itself in a series which is incapable of indicating the peculiar change in the nature of the quantity designated by $\frac{1}{a-b}$, which accompanies its change of sign. The *infinite* values, therefore, of the equivalent series (for in its general algebraical form, where no regard is paid to the specific values of the symbols, it is still an equivalent form,) is the indication of the *impossibility* of exhibiting the value of $\frac{1}{a-b}$ in a series of such a form under such circumstances.

Let us, in the second place, consider the more general series for $(a-b)^n$, or

$$(a-b)^n = a^n \left\{ 1 - n \cdot \frac{b}{a} + \frac{n(n-1)}{1 \cdot 2} \cdot \frac{b^2}{a^2} \right.$$
$$\left. - \frac{n(n-1)(n-2)}{1 \cdot 2 \cdot 3} \cdot \frac{b^3}{a^3} + \&c. \right\}$$

The inverse ratio of the successive coefficients of this series approximates continually to -1 as a limit, and the terms become all positive or all negative, according as the first negative coefficient is that of an *odd* or of an *even* power of $\frac{b}{a}$. It follows, therefore, that if a be greater than b, the series will be convergent and finite in all cases; if a be equal to b, it will be 0, 1, or ∞, according as n is positive, 0, or negative; and if a be less than b, it will be *infinite.*

The occurrence of the last of these signs or values is an indication *generally* that some change has taken place in the nature of the quantity expressed by $(a-b)^n$, in the transition from $a > b$ to $a < b$, which is of such a kind that the corresponding series is not competent to express it: thus, if $n = \frac{1}{2}$, then $(a-b)^n$ is affected with the sign $\sqrt{-1}$ when a is less than b, whilst no such sign is introduced nor introducible into the equivalent series corresponding to such relative values of a and b: and a similar change will take place, whenever a transition through *zero* or *infinity* takes place.

In this last case $(a-b)^n$ would appear to attain to *zero* or *infinity*, but not to pass through it, and no change would apparently take place in its affection corresponding to the change of affection of $a-b$; but the corresponding series will under the same circumstances change from being *finite* to *infinite*, a circumstance which we shall afterwards have occasion to notice, and which we shall endeavour to explain in the course of our observations upon the subject of diverging and converging series.

In the preceding examples the sign or symbol ∞ has not presented itself immediately, but has replaced an infinite series of terms, whose sum exceeded any finite magnitude; and it may be considered as indicating the *incompetence* of such a series to express the altered state or conditions of the quantity or fraction to which it was required to be altogether, as well as algebraically, equivalent. In the examples which follow, it will present itself immediately and will be found to be the indication of a change in the algebraical form of the term or terms in which it appears, or rather that no terms of the form assigned can present themselves in the required equivalent series or expression.

The integral $\int x^n \, dx = \frac{x^{n+1}}{n+1} + C$ is said to *fail* when $n = -1$, in as much as it appears that under such circumstances $\frac{x^{n-1}}{n+1}$ becomes ∞, which is an indication that the variable part of $\int x^n \, dx$ is no longer expressible by a function under the form $\frac{x^{n+1}}{n+1}$, but by one which must be determined by independent considerations. A knowledge, however, of the nature of its form in this particular case has enabled algebraists to bring it under a general form, by which the sign of *failure* or *impossibility* is replaced by the sign of *indetermination* $\frac{0}{0}$; for if we put $\frac{x^{n+1}}{n+1} + C = \frac{x^{n+1} - a^{n+1}}{n+1} + C$, (borrowing $\frac{-a^{n+1}}{n+1}$ from the arbitrary constant,) we shall get an expression which becomes $\frac{0}{0}$ when $n = -1$, and whose value, determined according to the rules which are founded upon the analytical properties of 0, will be $\log x + C$.

A more general example of the same kind, including the one which we have just considered, is given in the note to page 211, where it is required to determine the general form of $\frac{d^r}{dx^r} \cdot \frac{1}{x}$ and of $\frac{d^r}{dx^r} \cdot \frac{1}{x^n}$ (where n is a positive number) for all values of r: a formula is there constructed, from our knowledge of the form in the excepted case, which is capable of correctly expressing its value in all cases whatever.

The cases in which the series of Taylor is said to *fail* are of a similar nature. Thus, if $u = \varphi(x) = x + \sqrt{x - a}$, then

$$u' = \varphi(x + h) = u + \frac{du}{dx} h + \frac{d^2 u}{dx^2} \frac{h^2}{1 . 2} + \frac{d^3 u}{dx^3} \frac{h^3}{1 . 2 . 3} \text{ \&c.};$$

and if we suppose $x = a$, all the differential coefficients $\frac{du}{dx}$, $\frac{d^2 u}{dx^2}$, &c., become *infinite*, which is an indication that no terms of such a form exist in its developement, which becomes, under such circumstances, $a + \sqrt{h}$. The reasons of this *failure* in such cases have been very completely explained by Lagrange and other writers; but it is possible, by presenting the developement which constitutes Taylor's series under a somewhat different and a somewhat more general form, that the series may be so constructed as to include all the excepted cases.

There are two modes in which the developement of $\varphi(x + h)$ according to powers of h may be supposed to be effected. In the first and common mode we begin by excluding all those terms in the developement whose existence would be inconsistent with general values of the symbols: in the second we should assume the existence of all the terms which *may* correspond to values of the symbols, whether general or specific, and then prescribe the form which they *must* possess, consistently with the conditions which they are required to satisfy. If we adopt this second course, and assuming $u = \varphi(x)$ and $u' = \varphi(x + h)$, if we make

$$u' = u + \mathrm{A}\, h^a + \mathrm{B}\, h^b + \mathrm{C}\, h^c + \&c.,$$

the inquiry will then be, if there be such a term as $\mathrm{A}\, h^a$, where A is a function of x or a constant quantity, and a is any quantity whatsoever, what are the properties of A by which it may be determined? For this purpose we shall proceed as follows. It is very easy to show, from general considerations, that if u' be considered successively as a function of x and of h, $\frac{d^r u'}{d x^r} = \frac{d^r u'}{d h^r}$, for all values of r, whether whole or fractional, positive or negative: it will follow, therefore, (adopting the principles of differentiation to general indices which have been laid down in the note, p. 211,) that

$$\frac{d^a u'}{d h^a} = \frac{\Gamma(1+a)}{\Gamma(1)} \cdot \mathrm{A} + \frac{\Gamma(1+b)}{\Gamma(1+b-a)} \cdot \mathrm{B}\, h^{b-a} + \&c.,$$

omitting the arbitrary complementary functions, which will involve powers of h. In a similar manner we shall get

$$\frac{d^a u'}{d x^a} = \frac{d^a u}{d x^a} + \frac{d^a \mathrm{A}}{d x^a} \cdot h^a + \frac{d^a \mathrm{B}}{d x^a} \cdot h^b + \&c.$$

If these results be identical with each other, we shall find

$$\frac{\Gamma(1+a)}{\Gamma(1)} \cdot \mathrm{A} = \frac{d^a u}{d x^a},$$

and, therefore, $\mathrm{A} = \frac{1}{\Gamma(1+a)} \cdot \frac{d^a u}{d x^a}$, since $\Gamma(1) = 1$. It is easy to extend the same principle to the determination of the other coefficients, and we shall thus find

$$u' = u + \frac{d^a u}{d x^a} \cdot \frac{h^a}{\Gamma(1+a)} + \frac{d^b u}{d x^b} \cdot \frac{h^b}{\Gamma(1+b)} + \&c.; \quad (1.)$$

or, in other words, it follows that the coefficient of any power of h whose index is r will be

$$\frac{1}{\Gamma(1+r)} \cdot \frac{d^r u}{d x^r}.$$

The next step is to adapt the series (1) to the different cases which an examination of the constitution of the function u' will present to us.

If we suppose x to possess a general value, then u' and u will possess the same number of values, and no fractional power of h can present itself in the developement. In this case $\Gamma(1 + a) = 1 \,.\, 2 \ldots a$, and it may be readily proved that the successive indices a, b, c, &c., are the successive numbers 1, 2, 3, &c., and that consequently,

$$u' = u + \frac{d\,u}{d\,u} \cdot h + \frac{d^2 u}{d\,x^2} \cdot \frac{h^2}{1 \,.\, 2} + \frac{d^3 u}{d\,x^3} \cdot \frac{h^3}{1 \,.\, 2 \,.\, 3} + \text{\&c.}$$

It will also follow that the series for u' can involve no negative and integral power of h; for in that case the factorial $\Gamma(1 + a)$, which appears in its denominator, would become ∞, and the term would disappear. If it should appear, also, that for specific values of x any differential coefficient and its successive values should become infinite, they must be rejected from the developement, in as much as in that case the equation

$$\Gamma(1 + a)\,\mathrm{A} = \frac{d^a u}{d\,x^a}$$

would no longer exist, which is the only condition of the introduction of the corresponding terms. In other words, those terms in the developement of u' must be equally obliterated, which, under such circumstances, become either 0 or ∞.

If the general differential coefficient of u could be assigned, its examination would, generally speaking, enable us to point out its finite values wherever they exist, for those specific values of the symbols which make the integral differential coefficients *zero* or *infinity*. For all such values there will be a corresponding term in the developement of u' under those circumstances. Thus, if we suppose $u = x\sqrt{a - x}$, we shall find

$$\frac{1}{\Gamma(1+r)} \cdot \frac{d^r u}{d\,x^r} = \frac{(-1)^{r+1}\Gamma\left(\frac{3}{2}\right)}{\Gamma(1+r)\Gamma\left(\frac{3}{2} - r\right)} \cdot \left\{ \frac{r(a-x) - \left(\frac{3}{2} - r\right)x}{\left(\frac{3}{2} - r\right)(a-x)^{r-\frac{1}{2}}} \right\}:$$

if we make $x = a$, this expression will be neither *zero* nor *infinity* in two cases only, which are when $r = \frac{1}{2}$, and when $r = \frac{3}{2}$: in the first case we get,

$$\frac{1}{\Gamma\left(\frac{3}{2}\right)} \cdot \frac{d^{\frac{1}{2}} u}{d\,x^{\frac{1}{2}}} = \sqrt{-1} \,.\, a;$$

and in the second we get,

$$\frac{1}{\Gamma\left(\frac{5}{2}\right)} \frac{d^{\frac{3}{2}} u}{d x^{\frac{3}{2}}} = \frac{\sqrt{-1} \cdot \frac{3}{2}(a-a)}{\frac{3}{2} \Gamma(0) \times 0 \times (a-a)} = \sqrt{-1};$$

since $\Gamma(1) = 1 = 0\, \Gamma(0)$, and the symbol 0 in the denominator $= \frac{3}{2} - \frac{3}{2}$, is a *simple* zero. The corresponding developement of u' under such circumstances is

$$\sqrt{-a^2} \cdot h^{\frac{1}{2}} + \sqrt{-1} \cdot h^{\frac{3}{2}},$$

a result which is very easily verified.

If we pay a proper regard to the hypotheses which determine the existence of terms in the series for u' for specific values of the independent variable, we shall be enabled without difficulty to select the indices of the differential coefficients which can present themselves amongst the coefficients of the different powers of h in the developement. For, in the first place, $h^{\frac{m}{n}}$, and the differential coefficient whose index is $\frac{m}{n}$, will possess the same number of values, and the same signs of affection. If there be a term in u which $= \mathrm{P}\,(x-a)^{\frac{m}{n}}$, where P neither becomes *zero* nor *infinity*, when $x = a$, and where the multiple values of P, if any, are independent of those contained in $(x-a)^{\frac{m}{n}}$, then it will appear that the term of $\frac{d^{\frac{m}{n}} \cdot \mathrm{P} \cdot (x-a)^{\frac{m}{n}}}{d x^{\frac{m}{n}}}$ which is independent of $(x-a)^{\frac{m}{n}}$ is $\mathrm{P} \cdot \frac{d^{\frac{m}{n}} \cdot (x-a)^{\frac{m}{n}}}{d x^{\frac{m}{n}}}$, and that all the other terms of $\frac{d^{\frac{m}{n}} \cdot u}{d x^{\frac{m}{n}}}$, being either *zero* or *infinity* when $x = a$, or, if finite, introducing, through the medium of the factorial function by which they are multiplied, multiple values which are greater in number than those contained in u', must be rejected, as forming no part of the developement. It will of course follow, that the function P will become, under such circumstances, a function of h, and if we represent it by P′, and denote its values, and those of its successive differential coefficients, when $h = 0$, by p, p', p'', p''', &c., we shall find

$$\mathrm{P}' = p + p' h + p'' \frac{h^2}{1 \cdot 2} + p''' \frac{h^3}{1 \cdot 2 \cdot 3} + \text{\&c.},$$

none of which become *zero* or *infinity*, in as much as P does not vanish when $x = a$.

If there exist other terms in u of a similar kind, such as $Q(x-b)^{\frac{m'}{n'}}$, $R(x-c)^{\frac{m''}{n''}}$, &c., the same observations will apply to them. Such terms will correspond to values of x, which make radical expressions of any kind *zero* or *infinity*, and the form of the function u must be modified when necessary, so that such radicals may present themselves in single terms of the form $P(x-a)^{\frac{m}{n}}$. The same observations will apply to negative as well as positive values of $\frac{m}{n}$, unless we suppose $\frac{m}{n}$ a negative whole number. The principle of the exception in this last case may be readily inferred from the remarks in the note, p. 211, on the subject of the values of $\frac{d^{-r}}{dx^{-r}} \cdot \frac{1}{x^r}$, when n is a whole number. If we suppose, therefore, u to involve terms such as $P(x-a)^{\frac{m}{n}}$, $Q(n-b)^{\frac{m'}{n'}}$, &c., the most general form under which its developement can be put, supposing all terms which become *zero* or *infinity* for specific values of x to be rejected, will be as follows:

$$u' = u + \frac{du}{dx}h + \frac{d^2 u}{dx^2} \cdot \frac{h^2}{1.2} + \frac{d^3 u}{dx^3} \cdot \frac{h^3}{1.2.3} + \&c.,$$

$$+ \frac{a-a}{(x-a)} \cdot P' \cdot \frac{d^{\frac{m}{n}}(x-a)^{\frac{m}{n}}}{dx^{\frac{m}{n}}} \cdot \frac{h^{\frac{m}{n}}}{\Gamma\left(1+\frac{m}{n}\right)}$$

$$+ \frac{b-b}{x-b} \cdot Q' \cdot \frac{d^{\frac{m'}{n'}}(x-b)^{\frac{m'}{n'}}}{dx^{\frac{m'}{n'}}} \cdot \frac{h^{\frac{m'}{n'}}}{\Gamma\left(1+\frac{m'}{n'}\right)} + \&c.;$$

or,

$$u' = u + \frac{du}{dx}h + \frac{d^2 u}{dx^2}\,\frac{h^2}{1.2} + \frac{d^3 u}{dx^3}\,\frac{.h^3}{1.2.3} + \&c.,$$

$$+ \frac{a-a}{x-a}\left(p + p'h + p'' \cdot \frac{h^2}{1.2} + \&c.\right)h^{\frac{m}{n}}$$

$$+ \frac{b-b}{x-b}\left(q + q'h + q'' \cdot \frac{h^2}{1.2} + \&c.\right)h^{\frac{m'}{n'}}$$

$$+ \&c.$$

We have introduced the *discontinuous signs* or factors $\frac{a-a}{x-a}$,

$\frac{b-b}{x-b}$, &c., which become equal to 1 when $x=a$ or $x=b$, &c., but which are *zero* for all other values of x, to show that the terms into which they are multiplied disappear from the developement in all cases except for such specific values of x.

The existence of the terms of the series for u' is hypothetical only, and the equation which must be satisfied, as the essential condition of the existence of any assigned hypothetical term, at once directs us to reject those terms which would lead to infinite values of the differential coefficients, as well as those which possess multiple values which are incompatible with those contained in u'. It is quite obvious that upon no other principle could we either reject such infinite values, or justify the connexion of a series of terms with the general form of u', which have no existence except for specific values of x. The conclusion obtained is of considerable importance, in as much as it shows that the series of Taylor, if considered and investigated as having a contingent, and not a necessary existence, may be so exhibited as to comprehend all those cases in which it is commonly said to fail: and it will thus enable us to bring under the dominion of the differential calculus many peculiar cases in its different applications which have hitherto required to be treated by independent methods.

Thus, if it was required to determine the value of the fraction $\frac{(x^2-a^2)^{\frac{5}{2}}}{x^2(x-a)^{\frac{5}{2}}}$, when $x=a$, we should find it to be,

$$\frac{\frac{d^{\frac{5}{2}}}{dx^{\frac{5}{2}}}\cdot(x^2-a^2)^{\frac{5}{2}}}{\frac{d^{\frac{5}{2}}}{dx^{\frac{5}{2}}}\cdot x^2(x-a)^{\frac{5}{2}}},$$

or,

$$\frac{(x+a)^{\frac{5}{2}}\cdot\frac{d^{\frac{5}{2}}}{dx^{\frac{5}{2}}}\cdot(x-a)^{\frac{5}{2}}}{x^2\cdot\frac{d^{\frac{5}{2}}}{dx^{\frac{5}{2}}}(x-a)^{\frac{5}{2}}}=\frac{(2a)^{\frac{5}{2}}}{a^2}=4\sqrt{2a};$$

a conclusion which would be justified by the developement of the numerator and denominator of this fraction by the complete form of Taylor's series, when $x=a$.

Many delicate and rather obscure questions in the theory of *maxima* and *minima*, particularly those which Euler has denominated *maxima* and *minima* of the second species, and others also relating to the *singular* or *critical* points of curve lines, must depend for their dilucidation upon this more general view of Taylor's series, as connected with the consideration of general differential coefficients*.

* Euler has devoted an entire chapter of his *Calculus Differentialis* to the examination of what he terms the *differentials* of functions in certain peculiar cases. It is well known that he adopted Leibnitz's original view of the principles of the differential calculus, and considered differentials of the first and higher orders as *infinitesimal* values of *differences of the first and higher orders.* Such a principle necessarily excludes the consideration of differential coefficients as *essentially* connected with determinate powers of the increment of the independent variable, which may be said to constitute the essence of Taylor's theorem, and which must be the foundation of all theories of the differential calculus, which make its results depend upon the relation of forms, and not upon the relation of values. As long, however, as the independent variable continues indeterminate, the symbolical values of the differentials are the same upon both hypotheses. But when we come to the consideration of specific values of the independent variable which make differential coefficients above or below a certain order, infinite or zero, then such a view of the nature of differentials necessarily confounds those of different orders with each other. Thus, if $y = a^{\frac{3}{2}} + (x - a)^{\frac{3}{2}}$, Euler makes, when $x = a$, $d\, y = (d\, x)^{\frac{3}{2}}$, instead of $\dfrac{d^{\frac{3}{2}} y}{\Gamma\left(\frac{5}{2}\right)} = \dfrac{d^{\frac{3}{2}} y}{\frac{3}{4}\sqrt{\pi}} = (d\, x)^{\frac{3}{2}}$. If $y = 2\, a\, x - x^2 + a \sqrt{(a^2 - x^2)}$, he makes, when $x = a$, $d\, y = a\sqrt{-2\, a}\, .\, d\, x^{\frac{1}{2}}$, instead of

$$d^{\frac{1}{2}} y = \frac{a^{\frac{3}{2}} (d\, x)^{\frac{1}{2}}}{\sqrt{2\, \pi}} \left\{ \frac{4\sqrt{2}}{3} + \pi \sqrt{-1} \right\}.$$

These examples are quite sufficient to make manifest the inadequacy of merely arithmetical views of the principles of the differential calculus to exhibit the correct relation which exists between different orders of differentials, and, *à fortiori*, therefore, between different orders of differential coefficients.

M. Cauchy, in his *Leçons sur le Calcul Infinitesimal* (published in 1823), has attempted to conciliate the direct consideration of *infinitesimals* with the purely algebraical views of the principles of this calculus, which Lagrange first securely established; and it may be very easily conceded that no attempt of this able analyst, however much at variance with ordinary notions or ordinary practice, would fail from want of a sufficient command over all the resources of analysis. He considers *all infinite series as fallacious which are not convergent,* and that, consequently, the series of Taylor, when it takes the form of an indefinite series, is not generally true. It is for this reason that he has transferred it from the differential to the integral calculus, and exhibits it as a series with a finite number of terms completed by a *definite* integral. It is very true that M. Cauchy has perfectly succeeded in dispensing with the consideration of infinite series in the establishment of most of the great principles of the differential and integral calculus; but I should by no means feel disposed to consider his success in overcoming difficulties which such a course presents as a decisive proof of the expediency of following in his footsteps. The fact is, that if the operations of algebra be general, we must necessarily obtain indefinite series, and if the symbols we employ are general likewise, it will be impossible to determine, in *most cases,* *Signs of discontinuity* are those signs which, in conformity with the general laws of algebra, are equal to 1 between given limits of one or more of the symbols involved, and are equal to zero for all their other values. If merely *conventional* signs were required, we might assume arbitrary symbols for this purpose, attaching to them far greater clearness as *diventical marks*, the limits of the symbol or symbols between which the sign of discontinuity was supposed to be applied. Thus, we might suppose ${}^{x}\mathrm{D}_{a}^{0}$ to denote 1, when x was taken between 0 and a, to denote zero for all other values; ${}^{x}\mathrm{D}_{a+b}^{a}$, to denote 1, when x was taken between a and $a + b$, and zero for all other values; and similarly in other cases.

Thus, if $y = \alpha x + \beta$ and $y = \alpha' x + \beta'$ were the equations of two lines, and if we supposed that the generating point whose coordinates are x and y was taken in the first line between the limits 0 and a, and in the second line between the limits a and b, then we should have generally,

$$y = {}^{x}\mathrm{D}_{a}^{0}(\alpha x + \beta) + {}^{x}\mathrm{D}_{b}^{a}(\alpha' x + \beta') \qquad (1.)$$

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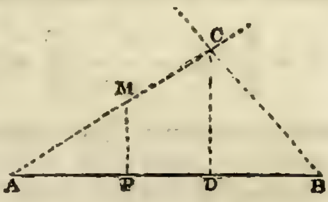
the convergency or divergency of the series which result. It is only, therefore, when we come to specific values that a question will arise *generally* respecting the character of the series: and it is only when we are compelled to deduce the function which generates the series from the application of the theory of limits to the aggregate of a finite number of its terms, that its convergency or divergency becomes important as affecting the practicability of the inquiry: in short, it must be an erroneous view of the principles of algebra which makes the result of any general operation dependent upon the fundamental laws of algebra to be fallacious. The deficiency should in all such cases be charged upon our power of interpretation of such results, and not upon the results themselves, or upon the certainty and generality of the operations which produce them: in short, the rejection of diverging series from analysis, or of such series as may become divergent, is altogether inconsistent with the spirit and principles of symbolical algebra, and would necessarily bring us back again to that tedious multiplication of cases which characterized the infancy of the science. A very instructive example of the consequences of adopting such a system may be seen in the researches of M. Liouville, which have been noticed in the note at p. 217.

Lagrange in his *Théorie des Fonctions Analytiques*, and in his *Calcul des Fonctions*, has given theorems for determining the limits between which the remainder of Taylor's series, after a finite number of terms, is situated: and the same subject has been very fully discussed in a memoir by Ampère, in the sixth volume of the *Journal de l'Ecole Polytechnique*. Such theorems are extremely important in the practical applications of this series, but they in no respect affect either the existence or the derivation of the series itself. It is a very common error to confound the order in which the conclusions of algebra present themselves, and to connect difficulties in the interpretation and application of results with the existence of the results themselves: and it is the influence of this prejudice which has induced some of the greatest modern analysts, not merely to deny the use, but to dispute the correctness of diverging series.

Messrs. Swinburne and Tylecote, the joint authors of a *Treatise on the true*

Thus, if in the triangle A C B, we draw C D, a perpendicular from the vertex to the base, and if we suppose A D $= a$, A B $= b$, A the origin of the coordinates, A B the axis of x, $y = \alpha x$ the equation of the line A C, and $y = \alpha' x + \beta'$ the equation of the line B C, then we should find that the value of y represented by the equation



$$y = {}^x\mathrm{D}_a^{\,0} \,.\, \alpha x + {}^x\mathrm{D}_b^{\,a} \,(\alpha' x + \beta')^* \qquad (2.)$$

would be *confined* to the two sides A C and B C of the triangle A B C, excepting only the point C, which corresponds to the *common* limit of the discontinuous signs. For if we suppose ${}^x\mathrm{D}_a^{\,0}$ and ${}^x\mathrm{D}_b^{\,a}$ to be true up to their limits, we shall find, when $x = a$, that ${}^x\mathrm{D}_a^{\,0} + {}^x\mathrm{D}_b^{\,a} = 2$. If we replace, however,

$${}^x\mathrm{D}_a^{\,0} \text{ by } {}^x\mathrm{D}_a^{\,0} - \frac{a-a}{x-a}, \text{ and } {}^x\mathrm{D}_b^{\,a} \text{ by } {}^x\mathrm{D}_b^{\,a} - \frac{b-b}{x-b},$$

Developement of the Binomial Theorem, which was published in 1827, have contended vigorously for the restriction of the meaning of the sign $=$ to simple arithmetical equality, and would reject its use when placed between a function and its developement, unless its complete remainder, after a finite number of terms, should replace the remaining terms of the series; or unless, when the indefinite series was supposed to be retained, the value or the generating function of this remainder could be assigned. In conformity with this principle they have assigned the remainder in the series for $(a + x)^n$, which they exhibit under the following form:

$$(a+x)^n = a^n + n\, a^{n-1}\, x + \ldots . \frac{n\,(n-1)\ldots(n-r+1)}{1\,.\,2\ldots\quad r} \,.\, a^{n-r}\, x^r$$

$$+ x^{r+1}\,(a+x)^n \left\{ \frac{1}{(a+x)^{r+1}} + \frac{(r+1)}{1} \,.\, \frac{a}{(a+x)^{r+2}} \right.$$

$$\left. + \ldots . \frac{(r+1)\,(r+2)\ldots(n-1)}{1\,.\,2\ldots(n-r-1)} \,.\, \frac{a^{n-r-1}}{(a+x)^n} \right\};$$

the remainder being $(a + x)^n\, x^{r+1}$ multiplied into $n - r$ terms of the developement of $\dfrac{1}{\{(a+x)-a\}^{r+1}}$, or of $\dfrac{1}{x^{r+1}}$.

The method which they have employed for this purpose, which is extremely ingenious, succeeds for integral values of n, whether positive or negative, but fails to assign the law when the index is fractional. But my own views of the principles of symbolical algebra would, of course, induce me to attach very little value to results which were exhibited in such a form as to be incapable of being generalized, a defect under which the formula given above evidently labours.

* The conventional sign ${}^x\mathrm{D}_b^{\,a}$ might be replaced, though not with perfect propriety, by the definite integral $\dfrac{1}{a-b}\displaystyle\int_b^a dx$.

and if we make, therefore,

$$y = \left\{ {}^{x}\mathrm{D}_a{}^0 - \frac{a-a}{x-a} \right\} \alpha\, x + \left\{ {}^{x}\mathrm{D}_b{}^a - \frac{b-b}{x-b} \right\} (\alpha' x + \beta') \qquad (3.)$$

the equation will be true for the ordinate of *every* point of the sides A C and C B of the triangle A B C.

More generally, if we suppose $y = \phi_1 x$, $y = \phi_2 x$, $y = \phi_3 x$, $y = \phi_4 x$, &c., to be the equations of a series of curves, then the equation of a polylateral curve composed of the several portions of the separate curves corresponding to values of x, included between the limits a and b, b and c, c and d, &c., would be,

$$y = \left({}^{x}\mathrm{D}_b{}^a - \frac{b-b}{x-b} \right) \phi_1 x + \left({}^{x}\mathrm{D}_c{}^b - \frac{c-c}{x-c} \right) \phi_2 x$$

$$+ \left({}^{x}\mathrm{D}_d{}^c - \frac{d-d}{x-d} \right) \phi_3 x + \text{\&c.}; \qquad (4.)$$

the value of the ordinate at each successive limit being replaced by that of the succeeding curve. In this manner, if we should grant the existence of the sign of discontinuity, we should be enabled to represent the equations of polygons, and of polylateral curves of every description.

It remains to consider the nature of the expressions which are competent to express ${}^{x}\mathrm{D}_b{}^a$.

The expressions which have been generally proposed for this purpose are either infinite series, or their equivalent *definite* integrals. Le Comte de Libri, however, a Florentine analyst of distinguished genius, has proposed* a finite exponential expression which will answer this purpose. The examination of the expression

$$e^{(\log 0)\, e^{(\log 0)\,(x-a)}}$$

would readily show that its value is 1 when x is greater than a, and that it is 0 when x is equal to or less than a. It will therefore follow that the product

$$e^{(\log 0)\, e^{(\log 0)\,(x-a)}} \times e^{(\log 0)\, e^{(\log 0)\,(b-x)}}$$

is equal to 1 between the limits a and b, and is equal to 0 *at those limits*, and for all other values. And, in as much as

* *Mémoires de Mathématique et de Physique*, p. 44. Florence 1829. The author has since been naturalized in France, and has been chosen to succeed Legendre as a member of the Institute: he has made most important additions to the mathematical theory of numbers.

$e^{(\log 0)} = 0$, we may replace the preceding product by the equivalent expression

$$0^{0^{(x-a)}} \times 0^{0^{(b-x)}}.$$

This expression, which is equivalent to ${}^{x}D_b^a - \frac{a-a}{x-a} - \frac{b-b}{x-b}$, has been applied by Libri to the expression of many important theorems in the theory of numbers *.

The definite integral $\int_0^\infty \frac{dr}{r} \sin r\, x$ has been shown by Euler † and many other writers, to be equal to $\frac{\pi}{2}$ when x is positive, to 0 when x is 0, and to $\frac{-\pi}{2}$ when x is negative. It follows, therefore, that

$$\frac{2}{\pi}\int_0^\infty \frac{dr}{r} \sin \frac{(b-a)}{2} r \cos\left\{x - \frac{(a+b)}{2} r\right\}$$

$$= \frac{1}{\pi}\int_0^\infty \frac{dr}{r} \sin (x-a) r + \frac{1}{\pi}\int_0^\infty \frac{dr}{r} \sin (x-b) r$$

is equal to 1, when x is between the limits a and b, to $\frac{1}{2}$, when x is at those limits, and to *zero*, for all other values. If we denote the definite integral $\frac{2}{\pi}\int_0^\infty \frac{dr}{r} \sin \frac{(b-a)}{2} r \cos\left\{x - \frac{(a+b)}{2}\right\} r$ by C_b^a, we shall get,

$$ {}^{x}D_b^a = C_b^a + \frac{a-a}{2(x-a)} + \frac{b-b}{2(x-b)};$$

and consequently the equation of a polylateral curve, such as that which is expressed by equation (4.), will be,

$$y = C_b^a \,.\, \varphi_1 x + C_c^b \,.\, \varphi_2 x + C_d^c \,.\, \varphi_3 x + \&c.,$$

in as much as at the limits we have $\varphi_1(b) = \varphi_2(b)$, $\varphi_2(c) = \varphi_3(c)$, and consequently for such limits $C_b^a\, \varphi_1(b) + C_c^b\, \varphi_2(b) = \varphi_1(b) = \varphi_2(b)$, and not $2\,\varphi_2(b)$.

All definite integrals which have determinate values within given limits of a variable not involved in the integral sign, may be converted into formulæ which will be equal to 1 within those

* *Crelle's Journal* for 1830, p. 67.

† *Inst. Calc. Integ.*, tom. iv.; Fourier, *Théorie de la Chaleur*, p. 442.; Frulloni, *Memorie della Società Italiana*, tom. xx. p. 448.; Libri, *Mémoires de Mathématique et de Physique*, p. 40.

limits and also including the limits, and to *zero* for all other values *. But the expressions which thence arise, though furnishing their results in strict conformity with the laws of symbolical combinations, possess no advantage in the business of calculation beyond the conventional and arbitrary signs of discontinuity which we first adopted for this purpose : but though it is frequently useful and necessary to express such signs *explicitly*, and to construct formulæ which may answer any assigned conditions of discontinuity, yet such conditions will be also very commonly involved *implicitly*, and their existence and character must be ascertained from an examination of the properties of the discontinuous formulæ themselves. We shall now proceed to notice some examples of such formulæ.

The well known series †

$$r\pi + \frac{x}{2} = \sin x - \frac{1}{2}\sin 2x + \frac{1}{3}\sin 3x - \frac{1}{4}\sin 4x + \&c. \quad (1.)$$

is limited to integral values of r, whether positive or negative, and to such values of $r\pi + \frac{x}{2}$ as are included between $\frac{\pi}{2}$ and $-\frac{\pi}{2}$: the value of r, therefore, is not *arbitrary* but *condi-*

* If a definite integral (C) has n determinate values $\alpha_1, \alpha_2, \ldots \alpha_n$, within the limits of the variable a and b, and no others, the values at those limits being included, and if C be equal to zero for all values beyond those limits, then we shall find

$${}^{x}D_b^a = -\frac{(C-\alpha_1)(C-\alpha_2)\ldots(C-\alpha_n)}{\alpha_1 \times \alpha_2 \times \ldots \alpha_n} + 1:$$

thus in the case considered in the text, we get

$${}^{x}D_b^a = -2(C-1)\left(C-\frac{1}{2}\right) + 1 = -2C^2 + 3C.$$

† The principle of the introduction of $r\pi$ in equation (1.) by which it is generalized, will be sufficiently obvious from the following mode of deducing it:

$$\log\left\{\frac{1+e^{x\sqrt{-1}}}{1+e^{-x\sqrt{-1}}}\right\} = \log\left\{1+e^{x\sqrt{-1}}\right\} - \log\left\{1+e^{-x\sqrt{-1}}\right\}$$

$$= \log e^{-x\sqrt{-1}} = x\sqrt{-1} + 2r\pi\sqrt{-1} = \left(e^{x\sqrt{-1}} - e^{-x\sqrt{-1}}\right)$$

$$-\frac{1}{2}\left\{e^{2x\sqrt{-1}} - e^{-2x\sqrt{-1}}\right\} + \frac{1}{3}\left\{e^{3x\sqrt{-1}} - e^{-3x\sqrt{-1}}\right\} + \&c.$$

and, therefore, dividing by $2\sqrt{-1}$, and replacing the exponential expressions by their equivalent values, we get

$$r\pi + \frac{x}{2} = \sin x - \frac{1}{2}\sin 2x + \frac{1}{3}\sin 3x - \frac{1}{4}\sin 4x + \&c.,$$

where x upon the second side of the equation may have any value between $+\infty$ and $-\infty$.

tional. If we successively replace, therefore, x by $\frac{\pi}{2} + x$ and $\frac{\pi}{2} - x$, we shall get

$$r\pi + \frac{\pi}{4} + \frac{x}{2} = \cos x + \frac{1}{2}\sin 2x - \frac{1}{3}\cos 3x - \frac{1}{4}\sin 4x + \&c.$$

$$r'\pi + \frac{\pi}{4} - \frac{x}{2} = \cos x - \frac{1}{2}\sin 2x - \frac{1}{3}\cos 3x + \frac{1}{4}\sin 4x + \&c.$$

Adding these two series together and dividing by 2, we get

$$\frac{(r+r')}{2}\pi + \frac{\pi}{4} = \cos x - \frac{1}{3}\cos 3x + \frac{1}{3}\cos 5x - \&c. \qquad (2.)$$

If x be included between $\frac{\pi}{2}$ and $-\frac{\pi}{2}$, then $r = 0$ and $r' = 0$, and we get

$$\frac{\pi}{4} = \cos x - \frac{1}{3}\cos 3x + \frac{1}{5}\cos 5x - \&c. \qquad (3.)$$

If x be included between $\frac{\pi}{2}$ and $\frac{3\pi}{2}$, then $r = -1$ and $r' = 0$, and we get

$$-\frac{\pi}{4} = \cos x - \frac{1}{3}\cos 3x + \frac{1}{5}\cos 5x - \&c. \qquad (4.)$$

If the limits of x be $\frac{3\pi}{2}$ and $\frac{5\pi}{2}$, $\frac{5\pi}{2}$ and $\frac{7\pi}{2}$, $\frac{-\pi}{2}$ and $-\frac{3\pi}{2}$, $-\frac{3\pi}{2}$ and $-\frac{5\pi}{2}$, we shall obtain values of the series (2.), which are alternately $\frac{\pi}{4}$ and $-\frac{\pi}{4}$.

Again, if in equation (1.), or $r\pi + \frac{x}{2} = \sin x - \frac{1}{2}\sin 2x + \frac{1}{3}\sin 3x - \frac{1}{4}\sin 4x + \&c.$, we replace x by $\pi - x$, we shall get

$$r'\pi + \frac{\pi - x}{2} = \sin x + \frac{1}{2}\sin 2x + \frac{1}{3}\sin 3x + \frac{1}{4}\sin 4x + \&c.$$

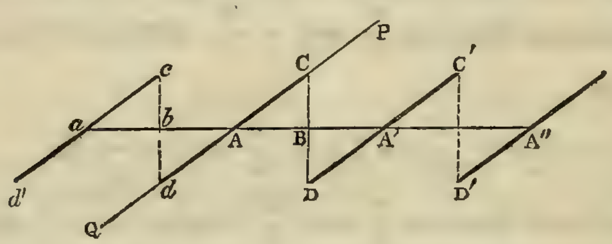
Adding these equations together and dividing by 2, we get

$$\frac{(r+r')}{2}\pi + \frac{\pi}{4} = \sin x + \frac{1}{3}\sin 3x + \frac{1}{5}\sin 5x + \&c. \quad (5.)$$

which may be easily shown to be equal to $\frac{\pi}{4}$ and $-\frac{\pi}{4}$ alternately, in the passage of x from 0 to π, from π to 2π, from 2π to 3π, &c., or from 0 to $-\pi$, from $-\pi$ to -2π, &c.: its values at those limits are *zero*.

The series (2.) and (5.) have been investigated by Fourier, in his *Théorie de la Chaleur* *, by a very elaborate analysis, which fails, however, in showing the dependence of these series upon each other and upon the principles involved in the deduction of the fundamental series: and they present, as we shall now proceed to show, very curious and instructive examples of discontinuous functions.

The equation $y = \frac{x}{2}$ is that of an indefinite straight line, Q A P, making an angle with the axis of x, whose tangent is

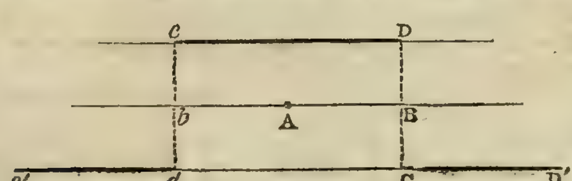


$\frac{1}{2}$, and which passes through the origin of the coordinates: whilst the equation

$$y = \sin x - \frac{1}{2}\sin 2x + \frac{1}{3}\sin 3x - \frac{1}{4}\sin 4x + \&c.$$

is that of a series of terminated straight lines, $d'\,c$, $d\,C$, $D\,C'$, &c., passing through points a, A, A$'$, &c., which are distant 2π from each other: the portion d C alone coincides with the primitive line, whose equation is $y = \frac{x}{2}$.

Again, the line whose equation is $y = \frac{\pi}{4}$, is parallel to the



* From page 167 to 190; also 267 and 356.

axis of x at the distance $\frac{\pi}{4}$ *above it*: the line whose equation is $-\frac{\pi}{4}$, is also parallel to the axis of x, at the distance $\frac{\pi}{4}$ *below it*: the line whose equation is

$$y = \cos x - \frac{1}{3}\cos 3x + \frac{1}{5}\cos 5x - \&c.$$

consists of *discontinuous* portions of the first and second of those lines, whose lengths are severally equal to π. The values of y at the points B and b, corresponding to $x = \frac{\pi}{2}$ and $-\frac{\pi}{2}$, are equal to *zero*, since the equidistant points D and C, c and d, are common to both equations at those points.

It would appear, therefore, in the cases just examined, that the conversion of one member of the equation of a line into a series of sines and cosines would change the character of that equation from being *continuous* to *discontinuous*, the coincidence of the two equations only existing throughout the extent of one complete period of circulation of the trigonometrical series: and more generally, if, in any other case, we could *effect* this conversion of one member of the equation of a curve into a series of sines or cosines, it is obvious that the second equation must be *discontinuous*, and that the coincidence would take place only throughout one period of circulation, whether from 0 to π or from $-\frac{\pi}{2}$ to $\frac{\pi}{2}$. It remains therefore to consider whether such a conversion is generally practicable.

Let us take n equidistant points in the axis of the curve whose equation is $y = \phi x$, between the limits 0 and π, those limits being excluded: if we denominate the corresponding values of the ordinate by $y_1, y_2, \dots y_n$, and if it be proposed to express the values of these ordinates by means of a series of sines (of n terms) such as

$$a_1 \sin x + a_2 \sin 2x + a_3 \sin 3x + \dots + a_n \sin nx,$$

then we shall get the following n equations to determine the n coefficients $a_1, a_2, a_3 \dots a_n$.

$$y_1 = a_1 \sin\frac{\pi}{n+1} + a_2 \sin\frac{2\pi}{n+1} + a_3 \sin\frac{3\pi}{n+1} + . a_n \sin\frac{n\pi}{n+1},$$

$$y_2 = a_1 \sin\frac{2\pi}{n+1} + a_2 \sin\frac{4\pi}{n+1} + a_3 \sin\frac{6\pi}{n+1} + . a_n \sin\frac{2n\pi}{n+1},$$

$$y_3 = a_1 \sin\frac{3\pi}{n+1} + a_2 \sin\frac{6\pi}{n+1} + a_3 \sin\frac{9\pi}{n+1} + . a_n \sin\frac{3n\pi}{n+1},$$

. . . .

$$y_n = a_1 \sin \frac{n\pi}{n+1} + a_2 \sin \frac{2n\pi}{n+1} + a_3 \sin \frac{3n\pi}{n+1} + . a_n \sin \frac{n^2\pi}{n+1}.$$

If any assigned coefficient a_m be required to be determined from this system of equations, we must multiply * them severally by

$$2 \sin \frac{m\pi}{n+1}, 2 \sin \frac{2m\pi}{n+1}, 2 \sin \frac{3m\pi}{n+1}, \ldots 2 \sin \frac{nm\pi}{n+1},$$

when all the coefficients except a_m will disappear from the *sum* of the resulting equations: and we shall thus find

$$a_m = \frac{2}{n+1}\left\{y_1 \sin \frac{m\pi}{n+1} + y_2 \sin \frac{2m\pi}{n+1} + \ldots y_n \sin \frac{nm\pi}{n+1}\right\}.$$

It would thus appear that it is always possible to determine a series of sines of n terms with finite and determinate coefficients, which shall be the equation of a curve which shall have n points in common with the curve whose equation is $y = \phi\, x$, within the limits corresponding to values of x between 0 and π; and it is obvious that the greater the number of those points, the more intimate would be the contact of these two curves throughout the finite space corresponding to those limits. If we should further suppose the number of those points to become infinitely great, then the number of terms of the trigonometrical series would be infinite likewise, and the coincidence of the curve which it expresses with the curve whose equation is $y = \phi\, x$, would be *complete* within those limits only, producing a species of contact to which the term *finite osculation* has been applied by Fourier †. Beyond those limits the curves would have no necessary relation to each other.

It would follow, also, from the preceding view of the theory of finite osculations, that the curve expressed by $y = \phi\, x$ might be perfectly arbitrary, continuous, or discontinuous. Thus, it might express the sides of a triangle, or of a polygon, or of a multilateral curve, or of any succession of points connected by any conceivable law; for in all cases when the corresponding ordinates of equidistant points are finite, we shall be enabled to determine values of the coefficients a_m which are finite or zero by the process which has been pointed out above.

* This is the process proposed by Lagrange in his "Théorie du Son," in the third volume of the *Turin Memoirs*, as stated by Poisson in his memoir on Periodic Series, &c., in the 19th cahier of the *Journal de l'Ecole Polytechnique*.

† *Théorie de la Chaleur*, page 250.

The hypothesis of n being infinite would convert the series for a_m into the definite integral *

$$\frac{2}{\pi}\int_{\pi}^{0} \varphi\, x \sin m\, x\, d\, x,$$

if we make $\frac{m\,\pi}{n+1} = x$ and $\frac{\pi}{n+1} = d\,x$: or otherwise if we assume the existence of the series

$$\varphi\, x = a_1 \sin x + a_2 \sin 2\,x + \ldots a_m \sin m\,n + \&c.,$$

it may be readily shown, by multiplying both sides of the equation by $\sin m\,x\,d\,x$, that

$$a_m = \frac{2}{\pi}\int_{\pi}^{0} \varphi\, x \sin m\, x\, d\, x:$$

and in a similar manner, if we should assume

$$\varphi\, x = a_0 \cos 0\, x + a_1 \cos x + \ldots a_m \cos m\, x + \&c.,$$

that

$$a_m = \frac{2}{\pi}\int_{\pi}^{0} \varphi\, x \cos m\, x\, d\, x\, \dagger.$$

Thus, if we should suppose $\varphi\, x = \cos x$, we should find

$$\cos x = \frac{4}{\pi}\left\{\frac{2}{1\,.\,3}\sin 2\,x + \frac{4}{3\,.\,5}\sin 4\,x + \frac{6}{5\,.\,7}\sin 6\,x + \&c.\right\};$$

a very singular result, which is of course only true between the limits 0 and π, excluding those limits ‡.

If we should suppose $\varphi\, x =$ a constant quantity $\frac{\pi}{2}$ between the limits 0 and α, and that it is equal to *zero* between α and π, we should find

$$\varphi\, x = \frac{(1-\cos\alpha)}{2}\sin x + \frac{(1-\cos 2\,\alpha)}{2}\sin 2\,x + \frac{(1-\cos 3\,\alpha)}{2}\times \sin 3\,x + \&c.,$$

excluding the limiting value α, when the value of the series is only $\frac{\pi}{4}$ §.

If we should suppose $\varphi\, x = {}^{x}\mathrm{D}_b{}^{0}\,.\,\alpha\, x + {}^{x}\mathrm{D}_b{}^{\pi}\,.\,(\alpha'\, x + \beta')$, which is the equation of the sides of a triangle (excluding the

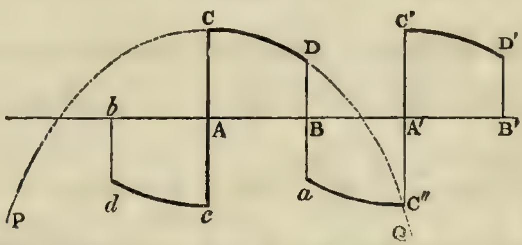
* Poisson, *Journal de l'Ecole Polytechnique*, cahier xix. p. 447.
† Fourier, *Théorie de la Chaleur*, pp. 235 & 240.
‡ *Ibid.*, p. 238; Poisson, *Journal de l'Ecole Polytechnique*, cahier xix. p. 418.
§ Fourier, *Théorie de la Chaleur*, p. 244.

limit $x = b$), whose base is represented by π, then we shall find *

$$\phi x = \frac{2}{\pi}\{\alpha\pi + (\alpha - \alpha')b\}\left\{\sin x - \frac{\sin 2x}{2} + \frac{\sin 3x}{3} - \&c.\right\}$$

$$+ \frac{2}{\pi}(\alpha - \alpha')\left\{\frac{\sin b \sin x}{1^2} + \frac{\sin 2b \sin 2x}{2^2} + \frac{\sin 3b \sin 3x}{3^2} + \&c.\right\}.$$

The trigonometrical series, in this last case, would represent a series of triangles placed alternately in an inverse position with respect to each other; and a similar observation would apply to the discontinuous curves which are represented by any series of sines and cosines. Thus, if $y = \phi x$ be the equation of the curve P C C″ Q, and if we suppose

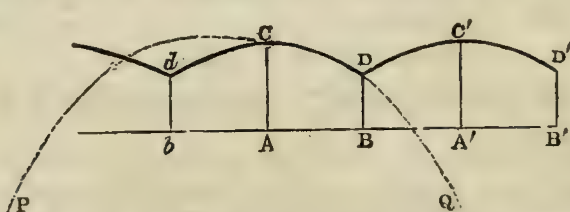


$$y = \phi x = a_1 \sin x + a_2 \sin 2x + a_3 \sin 3x + \&c.,$$

between the limits 0 and π; and if we make A B $= \pi$, A A′ $= 2\pi$, A B′ $= 3\pi$, &c., we shall get a *discontinuous* curve, consisting of a series of similar arcs, C D, *a* C″, C′ D′, &c., placed successively in an inverse relation with respect to each other upon each side of the axis of x, of which one arc C D alone coincides with the primitive curve.

If we should suppose the same curve to be expressed between the limits 0 and π by a series of cosines or

$$y = \phi x = a_0 + a_1 \cos x + a_2 \cos 2x + \&c.,$$



and if we make A B $= \pi$, A $b = -\pi$, A A′ $= 2\pi$, A B′ $= 3\pi$, &c., then the trigonometrical equation will represent a discontinuous curve d C D C′ D′, of which the portions C D and C d,

* Fourier has given a particular case of this series, p. 246.

C′ D′ and C′ D will be symmetrical by pairs; but one portion only, C D, will necessarily coincide with the primitive curve.

The theory of discontinuous functions has recently received considerable additions from a young analyst of the highest promise, Mr. Murphy, of Caius College, Cambridge. In an admirable memoir on the *Inverse Method of Definite Integrals* *, he has given general methods for representing discontinuous functions, of one or a greater number of *breaks*, by means which are more directly applicable to the circumstances under which they present themselves in physical problems than those which have been proposed by Fourier, Poisson, and Libri. Mr. Murphy had already, in a previous memoir †, given a most remarkable extension to the theory of the application of Lagrange's theorem to the expression of the least root of an equation, which we shall have occasion to notice hereafter; and he has shown that if $\phi\,(x)$ be an integral function of x then the coefficient of $\frac{1}{x}$ in the developement of $-\log\frac{\phi\,x}{x}$ will represent the least root of the equation $\phi\,x = 0$. We thus find that the *least* of the two quantities α and β will be represented by the coefficient of $\frac{1}{x}$ in the series for $\log\frac{(x+\alpha)\,(x+\beta)}{x}$, which is

$$\frac{\alpha\,\beta}{\alpha+\beta} + \frac{1\,.\,1}{2\,.\,4}\,.\,\frac{(\alpha\,\beta)^2}{\left(\frac{\alpha+\beta}{2}\right)^3} + \frac{1\,.\,1\,.\,3}{2\,.\,4\,.\,6}\,.\,\frac{(\alpha\,\beta)^3}{\left(\frac{\alpha+\beta}{2}\right)^5} + \&c. \quad (1.)$$

and if we replace α and β by $\frac{1}{\alpha}$ and $\frac{1}{\beta}$, the *least* of the two quantities $\frac{1}{\alpha}$ and $\frac{1}{\beta}$, or the *greatest* of the two quantities α and β, will be represented by

$$\frac{1}{\alpha+\beta} + \frac{1\,.\,1}{2\,.\,4}\,.\,\frac{\alpha\,\beta}{\left(\frac{\alpha+\beta}{2}\right)^3} + \frac{1\,.\,1\,.\,3}{2\,.\,4\,.\,6}\,.\,\frac{(\alpha\,\beta)^2}{\left(\frac{\alpha+\beta}{2}\right)^5} + \&c. \quad (2.)‡$$

* *Transactions of the Philosophical Society of Cambridge*, vol. iv. p. 374.

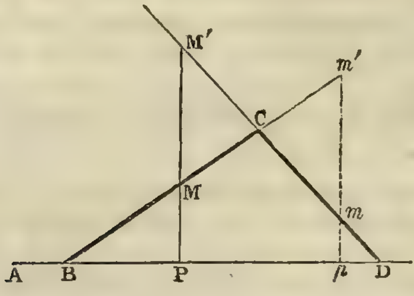
† *Ibid.* p. 125.

‡ If we represent the series (2.) by S, we shall get

$$\frac{d^{n-1}\,S}{(-1)^{n-1}\,\Gamma\,(n)\;d\,\alpha^{n-1}} = \frac{1}{\alpha^n} \text{ or } 0,$$

according as α is greater or less than β: thus $\frac{-d\,s}{d\,\alpha}$ would represent the attraction within and without a spherical shell, which is 0 or $\frac{1}{\alpha^2}$, where α is the distance from the centre.

Thus, if $y - \alpha x - \beta = 0$ and $y - \alpha' x - \beta' = 0$ be the equa-



tions of two lines B C and D C, forming a triangle with a portion B D of the axis of x, then the system of lines which they form will be expressed by the product

$$(y - \alpha x - \beta)\ (y - \alpha' x - \beta') = 0. \qquad (3.)$$

Now it is obvious that if common ordinates P M, P M′ be drawn to the two lines, the *least* of them will belong to the sides of the triangle B C D; if we denote, therefore, P M and P M′ by y_1 and y_2, the equation

$$y = \frac{y_1\, y_2}{y_1 + y_2} + \frac{1\,.\,1}{2\,.\,4} \cdot \frac{(y_1\, y_2)^2}{\left(\frac{y_1 + y_2}{2}\right)^3} + \frac{1\,.\,1\,.\,3}{2\,.\,4\,.\,6} \cdot \frac{(y_1\, y_2)^3}{\left(\frac{y_1 + y_2}{2}\right)^5} + \&c. \ (3.)$$

will become the equation of the sides of the triangle B C D, when y_1 and y_2 are replaced by their values; for y will denote P M for one side and $p\,m$ for the other.

In order to express a discontinuous function φ, which assumes the successive forms φ_1, φ_2, φ_3, &c., for different values of a variable which it involves between the limits α and β, β and γ, γ and δ, &c., Mr. Murphy assumes S $(\alpha_1\, z)$, S $(\beta_1\, z)$, S $(\gamma_1\, z)$, &c., to denote the coefficient of $\frac{1}{x}$ in the several series for

$$\log \frac{(x + \alpha)\ (x + z)}{x},\ \log \frac{(x + \beta)\ (x + z)}{x},\ \&c.,$$

and supposes

$$\varphi = f_1 \,.\, \frac{d\,\mathrm{S}\,(\alpha, z)}{d\,\alpha} + f_2 \,.\, \frac{d\,\mathrm{S}\,(\beta, z)}{d\,\beta} + f_3 \,.\, \frac{d\,\mathrm{S}\,(\gamma, z)}{d\,\gamma}, + \&c.$$

If α be less than z or z greater than α, then S $(\alpha, z) = \alpha$, and therefore $\frac{d\,\mathrm{S}\,(\alpha, z)}{d\,\alpha} = 1$: if β be less than z, then $\frac{d\,\mathrm{S}\,(\beta, z)}{d\,\beta} = 1$: if γ be less than z, then $\frac{d\,\mathrm{S}\,(\gamma, z)}{d\,\gamma} = 1$, and so on; con-

sequently, in the first case we have $\varphi = f_1 = \varphi_1$:
in the second,

$$\varphi = f_1 + f_2 = \varphi_2, \text{ and therefore } f_2 = \varphi_2 - \varphi_1:$$

in the third,

$$\varphi = f_1 + f_2 + f_3 = \varphi_3, \text{ and therefore } f_3 = \varphi_3 - \varphi_2.$$

It appears therefore that

$$\varphi = \varphi_1 \frac{d\,\mathrm{S}(\alpha, z)}{d\,\alpha} + (\varphi_2 - \varphi_1)\frac{d\,\mathrm{S}(\beta, z)}{d\,\beta} + (\varphi_3 - \varphi_2)\frac{d\,\mathrm{S}(\gamma, z)}{d\,\gamma} + \&c$$

is a formula which is competent to express all the required conditions of discontinuity *.

Equivalent forms may be considered as permanent within the limits of continuity, and no further, unless the requisite signs of discontinuity, whether *implicit* or *explicit*, exist upon both sides of the sign $=$: thus, the equation

$${}^x\mathrm{D}_\pi^{\,0} \cos x = \frac{4}{\pi}\left\{\frac{2}{1.3}\sin 2x + \frac{4}{3.5}\sin 4x + \frac{6}{5.7}\sin 6x + \&c.\right\}$$

is permanent within the limits indicated by the sign ${}^x\mathrm{D}_\pi^{\,0}$ and no further, and similarly in most of the cases which have been considered above. The imprudent extension of such equivalent forms, which has arisen from the omission of the necessary signs of discontinuity, has frequently led to very erroneous conclusions: thus, the equation

$${}^x\mathrm{D}_\infty^{\,0} \log x = \frac{1}{2}\left\{\frac{x^2-1}{x^2+1} + \frac{1}{2}\cdot\frac{x^4-1}{(x^2+1)^2} + \frac{1}{3}\cdot\frac{x^6-1}{(x^2+1)^3} + \&c.\right\} \dagger$$

which is true for all values of x between 0 and ∞, has been extended to all values of x between $-\infty$ and $+\infty$, and has thus been made the foundation of an argument for the identity of the logarithms of the same number, both when positive and negative.

There are two species of discontinuity which we have considered above, one of which may be called *instantaneous* and the other *finite*: the first generally accompanies such changes of form as are consequent upon the introduction of critical values

* These formulæ would require generally a correction at their limits, in order to render them symbolically general. The nature of these corrections may in most cases be easily applied from the observations which we have made above.

† This series is given by M. Bouvier in the 14th volume of Gergonne's *Annales des Mathématiques*. The conclusion referred to in the text assumes the identity of the logarithms of x^2 and of $(-x)^2$, which is in fact the whole question in dispute.

of the variables, when the corresponding equivalent form no longer exists, or when the conditions which determined its existence no longer apply; the second restricts the existence of the equivalent form to limits of the variable which have a finite difference from each other. In neither case, if we suppose the conditions of the discontinuity to be implicitly involved, or if we suppose the explicit signs of discontinuity to be assumed conformably to the general laws of algebra, can we consider the law of the permanence of equivalent forms to be violated. It is only when a continuous formula is assumed to be equivalent to a discontinuous formula, without the introduction of the requisite sign of discontinuity to limit the extent of the continuous formula, that we can suppose this fundamental law to be violated or the asserted equation between such expressions to be false. Many important errors have been introduced into analysis from the neglect of those conditions.

The identity of the values of powers of 1, whose indices are general whole numbers, and also of the sines and cosines of angles which differ from each other by integral multiples of 360°, is a frequent source of error in the generalization of equivalent forms, when the symbols which express those indices or multiples are no longer whole numbers. A very remarkable example of both these sources of error has occurred in the formula

$$\begin{aligned}(2\cos x)^m = {} & \cos m\,(2\,r\,\pi + x) + m\cos(m-2)\,(2\,r\,\pi + x) \\ & + \frac{m\,(m-1)}{1\,.\,2}\cos(m-4)\,(2\,r\,\pi + x) + \&\text{c.} \\ & + \sqrt{-1}\,\{\sin m\,(2\,r\,\pi + x) + m\sin(m-2)\,(2\,r\,\pi + x) \\ & + \frac{m\,(m-1)}{1\,.\,2}\sin(m-4)\,(2\,r\,\pi + x) + \&\text{c.} \qquad (1.)\end{aligned}$$

If we suppose m to be a whole number, this equation degenerates into

$$\begin{aligned}(2\cos x)^m = \cos m\,x + m\cos(m-2)\,x + \frac{m\,(m-1)}{1\,.\,2} \\ \cos(m-4)\,x + \&\text{c.} \qquad (2.)\end{aligned}$$

the series first discovered by Euler, and which he assumed to be true for all values of m. If, however, we should suppose m to be a fraction of the form $\frac{p}{q}$, we should have q values of the first member of the equation (2.), and only one of the second. And if we should confine our attention to the arithmetical value (p) of the first, it would not be equal to the second, unless m was a whole number; for if we should denote the series of cosines

$$\cos (m\, 2\, r\, \pi + x) + m \cos (m - 2) (2\, r\, \pi + x) + \&c., \text{ by } C_r,$$

and the series of sines

$$\sin m (2\, r\, \pi + x) + m \sin (m - 2) (2\, r\, \pi + x) + \&c., \text{ by } S_r,$$

we should find, when $\cos x$ is positive,

$$\rho = \frac{C_r}{\cos 2\, m\, r\, \pi} = \frac{S_r}{\sin 2\, m\, r\, \pi};$$

and when $\cos x$ is negative,

$$\rho = \frac{C_r}{\cos m\ (2\, r + 1)\, \pi} = \frac{S_r}{\sin m\ (2\, r + 1)\, \pi}.$$

It will follow, therefore, that when r is *not* a whole number, ρ will be expressible *indifferently* by a series of cosines or of sines, unless $\cos 2\, m\, r\, \pi = 0$ or $\sin 2\, m\, r\, \pi = 0$, when $\cos x$ is positive, or $\cos m\ (2\, r + 1)\ \pi = 0$, or $\sin m\ (2\, r + 1)\, \pi = 0$, when $\cos x$ is negative.

In a similar manner, assuming

$$X' = 1 - \frac{n^2}{1\,.\,2}\,.\cos^2 x + \frac{n^2\,(n^2 - 2^2)}{1\,.\,2\,.\,3\,.\,4} \cos^4 x$$
$$- \frac{n^2\,(n^2 - 2^2)\,(n^2 - 4^2)}{1\,.\,2 \ldots 6} \cos^6 x + \&c.$$

and

$$X' = n \left\{ \cos x - \frac{(n^2 - 1^2)}{1\,.\,2\,.\,3} \cos^3 x \right.$$
$$\left. + \frac{(n^2 - 1^2)\,(n^2 - 3^2)}{1\,.\,2\,.\,3\,.\,4\,.\,5} \cos^5 x - \&c. \right\}$$

we shall find

$$\cos n\, (2\, r\, \pi + x) = \cos n \left(2\, r + \frac{1}{2}\right) \pi\,.\,X$$
$$+ \cos (n - 1) \left(2\, r + \frac{1}{2}\right) \pi\,.\,X'.$$

If we suppose r to be equal to zero, this equation will become

$$\cos n\, x = \cos \frac{n\, \pi}{2}\,.\ X + \cos \frac{(n - 1)\, \pi}{2}\,.\,X',$$

which is the form which has been erroneously assigned by Lagrange* and Lacroix† as generally true for all values of n.

Many other examples of similar *undulating* functions, ex-

* *Calcul des Fonctions*, chap. xi.

† *Traité du Calcul Diff. et Intég.*, tom. i. p. 264.

pressing the various relations between the cosines and sines of multiple arcs and the powers of simple arcs, whether ascending or descending, have been given by Lagrange * and other writers as general, which are either *degenerate* forms of the correct and more comprehensive equations, or altogether erroneous. Poisson had pointed out some of the inconsistencies to which some of these imperfect equations lead, and had slightly hinted at their cause and their explanation; and the discussion of such cases became soon afterwards a favourite subject of speculation with many writers in the Mathematical Journals of France † and Germany ‡; but the complete theory and correction of these expressions was first given by M. Poinsot in an admirable memoir which was read to the Academy of Sciences of Paris in 1823, and published in 1825. They form a most remarkable example of expressions extremely simple and elementary in their nature, which have escaped from the review and analysis of the greatest of modern analysts, in forms which were not merely imperfect, but in some cases absolutely erroneous.

The difficulties which have presented themselves in the theory of the logarithms of negative numbers, as compared with those of the same numbers with a positive sign, have had a very similar origin. If we consider the signs of quantities as *factors* of their arithmetical values, and if we trace them throughout the whole course of the changes which they undergo, we shall find many examples of results which are *identical* when considered in their final equivalent forms, but which are not in every respect identical when considered with respect to their derivation: thus $(+a)^2$ is identical with $(-a)^2$, when considered in their common result $+a^2$, but not when considered with respect to their derivation. Let us now consider their several logarithms, the common arithmetical value of the logarithm of a being denoted by ρ:

$$\log(+a)^2 = \log(1)^2 a^2 = 4 r \pi \sqrt{-1} + 2\rho \qquad (1.)$$

$$\log(-a)^2 = \log(-1)^2 a^2 = (2r+1)\, 2\pi \sqrt{-1} + 2\rho \qquad (2.)$$

$$\log a^2 = \log 1 \,.\, a^2 = 2 r \pi \sqrt{-1} + 2\rho \qquad (3.)$$

It thus appears that the values of $\log(+a)^2$ and $\log(-a)^2$ are included amongst those of $\log a^2$, but not conversely; and also that the values of $\log(+a)^2$ and $\log(-a)^2$, the arithmetical value being excepted, are not included in each other.

* *Correspondence sur l'Ecole Polytechnique*, tom. ii. p. 212.

† In Gergonne's *Annales des Mathématiques*, tom. xiv. xv. xvi. xvii.

‡ In Crelle's *Journal für die reine und angewandte Mathematik*. Berlin.

Again, if we consider $-a^m$ as originating from $(-1)(+a)^m$, we shall get

$$\log - a^m = (2r + 2mr' + 1)\pi\sqrt{-1} + m\rho^*:$$

if we suppose $m = \frac{1}{2}$, $r = 0$ and $r' = -1$, we shall get

$$\log - \sqrt{a} = \frac{1}{2}\rho = \frac{1}{2}\log a = \log\sqrt{a};$$

or the logarithm of a negative quantity will be identical with the logarithm of the same quantity with a positive sign. In a similar manner, if we suppose $m = \frac{p}{2n}$, where p is prime to n, $r' = -n$ and $r = \frac{p-1}{2}$, then $2r + 2mr' + 1 = 0$, and the corresponding logarithm of $-a^m$ will coincide with the arithmetical logarithm of a^m. We should thus obtain *possible* logarithms of negative numbers in those cases in which we should be prepared to expect them from the ordinary definition† of logarithms.

In the absence of all knowledge of the specific process of derivation of quantities, such as a^m and $-a^m$, we should consider their logarithms as identical with those of 1^m. A and $(-1)\,1^m$. A, where A is the arithmetical value of a^m: and in considering the different orders of logarithms which correspond to the same value of a^m or of $-a^m$, they will be found to differ from each other by the logarithms of 1^m and $(-1)\,1^m$ only, which are $2mr\pi\sqrt{-1}$ and $(2r + 2mr' + 1)\pi\sqrt{-1}$ respectively. The logarithms in question are Napierian logarithms whose base is e. If we should suppose the logarithms to be calculated to any other base, we should replace the Napierian logarithms of 1^m and $(-1)\,1^m$ by the logarithms of those quantities (or signs) multiplied by the *modulus* M: the same remarks will apply to such logarithms which have been made with respect to Napierian logarithms.

The question of the identity of the logarithms of the same number, whether positive or negative, was agitated between Leibnitz and Bernoulli, between Euler and D'Alembert, and has been frequently resumed in later times. The arguments in

* Peacock's *Algebra*, p. 569.

† The logarithm being defined to be the index of the power of a given base which is equal to a given number, it would follow, since $a^{\frac{1}{2}} = \pm n$, that $\frac{1}{2}$ is equally the logarithm of $+n$ and $-n$. The same remark applies to all indices or *logarithms* which are rational fractions with even denominators.

favour of the affirmative of this proposition, which were for the most part founded upon the analytical interpretation of the properties of the hyperbola and logarithmic curve, were not entitled to much consideration, in as much as they were not drawn from an analysis of the course followed in the derivation of the symbolical expressions themselves and from the principles of interpretation which those laws of derivation authorized. A very slight examination of those principles, combined with a reference to those upon which algebraical signs of affection are introduced, will readily show the whole of the very limited number of cases in which such a proposition can be considered to be true *.

* In the 15th volume of the *Annales des Mathématiques* of Gergonne, there is an ingenious paper by M. Vincent on the construction of the logarithmic and other congenerous transcendental curves. Thus, if $y = \epsilon^x$ there will be in the plane of $x\,y$ a continuous branch such as is commonly considered, and a discontinuous branch corresponding to those negative values of y which arise from values of x, which are expressible by rational fractions with even denominators: thus, if we suppose the line between $x = 0$ and $x = 1$ to be divided into an even number $2\,p$ of parts, (where p is an odd number,) the values of x will form a series of fractions,

$$0, \frac{1}{2p}, \frac{1}{p}, \frac{3}{2p}, \frac{2}{p}, \ldots \frac{2p-1}{2p}, 1,$$

which have alternately odd and even denominators, and which correspond therefore to values of y which are alternately single and double. If we may suppose, therefore, a curve to be composed of the successive apposition of points, the complete logarithmic curve will consist of two symmetrical branches, one above and the other below the axis of x, one of which, in corresponding parts of the curve, will have double the number of points with the other. The inferior curve, therefore, may in this sense be considered as *discontinuous*, being composed of an infinite number of *conjugate* points, forming, in the language of M. Vincent, *une branche pointillée*. The same remark applies to other exponential curves, such as the *catenary*, &c.

It was objected to this theory of M. Vincent by M. Stein, another writer in the same journal, that every fractional index in this interval might be converted into an equivalent fraction with an even denominator, which would give a double possible value of the ordinate, which would be different from that given by the fractional index in its lowest terms; and that consequently there would necessarily be a double ordinate for every point of the axis, and therefore also a double number, one positive and the other negative, corresponding to every logarithm. In reply to this objection, it is merely necessary to observe that the values of $a^{\frac{m}{n}}$ and $a^{\frac{mp}{np}}$ or of $1^{\frac{m}{n}}$ and $1^{\frac{mp}{np}}$ are in every respect identical with each other, the $n\,p$ values in the second case consisting merely of p periodical repetitions of those in the first.

In a paper in the *Philosophical Transactions* for 1829, Mr. Graves has given a very elaborate analysis of logarithmic formulæ, and has arrived at some conclusions of great generality which it is difficult to reconcile with those which have been commonly received. Amongst some others may be mentioned the formula which he has given for the Napierian logarithms of 1, *Convergency and Divergency of Series.*—The subject of divergent series, their origin, their interpretation and their use in analysis, is one of great importance and great difficulty, and has been and continues to be the occasion of much controversy and doubt. I shall feel it necessary, for such reasons, to notice it somewhat in detail.

If the operations of algebra be considered as general, and the symbols which are subject to them as unlimited in value, it will be impossible to avoid the formation of divergent as well as of convergent series: and if such series be considered as the results of operations which are definable, *apart* from the series themselves, then it will not be very important to enter into such an examination of the relation of the arithmetical values of the successive terms as may be necessary to ascertain their convergency or divergency; for, under such circumstances, they must be considered as equivalent forms representing their generating function, and as possessing, for the purposes of such operations, equivalent properties. Thus, if they result from the division of the numerator of an algebraical fraction by its denominator, then they will *produce* the numerator when multiplied into the denominator or *divisor*: if they result from the extraction of the square or cube root of an algebraical expression, then their *square* or *cube* will produce that expression; and similarly in other cases, *no regard*

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which is not $2 r \pi \sqrt{-1}$, but $\frac{2 r \pi}{2 r' \pi - \sqrt{-1}}$, which, though it includes the former, is not included by it. It appears to me, however, that there exists a fundamental error in the attempt which has been made by Mr. Graves to generalize the ordinary logarithmic formulæ upon the same principles which have been applied by Poinsot to the generalization of the trigonometrical series which have been noticed in the text. He assumes $f(\theta) = \cos\theta + \sqrt{-1}\sin\theta = e^{\theta\sqrt{-1}}$ and makes the series for $f(\theta)$ and $f^{-1}(\theta)$, combined with the equation $f(x\theta) =$ a value of $f(\theta)^x$, and therefore $f^{-1} f \theta = 2 r \pi + \theta$, the foundation of his logarithmic developements: in other words, he makes $e^{\sqrt{-1}}$ a *periodic* quantity the *base* of his system of logarithms, an assumption which is essential to the truth of the formula $f^{-1} f \theta = 2 r \pi + \theta$ and to the generalization of the series for $f^{-1}\theta$ by means of it; an hypothesis which is altogether at variance with our notions of logarithms as ascertained by the ordinary definition. The logarithms of ± 1 and of $(\pm 1)^m$ alone, for very obvious reasons, can be considered as possessing such a character.

Though I have felt myself called upon to state my objections to the fundamental principle assumed in this memoir of Mr. Graves, and consequently to many of the conclusions which are founded upon it, yet I think it right at the same time to observe that it displays great skill and ingenuity in the conduct of the investigations, and is accompanied by many valuable and original observations upon the general principles of analysis.

being paid in such cases to terms which are at an infinite distance from the origin.

It is this last condition, which, though quite indispensable, is rather calculated to offend our popular notions of the values of series as exhibited in their sums. We speak of series as having *sums* when the arithmetical values of their terms are considered, and when the actual expression for the sum of n terms does not become infinite when n is infinite, or when, in the absence of such an explicit expression, we can show from other considerations that its value is finite. In all other cases the series, arithmetically speaking, may be considered as divergent, and therefore as having no *sum* *, if the word *sum* be used in an arithmetical sense only, as distinguished from generating function.

We are in the habit of considering quantities which are *infinitely great* and *infinitely little* as very differently circumstanced with respect to their relation to finite magnitude. We at once identify the latter with *zero*, of which we are accustomed to speak as if it had a real existence; but if we subject our ideas of *zero* and *infinity* to a more accurate analysis, we shall find that it is equally impossible for us to conceive either one or the other as a real state of existence to which a magnitude can attain or through which it can pass. But it is the relation which magnitudes in their finite and conceivable state still bear to other magnitudes in their course of continued increase or continued diminution, which enables us to consider their symbolical relations when they cease to be finite; and whilst quantities infinitely little are neglected as being absorbed in a finite magnitude, so likewise finite magnitudes are considered as being absorbed in *infinity*, and therefore neglected when considered with relation to it. The principle, therefore, of neglecting terms beyond a finite distance from the origin, in converging series, is both safe and intelligible, whilst the case is very different with respect to the neglect of similar terms in a diverging series. Of such series it is said that they have no arithmetical sum; but it may be said in the same sense of all algebraical series involving general symbols that they have no *sum*. But it is not the business of symbolical algebra to deal with arithmetical values, but with symbolical results only; and such series must be considered with reference to the functions which generate them, and the laws of the operations employed for that purpose. The neglect, therefore, of terms beyond a

* This would appear Cauchy's view of the subject: see the 6th chapter of his *Cours d'Analyse*.

finite distance from the origin would be perfectly safe as far as it does not influence the determination of the series from the generating function, or the generating function from the series; and it is upon this principle that the practice is both founded and justified. A few examples may make this reasoning more plain.

Let it be required to determine the function which generates the series

$$a + a\,x + a\,x^2 + a\,x^3 + \&c. \qquad (1.)$$

Let s be taken to represent this function, and therefore

$$\begin{aligned} s &= a + a\,x + a\,x^2 + a\,x^3 + \&c. \\ &= a + x\,\{a + a\,x + a\,x^2 + a\,x^3 + \&c.\} \\ &= a + x\,s: \text{ consequently} \end{aligned}$$

$$s = \frac{a}{1 - x}.$$

If the arithmetical values of the terms of this series be considered, and if x be less than 1, then $\frac{a}{1-x}$ is the *sum* of the series: in all other cases it is its generating function.

We may consider, however, s (whether it expresses a *sum* or a generating function) as identical with s_1, s_2, s_3, &c., in the several expressions

$$\begin{aligned} s &= a + x\,s_1 \\ s &= a + a\,x + x^2\,s_2 \\ s &= a + a\,x + a\,x^2 + x^3\,s_3 \\ s &= a + a\,x + a\,x^2 + \ldots a\,x^{m-1} + x^m\,s_m: \end{aligned}$$

for if the number of terms of the series s be expressed by n and if n be infinite, we must consider $s_1, s_2, s_3, a \ldots s_m$ as absolutely *identical* expressions; for otherwise we must consider an *infinite* as possessing the properties of an *absolute* number, and must cease to regard *infinities* with finite differences as identical quantities when compared with each other. It is for this reason that we assume it as a principle that no regard must be paid to terms at an infinite distance from the origin, whatever their arithmetical values may be.

The sum of the series

$$a - a + a - a + \&c.$$

was assigned by Leibnitz, upon very singular metaphysical considerations, to be $\frac{a}{2}$: the principle just stated would allow us to put

$$s = a - (a - a + a - a + \&c.)$$
$$= a - s;\ \text{and therefore } s = \frac{a}{2}*.$$

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* The same principle would show that the equation

$$x = a + f\Big(a + f\big(a + f(a + \ldots)\big)\Big)$$

is identical with the equation

$$x = a + f(x);$$

and that

$$x = af\Big(af\big(af(a\ldots)\big)\Big)$$

is identical with

$$x = af(x).$$

The example in the text is the most simple case of a class of periodic series, the determination of whose sums to infinity has been the occasion of much controversy and of many curious researches. The general property of such series is the perpetual recurrence of the same group of terms whose sum is equal to *zero*: thus, if there should be p terms in each group, and if the number of terms $n = m\,p + i$, their sum would be identical with that of the i first terms of the series; and if we should denote those terms by $a_1, a_2, \ldots a_p$, and if we should take the successive values of this sum for all the values of i between 1 and p inclusive, their aggregate value would be represented by

$$p\,a_1 + (p-1)\,a_2 + (p-2)\,a_3 + \ldots a_p,$$

of which the average (A) or mean would be represented by

$$\frac{p\,a_1 + (p-1)\,a_2 + (p-2)\,a_3 + \ldots a_p}{p}.$$

If this periodic series was continued to infinity, it was contended by Daniel Bernoulli, in memoirs in the 17th and 18th volumes of *Novi Commentarii Petropolitani,* for 1772 and 1773, that its sum would be correctly represented by the average (A), in as much as it was equally probable that any one of the p values would be the true one. Upon this principle it would follow, that of the apparently identical series

$$1 - 1 + 1 - 1 + 1 - \&c.\ \ldots$$
$$1 + 0 - 1 + 1 + 0 - 1 + 1 + \&c.\ \ldots$$
$$1 + 0 + 0 - 1 + 1 + 0 + 0 - 1 + \&c.$$

the first would be equal to $\frac{1}{2}$, the second to $\frac{2}{3}$, and the third to $\frac{3}{4}$. In the same manner we should find

$$1 + 1 - 1 - 1 + 1 + 1 - 1 - 1 +, \&c.$$

equal to 1, and

$$1 + 1 + 0 - 1 - 1 + 1 + 1 + 0 - 1 - 1 + 1 + 1 + \&c.$$

equal to $\frac{6}{5}$. The same observations would apply to the series

$$1 + \cos x + \cos 2x + \cos 3x + \cos 4x + \&c.$$

and

$$1 + \cos x + 0 + \cos 2x + \cos 3x + 0 + \cos 4x + \&c.$$

where x is commensurable with 2π.

These conclusions, however, though curious and probable, rested upon no

If we consider this principle of the identity of series, whose terms within a finite distance from the origin are identical, as established, we shall experience no difficulty in admitting the perfect algebraical equivalence of such series, and their gene-

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secure basis founded upon the general principles of analysis, and their truth was not, therefore, generally admitted amongst mathematicians. In the year 1798, Callet, the author of the logarithmic tables which go by his name, presented a memoir to the Institute for the purpose of showing that the sums of such periodic series were really indeterminate: thus, if we divide 1 by $1 + x$ and subsequently make $x = 1$, we get

$$1 - 1 + 1 - 1 + \&c. \qquad (1.)$$

the value of which is $\frac{1}{2}$. In a similar manner, if we divide $1 + x$ by $1 + x + x^2$, we get for the quotient

$$1 - x^2 + x^3 - x^5 + x^6 - x^8 + \&c.,$$

which becomes the same series (1.), though the value of the generating function under the same circumstances becomes $\frac{2}{3}$. The same remark applies to the result of the division of $1 + x + x^2 + \ldots x^m$ by $1 + x + x^2 + \ldots x^n$, which produces the same series (1.) when $x = 1$, though under such circumstances its generating function becomes $\frac{m}{n}$.

This memoir of Callet gave occasion to a most elegant Report upon this delicate point of analysis by Lagrange, who justified upon very simple principles the conclusion of Daniel Bernoulli. The series which results from the division of $1 + x$ by $1 + x + x^2$, if the deficient terms be replaced, becomes

$$1 + 0 . x - x^2 + x^3 + 0 . x^4 - x^5 + x^6 + 0 . x^7 - x^8 + \&c.,$$

which degenerates, when $x = 1$, into the series

$$1 + 0 - 1 + 1 + 0 - 1 + 1 + \&c.,$$

and not into the series (1.). The same remark applies to the series which arises from the division of $1 + x + \ldots x^m$ by $1 + x + \ldots\ldots x^n$, $n > m$; which becomes, when $x = 1$,

$$1 + 0 + 0 + 0 + \&c. \quad - 1 + 0 + 0 + \&c. \quad + 1 + 0 + \&c.,$$

which is equal, by Bernoulli's rule, to $\frac{m}{n}$.

But it is not necessary to resort to this expedient for the purpose of determining the sums of such series; for the series

$$a_1 + a_2 x + a_3 x^2 + \quad \ldots a_p x^{p-1} + a_1 x^p + \&c.$$

is a recurring series resulting from the developement of

$$\frac{a_1 + a_2 x + a_3 x^2 + \ldots a_p x^{p-1}}{1 - x^p},$$

which becomes $\frac{0}{0}$ when $x = 1$. If we replace x by $\frac{1}{z}$, this fraction will become rating functions. For the same principle would justify us in rejecting remainders after an infinite number of terms, whatever their arithmetical values may be; for such remainders can influence no terms at a finite distance from the origin, and therefore can in no respect affect any reverse operation, by which it may be required to pass from the series to any expression dependent upon the generating function. Thus, if

$$\frac{a}{1-x} = a + a\,x + a\,x^2 + \&c. \ldots\ldots = s,$$

we shall get

$$a = (1 - x)\, s = a,$$

if we reject remainders after an infinite number of terms; and similarly in other cases. It would thus appear that algebraical equivalence is not necessarily dependent upon the arithmetical equality of the series and its generating function.

It is, however, an inquiry of the utmost importance to be able to ascertain when this arithmetical equality exists; or, in other words, to ascertain under what circumstances we can determine the *sum* of the series, either from our knowledge of the law of formation of its successive terms, or approximate, to any required

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$$\frac{a_1 z^p + a_2 z^{p-1} + \ldots a_p z}{z^p - 1},$$

which becomes by the application of the ordinary rule of the differential calculus, when $z = 1$ or $x = 1$,

$$\frac{p\, a_1 + (p-1)\, a_2 + \ldots\ a_p}{p},$$

which is the average or mean value determined by Bernoulli's rule.

The discussion of the values of these periodic series has been resumed by Poisson in the twelfth volume of the *Journal de l'Ecole Polytechnique.* He considers them as the limits of these series when considered as converging series, a view of their origin and meaning which is almost entirely coincident with that of Lagrange. Thus, the sum of the series

$$\sin q + p \sin (x + q) + p^2 \sin (2\,x + q) + \&c.$$

is equal to

$$\frac{\sin q + p \sin (x - q)}{1 - 2\,p \cos x + p^2},$$

when p is less than 1, an expression which degenerates, when $p = 1$, into

$$\frac{1}{2} \sin q + \frac{1}{2} \cos q \cot \frac{x}{2},$$

which may be considered, therefore, as the limit of the sum of the series

$$\sin q + \sin (x + q) + \sin (2\,x + q) + \&c.\ \textit{in infin.}$$

degree of accuracy, to its value by the aggregation of a finite number of those terms. Many tests of the *summability* of series (considered as different from the determination of their generating functions,) have been proposed, possessing very different degrees of certainty and applicability. The geometrical series which we have just been considering is *convergent* or *divergent*, that is, *summable* or *not*, according as x is greater or less than 1; and it is convenient, for this and for other reasons, to assume it as the measure of the convergency or divergency of other series. If it can be shown that a *converging* geometrical series can be formed whose terms within a finite and assignable distance from the origin become severally greater than those corresponding to them of the assigned series, then that series is *convergent*. And if it can be shown that a *divergent* geometric series can be formed whose terms within a finite and assignable distance from the origin are severally less than those corresponding to them of the assigned series, then that series is divergent*. Such tests are certain, as far as they are applicable; but there may be many cases, both of divergent and convergent series, which they are not sufficiently delicate to comprehend.

It would appear from the preceding observations that diverging series have no arithmetical sums, and consequently

* Peacock's *Algebra*, Art. 324, and following. Cauchy, *Cours d'Analyse Algébrique*, chap. vi. This last work contains the most complete examination of the *tests of convergency* with which I am acquainted.

The measure of convergency mentioned in the text, which was first suggested and applied by D'Alembert, will immediately lead to the following:

"If u_n represent the n^{th} term of a series, it is *convergent* (or will become so) if the superior limit of $(u_n)^{\frac{1}{n}}$ be less than 1, when n is infinite; *divergent* in the contrary case."

"If the limit of the ratio u_{n+1} to u_n be less than 1, the series is *convergent*, and *divergent* in the contrary case."

Many other consequences of these and other tests are mentioned by Cauchy in the work above referred to.

M. Louis Olivier, in the second volume of *Crelle's Journal*, has proposed the following test of convergency. "If the limit of the value of the product $n\,u_n$ be *finite* or *zero* when n is infinite, then the series is *divergent* in the first case, and *convergent* in the second." This principle, however, though apparently very simple and elementary, has been shown by Abel, in the same Journal, to be not universally true. Thus, the series

$$\frac{1}{2\log 2} + \frac{1}{3\log 3} + \frac{1}{4\log 4} + \dots + \frac{1}{n\log n} + \dots$$

may be shown to be infinite, though the product $n\,u_n$ is equal to *zero* when n is infinite. The same acute and original analyst has shown that there is no function of n whatever which multiplied into u_n will produce a result which is zero or *finite* when n is infinite, according as the series is *convergent* or *divergent*.

admit of no arithmetical interpretation. And it will be afterwards made to appear that such series do not include in their expression, at least in many cases, all the algebraical conditions of their generating functions. Before we proceed, however, to draw any inferences from this fact, it may be expedient in the first instance to give a short analysis of some of the circumstances in which such series originate.

The series

$$\frac{1}{a-b} = \frac{1}{a} + \frac{b}{a^2} + \frac{b^2}{a^3} + \text{\&c.}$$

is convergent or divergent according as a is greater or less than b. As this series is incapable, from its form, of receiving a change of sign corresponding to a change in the relation of a and b to each other, it would evidently be *erroneous* in the latter case if it admitted of any arithmetical value, in as much as it would then be equivalent to a quantity which is no longer arithmetical. In this case, therefore, the series may be replaced by the symbol ∞, which is the proper *sign* of *transition*, (see page 237,) which indicates a change in the constitution of the generating function, of such a kind as to be incapable of being expressed by the series which is otherwise equivalent to it. The same observations apply to the equation

$$(a-b)^n = a^n \left\{1 - n\frac{b}{a} + \frac{n(n-1)}{1 \cdot 2} \cdot \frac{b^2}{a^2} - \frac{n(n-1)(n-2)}{1 \cdot 2 \cdot 3} \cdot \frac{b^3}{a^3} + \text{\&c.}\right\},$$

as we have already stated in our remarks upon signs of transition, in page 237. It will be extremely important, however, to examine, both in this and in other cases, the circumstances which attend the transition from generating functions to their equivalent series, in as much as they will serve to explain some difficulties which have caused considerable embarrassment.

The two series

$$\frac{1}{(a-b)^2} = \frac{1}{a^2}\left\{1 + \frac{2b}{a} + \frac{3b^2}{a^2} + \frac{4b^3}{a^3} + \text{\&c.}\right\}$$

and

$$\frac{1}{(b-a)^2} = \frac{1}{b^2}\left\{1 + \frac{2a}{b} + \frac{3a^2}{b^2} + \frac{4a^3}{b^3} + \text{\&c.}\right\}$$

will be divergent in one case, and convergent in the other, whatever be the relation of a and b, though they both equally represent $\frac{1}{a^2 - 2\,a\,b + b^2}$ and $\frac{1}{b^2 - 2\,a\,b + a^2}$, which are algebraically, as well as arithmetically, equivalent to each other. It might be contended, therefore, that in this instance the sign ∞, which replaces one of the two series, is no indication of a change in the constitution of the generating function which is consequent upon a change of the sign of $a - b$ or $b - a$. But though $a^2 - 2\,a\,b + b^2$ is equal to $(a - b)^2$, and $b^2 - 2\,a\,b + a^2$ to $(b - a)^2$; and though $a^2 - 2\,a\,b + b^2$ is identical in value and signification with $b^2 - 2\,a\,b + a^2$ when they are considered without reference to their origin, yet we should not, on that account, be justified in considering $(a - b)^2$ and $(b - a)^2$ as algebraically identical with each other. The first is equal to $(+1)^2 (a - b)^2$, and the second to $(-1)^2 (a - b)^2$; or the first to $(-1)^2 (b - a)^2$, and the second to $(+1)^2 (b - a)^2$. But the signs $(+1)^2$ and $(-1)^2$ are not algebraically identical with each other, though identical when considered in their common result, in as much as their square and other roots and logarithms are different from each other*. It follows, therefore, that there is a symbolical change in the quantity denoted by $\frac{1}{(a - b)^2}$ in its passage through infinity, which is indicated by the *infinite* value of the equivalent series, in as much as it is not competent to express, in its developed form, the algebraical change which its generating function has undergone. The same remarks will apply to the series for $(a - b)^n$ and $(b - a)^n$, in all cases in which n is a negative even number. When n is a negative odd number, the change of constitution of the generating function is manifest, and requires no explanation.

The two series

$$\frac{1}{a + b} = \frac{1}{a}\left\{1 - \frac{b}{a} + \frac{b^2}{a^2} - \frac{b^3}{a^3} + \frac{b^4}{a^4} - \&c.\right\}$$

and

$$\frac{1}{b + a} = \frac{1}{b}\left\{1 - \frac{a}{b} + \frac{a^2}{b^2} - \frac{a^3}{b^3} + \frac{a^4}{b^4} - \&c.\right\}$$

correspond to the same generating function, though one of them is divergent, and the other convergent. But the divergent series, whose terms are alternately positive and negative, cannot be replaced by the symbol ∞, in as much as it does not indicate

* Thus, if a denote a line, $(+a)^2$ and $(-a)^2$ can only be considered as identical in their common result a^2. When $(+a)^2$ and $(-a)^2$ are considered with reference to each other, they are not identical quantities, though equal to each other.

any change in the constitution of the generating function. They may both of them, therefore, be considered as representing the value of this function, though in one case only can we approximate to its arithmetical value by the aggregation of any number of its terms *.

Similar observations would apply to the series

$$(a + b)^n = a^n \left\{ 1 + \frac{n\,b}{a} + \frac{n\,(n-1)}{1\,.\,2} \cdot \frac{b^2}{a^2} + \&c. \right\}$$

when n is not a positive whole number. In all such cases, the developement will sooner or later become a series, whose terms are alternately negative and positive, and which will be divergent or convergent, according to the relation of a and b to each other. More generally we might assume it as a general proposition, "that divergent series which correspond to no change in the constitution of the generating function, will have their terms or groups of terms alternately positive and negative:" and conversely, "that divergent series which correspond to a change in the constitution of the generating function, will have all their terms or groups of terms affected with the same sign, whether + or −, and the whole series may be replaced by the symbol ∞."

In both these propositions the change of which we speak is that which corresponds to those values of the symbols which convert the equivalent series from convergency to divergency, and conversely.

I am not aware of any proof of the truth of these important propositions which is more general than that which is derived from an induction founded upon an examination of particular cases. But such or similar conclusions might be naturally expected to follow from the fundamental principles and assumptions of symbolical algebra. If the rules of algebra be perfectly general, all symbolical conclusions which follow from them must be equally true: and those rules have been so assumed, that when the symbols of algebra represent arithmetical quantities, the operations with the same names represent arithmetical operations, and become symbolical only when the corresponding arithmetical operations are no longer possible. It will be essential, therefore, to the perfection of algebraical language that it should be competent to express fully its own limitations.

* The equations $s = \frac{1}{a} - \frac{b\,s}{a}$ and $s = \frac{1}{b} - \frac{a\,s}{b}$ will equally give us $s = \frac{1}{a+b}$ in one case, and $s = \frac{1}{a+b}$ in the other, whatever be the relation of and b.

Such limitations will be conveyed by the introduction of signs of affection, of signs of transition, or of signs of discontinuity, which may be involved either implicitly or explicitly. It is for such reasons that all those signs must be considered in the interpretation of algebraical formulæ, and their occurrence will at once suggest the necessity of such an examination of the circumstances of their introduction as may be required for their correct explanation*.

We thus recognise two classes of diverging series, which are distinct in their origin and in their representation. The first may be considered as involving the symbol or sign ∞ implicitly, and as capable, therefore, of the same interpretation as we give to the sign when it presents itself explicitly. The second represents finite magnitudes, which in their existing form are incapable of calculation by the aggregation of any number of their terms. Such series are in many cases capable of transformations of form, which convert them into equivalent converging series; and in some cases, where such a transformation is not practicable, or is not effected, the approximate values of the generating functions may be determined, from indirect considerations, supplied by very various expedients.

The well known transformation of the series

$$a x - b x^2 + c x^3 - d x^4 + e x^5 - f x^6 + \&c.,$$

which Euler has given†, into the equivalent series

$$\frac{x}{1+x} a - \frac{x^2}{(1+x)^2} . \Delta a + \frac{x^3}{(1+x)^3} . \Delta^2 a - \frac{x^4}{(1+x)^4} . \Delta^3 a + \&c.$$

would be competent to convert a great number of divergent series of the second class into equivalent convergent series, or into such as would become so. In this manner the Leibnitzian series

$$1 - 1 + 1 - 1 + \&c.$$

may be shown to be equal to $\frac{1}{2}$. The series

$$1 - 3 + 6 - 10 + 15 - 21 + \&c.$$

* The essential character of arithmetical division is that the quotient should approximate continually to its true value, and that the terms of the quotient which are introduced by each successive operation should be less and less continually. In the formation, therefore, of the quotient of $\frac{1}{a-b}$ and $\frac{1}{a+b}$ the analogy between the arithmetical and algebraical operation would cease to exist, unless a was greater than b, or unless the several terms in the quotient went on diminishing continually.

† *Institutiones Calculi Differentialis*, Pars posterior, cap. i.

of triangular numbers to $\frac{1}{3}$. The series

$$1 - 4 + 9 - 16 + 25 - \&c.$$

of square numbers to 0. The series of tabular logarithms

$$\log 2 - \log 3 + \log 4 - \log 5 + \&c.,$$

would be found to be equal to ·0980601 nearly. If we should suppose x negative and greater than 1, the original and the transformed series would become divergent series of the first class.

The series

$$\log a = (a-1) - \frac{(a-1)^2}{2} + \frac{(a-1)^3}{3} - \frac{(a-1)^4}{4} + \&c.$$

is divergent when a is greater than 2, and convertible by Euler's formula into the convergent series

$$\frac{(a-1)}{a} + \frac{1}{2}\cdot\frac{(a-1)^2}{a^2} + \frac{1}{3}\cdot\frac{(a-1)^3}{a^3} + \frac{1}{4}\,\frac{(a-1)^4}{a^4} + \&c.;$$

or by the method of Lagrange into the series

$$n\,(\sqrt[n]{a} - 1) - \frac{n}{2}\,(\sqrt[n]{a} - 1)^2 + \frac{n}{3}\,(\sqrt[n]{\ } - 1)^3 - \&c.,$$

which may be made to possess any required degree of convergency. But it is not necessary to produce further examples of such transformations, which embrace a very great part of the most refined artifices which have been employed in analysis.

One of the most remarkable of these artifices presents itself in a series to which Legendre has given the name of *demiconvergent**. The factorial function $\Gamma(1 + x)$ is expressed by the *continuous* expression

$$\left(\frac{x}{e}\right)^{x} (2\,\pi\,x)^{\frac{1}{2}}\,\mathrm{R},$$

where R is a quantity whose Napierian logarithm is expressed by

$$\frac{\mathrm{A}}{1\,.\,2\,.\,x} - \frac{\mathrm{B}}{3\,.\,4\,.\,x^2} + \frac{\mathrm{C}}{5\,.\,6\,.\,x^4} - \&c.,$$

where A, B, C, &c., are the *numbers* of Bernoulli. The law of formation of these numbers, as is well known, is extremely

* *Fonctions Elliptiques*, tom. ii. chap. ix. p. 425.

irregular, and after the third term they increase with great rapidity. The series under consideration, therefore, even for considerable values of x, becomes divergent after a certain number of terms. But an approximate value of the series will be obtained from the aggregation of the *convergent terms only*: and it has been proved by a German analyst* that the error which is thus made in the value of the generating function will in this case be less than the last of the convergent, or the first of the divergent, terms.

It has been usual amongst some later mathematicians of the highest rank to denominate diverging series, without any distinction of their class, as *false*, not merely when arithmetical values are considered, but also when employed as equivalent forms, in purely symbolical processes. The view of their origin and nature which we have taken above would explain the sense in which they might be so considered in relation both to arithmetical processes and to the calculation of arithmetical values. It seems, however, an abuse of terms to apply the term *false* to any results which necessarily follow from the laws of algebra. M. Poisson, perhaps the most illustrious of living analysts, has referred, in confirmation of this opinion, to some examples of erroneous conclusions produced through the medium of divergent series†; and as the question is one of great importance and of great difficulty, I shall venture to notice them in detail.

Let it be required to express the value of

$$z = \int_{-1}^{+1} \frac{dx}{\{(1 - 2ax + a^2)(1 - 2bx + b^2)\}^{\frac{1}{2}}}$$

by means of series.

Assuming $K = (1 - 2ax + a^2)^{-\frac{1}{2}}$ and $K' = (1 - 2bx + b^2)^{-\frac{1}{2}}$, let us suppose K and K′ developed according to ascending and descending powers of a and b respectively; or,

$$\begin{cases} K = 1 + a X_1 + a^2 X_2 + a^3 X_3 + \&c. \\ K' = 1 + b X_1 + b^2 X_2 + b^3 X_3 + \&c. \end{cases}$$

$$K = \frac{1}{a} + \frac{1}{a^2} X_1 + \frac{1}{a^3} X_2 + \frac{1}{a^4} X_3 + \&c.$$

$$K' = \frac{1}{b} + \frac{1}{b^2} X_1 + \frac{1}{b^3} X_2 + \frac{1}{b^4} X_3 + \&c.$$

* Erchinger in Schrader's *Commentatio de Summatione Seriei, &c.* Weimar 1818.

† *Journal de l'Ecole Polytechnique*, tom. xii.

The coefficients X_1, X_2, X_3, &c., are *reciprocal** functions, possessing the following remarkable property, that $\int_{-1}^{+1} X_m X_n \, d x = 0$, in all cases, unless $n = m$, in which case $\int_{-1}^{+1} X_n X_n \, d x = \frac{1}{2n+1}$.

The knowledge of this property will readily enable us to determine the following four different values of z:

$$z_1 = 1 + \frac{a b}{3} + \frac{a^2 b^2}{5} + \frac{a^3 b^3}{7} + \&c.$$

$$z_2 = \frac{1}{b} + \frac{a}{3 b^2} + \frac{a^2}{5 b^3} + \frac{a^3}{7 b^4} + \&c.$$

$$z_3 = \frac{1}{a} + \frac{b}{3 a^2} + \frac{b^2}{5 a^3} + \frac{b^3}{7 a^4} + \&c.$$

$$z_4 = \frac{1}{a b} + \frac{1}{3 a^2 b^2} + \frac{1}{5 a^3 b^3} + \frac{1}{7 a^4 b^4} + \&c.$$

Whatever be the relation of a and b to each other and to 1, two of these four series are convergent, and two of them divergent. But it appears from the examination of the finite integral $\int_{-1}^{+1} \mathrm{K} \, \mathrm{K}' \, d x$, that one only of these two convergent series gives the correct value of z, being that which arises from the combination of the *two convergent* developements of K and K′, whilst the incorrect value arises from the combination of a convergent developement of K with a divergent developement of K′, or conversely. The conclusion which is drawn from this fact is, that the introduction of the divergent developement of K or of K′ *vitiates* the corresponding value of z, even though that value is expressed by a convergent series. Let us now examine how far the definite integral of $\int_{-1}^{+1} \mathrm{K} \, \mathrm{K}' \, d x$ will justify such an inference.

If we denote K K′ by $\frac{1}{p}$, we shall easily find,

* Functions which possess this property have been denominated *reciprocal* functions by Mr. Murphy, in a second memoir on the Inverse Method of Definite Integrals, in the fifth volume of the *Transactions of the Philosophical Society of Cambridge*, in which general methods are given for discovering all species of such functions, and where one very remarkable form of them is assigned. The functions referred to in the text were first noticed by Legendre, in his first memoir on the Attraction of Ellipsoids, and subsequently, at great length, in the Fifth Part of his *Exercices du Calcul Intégral*. Cauchy has used the term *reciprocal* function in a different sense; see *Exercices des Mathématiques*, tom. ii. p. 141.

$$\int_{-1}^{+1} \frac{d\,x}{p} = \frac{1}{2\sqrt{a\,b}} \log \left(\frac{p\,d\,p}{d\,x} + 2\,p\sqrt{a\,b}\right) + \text{const.};$$

and if we denote by r and r' the extreme values of p, when $x = -1$ and $x = +1$, we shall find,

$$z = \int_{-1}^{+1} \frac{d\,x}{p} = \frac{1}{4\sqrt{a\,b}} \log \left\{\frac{2\,r\sqrt{a\,b} + 4\,a\,b - (a+b)(1+a\,b)}{2\,r'\sqrt{a\,b} - 4\,a\,b - (a+b)(1+a\,b)}\right\},$$

in as much as $\frac{p\,d\,p}{d\,x}$ is $4\,a\,b - (a + b)(1 + a\,b)$ in one case, and $-4\,a\,b - (a + b)(1 + a\,b)$ in the other. It will appear likewise that r and r' will have the same sign, whether $+$ or $-$, in as much as p will preserve the same sign throughout the whole course of the integration. If, therefore, $r' = +(1 + a)(1 + a)$, then $r = +(1 - a)(1 - b)$; and if $r' = -(1 + a)(1 + b)$, then $r = -(1 - a)(1 - b)$. It thus appears that $(1 - a)(1 - b)$ must have the same sign with $(1 + a)(1 + b)$, and consequently if $a > 1$, and $b > 1$, we shall have,

$$z = \frac{1}{4\sqrt{a\,b}} \log . \frac{(a-1)(b-1)\sqrt{a\,b} + 4\,a\,b - (a+b)(1+a\,b)}{(a+1)(b+1)\sqrt{a\,b} - 4\,a\,b - (a+b)(1+a\,b)}$$

$$= \frac{1}{4\sqrt{a\,b}} \log . \frac{(\sqrt{a\,b}+1)^2}{(\sqrt{a\,b}-1)^2} \text{ (striking out the common divisor}$$

$$2\sqrt{a\,b} - a - b) = \frac{1}{2\sqrt{a\,b}} \log \frac{\sqrt{a\,b}+1}{\sqrt{a\,b}-1} = \pm z_4.$$

If $a < 1$ and $b < 1$, we shall find $r = (1 - a)(1 - b)$, and

$$z = \frac{1}{2\sqrt{a\,b}} \log \left(\frac{1 + \sqrt{a\,b}}{1 - \sqrt{a\,b}}\right) = \pm z_1.$$

If $a < 1$ and $b > 1$, we shall find $r = (1 - a)(1 - b)$, and

$$z = \frac{1}{2\sqrt{a\,b}} \log \left(\frac{\sqrt{b} + \sqrt{a}}{\sqrt{b} - \sqrt{a}}\right) = \pm z_2.$$

If $a > 1$ and $b < 1$, we shall find $r = (a - 1)(1 - b)$, and

$$z = \frac{1}{2\sqrt{a\,b}} \log \left(\frac{\sqrt{a} + \sqrt{b}}{\sqrt{a} + \sqrt{b}}\right) = \pm z_3.$$

It would thus appear that the definite integral would furnish erroneous values of z if no attention was paid to those values of the factors of r and r', which the circumstances of the integration require: and it may be very easily shown that an attention to the developements of K and K$'$ will, with equal certainty, enable us to select the proper developement for z. Thus, if $a > 1$

and $b > 1$, we have $r = (a-1)(b-1) = \dfrac{1}{\{(a-1)^2(b-1)^2\}^{-\frac{1}{2}}}$: and the value of z (z_4) is determined by the combination of the two last developements. In a similar manner, if $a < 1$ and $b < 1$, z (z_1) will be formed by the combination of the two first. If $a < 1$ and $b > 1$, then $r = (1-a)(b-1) = \dfrac{1}{\{(1-a)^2(b-1)^2\}^{-\frac{1}{2}}}$: and the value of z (z_2) is formed by the combination of the first and third developement. And if $a > 1$ and $b < 1$, then the value of z (z_3) will be formed by the combination of the second and third developements: in other words, the selection of the developements is not arbitrary, in as much as $\{(1-a)^2\}^{-\frac{1}{2}}$ and $\{(a-1)^2\}^{-\frac{1}{2}}$ ought not to be considered, as we have already shown, as identical quantities.

These combinations of the convergent and divergent series form all the four values of z, of which it appears that one value alone is correct for any assigned relation of a and b to 1, being that which arises from the combination of the convergent series for K and K′ only. The considerations, however, which determine the selection of the correct developement of z are as definite and certain when the general series are employed as when that value is determined directly from the definite integral which expresses the value of z. It would appear to me, therefore, that not only was the employment of divergent series necessary for the determination of *all* the values of z, but that when the theory of their origin is perfectly understood they are perfectly competent to express all the limitations which are essential to their usage. The attempt to exclude the use of divergent series in symbolical operations would necessarily impose a limit upon the universality of algebraical formulæ and operations which is altogether contrary to the spirit of the science, considered as a science of symbols and their combinations. It would necessarily lead to a great and embarrassing multiplication of cases; it would deprive almost all algebraical operations of much of their certainty and simplicity; and it would altogether change the order of the investigation of results when obtained, and of their interpretation, to which I have so frequently referred in former parts of this Report, and upon which so many important conclusions have been made to depend.

Elementary Works on Algebra.—There are few tasks the execution of which is so difficult as the composition of an elementary work; and very few in which, considering the immense number of such works, complete success is so rare. They require, indeed, a union of qualities which the class of writers who usually undertake such works are not often competent to furnish. Great simplicity in the exposition and exemplification of first principles, a perfect knowledge of the consequences to which they lead, and great forbearance in not making them an occasion for the display of the peculiar opinions or original researches of their authors.

There is, in fact, only one elementary work which is entitled to be considered as having made a very near approach to perfection. The *Elements* of Euclid have been the text-book of geometers for two thousand years; and though they labour under some defects, which may or may not admit of remedy, without injury to the body of the work, yet they have not received any fundamental change, either in the propositions themselves, or in their order of succession, or in the principles of their demonstrations, in the propriety of which geometers of any age or country have been found to acquiesce. It is true that both the objects and limits of the science of geometry are perfectly defined and understood, and that systems of geometry must, more or less, necessarily approach to a common arrangement, in the order of their propositions, and to common principles as the bases of their demonstrations. But even if we should make every allowance for the superior simplicity of the truths to be demonstrated, and for the superior definiteness of the objects of the science to be taught, and also for the superior sanction and authority which time and the respect and acceptance of all ages have assigned to this remarkable work, we may well despair of ever seeing any elementary exposition of the principles of algebra, or of any other science, which will be entitled to claim an equal authority, or which will equally become a model to which all other systems must, more or less, nearly approximate.

There are great difficulties in the elementary exposition of the principles of algebra. As long as we confine our attention to the principles of arithmetical algebra, we have to deal with a science all whose objects are distinctly defined and clearly understood, and all whose processes may be justified by demonstrative evidence. If we pass, however, beyond the limits which the principles of arithmetical algebra impose, both upon the representation of the symbols, and upon the extent of the operations to which they are subject, we are obliged to abandon the aid which is afforded by an immediate reference to the sensible objects of our reasoning. In the preceding parts of this Report we have endeavoured to explain the true connexion between arithmetical and symbolical algebra, and also the course which must be followed in order to give to the principles of the latter in their most general form such a character as may be adequate to justify all its conclusions. But the necessity which is thus imposed upon us of dealing with abstractions of a nature so complete and comprehensive, renders it extremely difficult to give to the principles of this science such a form as may bring them perfectly within the reach of a student of ordinary powers, and which have not hitherto been invigorated by the severe discipline of a course of mathematical study.

The range of the science of algebra is so vast, and its applications are so various, both in their objects and in their degrees of difficulty, that it is quite impossible to fix absolutely the proper proportion of space which should be assigned to the developement of its different departments. If a system of algebra could be confined to the statement of fundamental principles, and to the establishment of fundamental propositions only, it might be possible to approximate to a fixed standard, which should possess the requisite union of simplicity and of sufficient generality. But it is a science which cannot be taught by an exposition of principles and their general consequences only, but requires a more or less lengthened institution of examples of many of its different applications, in order to produce in the student mechanical habits of dealing with symbols and their combinations. The extent also to which such developements are necessary will vary greatly with the capacities of different students, and it would be quite impossible to determine any just mean between diffuseness and compression which shall be best adapted to the wants of the general average of students, or to the systems of instruction followed by the general average of teachers.

In the early part of the last century the *Algebra* of Maclaurin was almost exclusively used in the public education of this country. It is unduly compressed in many of its most essential elementary parts, and is also unduly expanded in others which have reference to his own discoveries. It was written, however, in a simple and pure taste, and derived no small part of its authority as a text-book from the great and well-merited reputation of its author. It was subsequently, in a great measure, superseded, in the English Universities at least, by the large work of Sanderson, which was composed by this celebrated teacher to meet the wants of his numerous pupils. It was, in consequence, swelled out to a very unwieldy size by a vast number of examples worked out at great length; and it laboured under the very serious defect of teaching almost exclusively arithmetical algebra, being far behind the work of Maclaurin in the exposition of general views of the science. At the latter end of the last century Dr. Wood, the present learned and venerable master of St. John's College Cambridge, in conjunction with the late Professor Vince, undertook the publication of a series of elementary works on analysis, and on the application of mathematics to different branches of natural philosophy, principally with a view to the benefit of students at the Universities. The works of the latter of these two writers have already fallen into very general neglect, in consequence partly of their want of elegance, and partly in consequence of their total unfitness to teach the more modern and improved forms of those different branches of science. But the works of his colleague in this undertaking have continued to increase in circulation, and are likely to exercise for many years a considerable influence upon our national system of education; for they possess in a very eminent degree the great requisites of simplicity and elegance, both in their composition and in their design. The propositions are clearly stated and demonstrated, and are not incumbered with unnecessary explanations and illustrations. There is no attempt to bring prominently forward the peculiar views and researches of the author, and the different parts of the subjects discussed are made to bear a proper subordination to each other. It is the union of all these qualities which has given to his works, and particularly to his *Algebra*, so great a degree of popularity, and which has secured, and is likely to continue to secure, their adoption as text-books for lectures and instruction, notwithstanding the absence of very profound and philosophical views of the first principles, and their want of adaptation, in many important particulars, to the methods which have been followed by the great continental writers.

In later times a great number of elementary works on algebra, possessing various degrees of merit, have been published. Those, however, which have been written for purposes of instruction only, without any reference to the advancement of new views, either of the principles of the science, or to the extension of its applications, have generally failed in those great and essential requisites of simplicity, and of adequate, but not excessive, illustration, for which the work of Dr. Wood is so remarkably distinguished; whilst other works, which have possessed a more ambitious character, have been generally devoted too exclusively to the developement of some peculiar views of their authors, and have consequently not been entitled to be generally adopted as text-books in a system of academical or national education. There are, however, many private reasons which should prevent the author of this Report from enlarging upon this part of his subject, who is too conscious that there are few defects which he could presume to charge upon the works of other authors from which he could venture to exempt his own.

The elementary works on algebra and on all other branches of analytical and physical science which have been published in France since the period of the Revolution, have been very extensively used, not merely in this country, but in almost every part of the continent of Europe where the French language is known and understood. The great number of illustrious men who took part in the lectures at the Normal and Polytechnic Schools at the time of their first institution, and the enlarged views which were consequently taken of the principles of elementary instruction and of their adaptation to the highest developement of the several sciences to which they lead, combined with the powerful stimulus given to the human mind in all ranks of life, in consequence of the stirring events which were taking place around them, at once placed the scientific education of France immensely in advance of that of the rest of Europe. The works of Lagrange, particularly his *Calcul des Fonctions* and his *Théorie des Fonctions Analytiques*, which formed the substance of lectures given at the *Ecole Polytechnique*, exhibited the principles of the differential and integral calculus in a new light, and contributed, in connexion with his numerous other works and memoirs, which are unrivalled for their general elegance and fine philosophical views, to familiarize the French student with the most perfect forms and with the most correct and at the same time most general principles of analytical science. The labours of Monge also, upon the application of algebra to geometry, succeeded in bringing all the relations of space, with which every department of natural philosophy is concerned, completely under the dominion of analysis *, and thus enabled their elementary and other writers to exhibit the mathematical principles of every branch of natural philosophy under analytical and symmetrical forms. Laplace himself gave lectures on the principles of arithmetic and of algebra, which appear in the *Séances de l'Ecole Normale* and in the *Journal de l'Ecole Polytechnique*; and there are very few of the illustrious men of science, of that or of a subsequent period, who have done so much honour to France, who have not been more or less intimately associated with carrying

* The developement of the details of this most important branch of analytical science, which has been so extensively and successfully cultivated in France, is greatly indebted to Monge's pupils in the Polytechnic School, many of whom have subsequently attained to great scientific eminence: their results are chiefly contained in the three volumes of *Correspondance sur l'Ecole Polytechnique*.

on the business of national education in its highest departments. The influence of such men has been felt not merely in the very general diffusion of scientific knowledge in that great nation, but also in the form and character of their elementary books, which are generally remarkable for their precision and clearness of statement, for their symmetry of form, and for their adaptation to the most extensive developement of the several sciences upon which they treat.

The elementary works of M. Lacroix upon almost every department of analytical science have been deservedly celebrated: they possess nearly all the excellences above enumerated as characteristic of French elementary writers, and they are also remarkable for the purity and simplicity of the style in which they are written *. The *Cours des Mathématiques Pures* of M. Francœur possesses merits of a similar kind, being too much compressed, however, for the purposes of self-instruction, though well adapted to form a basis for the lectures of a teacher. The works of M. Garnier are chiefly valuable for their careful illustration of, and judicious selection from, the writings of Lagrange, and are well calculated to make the general views and principles of that great analyst and philosopher familiar to the mind of a student. The *Arithmetic*, *Algebra* †, and *Application of Algebra to Geometry*, of M. Bourdon are works of more than ordinary merit, and present a very clear and fully developed view of the elements of those sciences. Many other works have been published of the same kind and with similar views by Reynaud, Boucharlat and other writers.

I am too little acquainted with the elementary works which are used in the different Universities of Germany to be able to express any opinion of their character. Those which I have seen have been wanting in that precise and symmetrical form which constitutes the distinguishing merit of the French elementary writers; but they are generally copious, even to excess, in their examples and illustrations. The immense developement which public instruction, in all its departments, has received in that country would lead us to conclude that they possess elementary mathematical works, which are at least not inferior to those which

* Before the Revolution, the *Cours des Mathématiques Pures et Appliquées* of Bezout, in six volumes, was generally used in public education in France: it is a work much superior to any other publication of that period of a similar kind which was to be found in any European language.

† A part of the *Algebra* of Bourdon has been translated and highly commended by Mr. De Morgan, a gentleman whose philosophical work on Arithmetic and whose various publications on the elementary and higher parts of mathematics, and particularly those which have reference to mathematical education, entitle his opinion to the greatest consideration.

exist in other languages: and the labours of Gauss, Bessel, and Jacobi, and the numerous and important memoirs which appear in their public Journals and Transactions upon the most difficult questions of analysis and the physical sciences, sufficiently show that the mathematical literature of this most learned nation is not less diligently and successfully cultivated than that which belongs to every other department of human knowledge.

The combinatorial analysis, which Hindenburg first introduced, has been cultivated in Germany with a singular and perfectly national predilection *; and it must be allowed that it is well calculated to compress into the smallest possible space the greatest possible quantity of meaning. In the doctrine of series it is also frequently of great use, and enables us to exhibit and to perceive relations which would not otherwise be easily discoverable. Without denying, however, the advantages which may attend either the study or the use of the notation of the combinatorial analysis, it may be very reasonably doubted whether those advantages form a sufficient compensation for the labour of acquiring an habitual command over the use and interpretation of a conventional symbolical language, which is necessarily more or less at variance with the ordinary usage and meaning of the symbols employed and of the laws of their combinations. These objections would apply, if such a conventional use of symbolical language was universally adopted and understood; but they acquire a double force and authority, when it appears that they are only partially used in the only country † in which the combinatorial analysis is extensively cultivated, and that, consequently, those works in which it is adopted are excluded from general perusal, in consequence of their not being written in that peculiar form of symbolical language with which our mathematical associations are indissolubly connected.

Trigonometry.—The term Trigonometry sufficiently indicates the primitive object of this science, which was the determination, from the requisite data, of the sides and angles of triangles: it was in fact considered in a great degree as an inde-

* See Eytelwein's *Grundlehre der höhern Analysis*, a very voluminous work, which contains the principal results of modern analysis and of the theory of series exhibited in the language and notation of this analysis.

† Professor Jarrett, of Catherine Hall, Cambridge, in some papers in the *Transactions of the Philosophical Society of Cambridge*, and in a Treatise on Algebraical Developement, has attempted to introduce the use of the language of the combinatorial analysis. The great neglect, however, which has attended those speculations, which are very general and in some respects extremely ingenious, is a sufficient proof of the difficulty of overcoming those mathematical habits which a long practice has generated and confirmed.

pendent science, and not as auxiliary to the application of algebra to geometry. It is to Euler* that we are indebted for the emancipation of this most important branch of analytical science from this very limited application, who first introduced the functional designations sin z, cos z, tan z, &c., to denote the sine, cosine, tangent, &c., of an arc z, whose radius is 1, which had previously been designated by words at length, or by simple and independent symbols, such as a, b, s, c, t, &c. The introduction of this new algorithm speedily changed the whole form and character of symbolical language, and greatly extended and simplified its applications to analysis, and to every branch of natural philosophy.

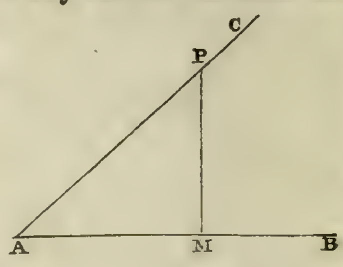
The angles which enter into consideration in trigonometry are generally assumed to be measured by the arcs of a circle of a given radius, and their sines and cosines are commonly defined with reference to the determination of these arcs, and not with reference to the determination of the angles which they measure. It is in consequence of this defined connexion of sines and cosines with the arcs, and not immediately with the angles which they measure, that the radius of the circle upon which those arcs are taken must necessarily enter as an element in the comparison of the sines and cosines of the same angle determined by different measures: and though they were generally, at least in later writers, reduced to a common standard, by assuming the radius of this circle to be 1, yet formulæ were considered as not perfectly general unless they were expressed with reference to any radius whatsoever†. In the application, likewise, of such formulæ to the business of calculation, the consideration of the radius was generally introduced, producing no small degree of confusion and embarrassment; and even in the construction of logarithmic tables of sines and cosines the

* *Introductio in Analysim Infinitorum*, vol. i. cap. viii. "Quemadmodum logarithmi peculiarem algorithmum requirunt, cujus in universa analysi summus extat usus, ita quantitates circulares ad certam quoque algorithmi normam perduxi: ut in calculo æque commode ac logarithmi et ipsæ quantitates algebraicæ tractari possent."—Extract from Preface.

† We may refer to Vince's *Trigonometry*, a work in general use in this country less than a quarter of a century ago, and to other earlier as well as contemporary writers on this subject, for examples of formulæ, which are uniformly embarrassed by the introduction of this extraneous element. Later writers have assumed the radius of the circle to be 1, and have contented themselves with giving rules for the conversion of the resulting formulæ to those which would arise from the use of any other radius. It is somewhat remarkable that the elementary writers on this subject should have continued to encumber their formulæ with this element long after its use had been abandoned by Euler, Lagrange, Laplace, and all the other great and classical mathematical writers on the Continent.

occurrence of negative logarithms was avoided by a *fiction*, which supposed them to be the sines and cosines of arcs of a circle whose radius was 10^{10}.

A very slight modification of the definition of the sine and cosine would enable us to get rid of this element altogether. In a right-angled triangle, the ratio of any two of its sides will determine its *species*, and consequently the magnitude of its angles. If we suppose, therefore, a point P to be taken in one (A C) of the two lines A C and A B containing the angle B A C (θ), and P M to be drawn perpendicular to the other line (A B), then we may define the



sine of θ to be the ratio $\frac{\text{P M}}{\text{A P}}$, and the cosine of θ to be the ratio $\frac{\text{A M}}{\text{A P}}$. By such definitions we shall make the sine and cosine of an angle depend upon the angle itself, and not upon its measure, or upon the radius of the circle in which it is taken: and upon this foundation all the formulæ of trigonometry may be established, and their applications made, without the necessity of mentioning the word *radius**.

If we likewise assume the ratio of the *arc* which subtends an angle to the *radius* of the circle in which it is taken, and not the arc itself, for the measure of an angle, we shall obtain a quantity which is independent of this radius. In assuming, therefore, the angle θ to be not only measured, but also represented by this ratio, we shall be enabled to compare sin θ and cos θ directly with θ, and thus to express one of them in terms of the other. It is this hypothesis which is made in deducing the exponential expressions for the sine and cosine, and the series which result immediately from them†.

* See *A Syllabus of a Course of Lectures upon Trigonometry, and the Application of Algebra to Geometry*, published at Cambridge in 1833, in which all the formulæ of trigonometry are deduced in conformity with these definitions.

† If we should attempt to deduce the exponential expressions for sin θ and cos θ from the system of fundamental equations,

$$\cos^2\theta + \sin^2\theta = 1 \qquad (1.)$$

$$\cos\theta = \cos(-\theta) \qquad (2.)$$

$$\sin\theta = -\sin(-\theta) \qquad (3.)$$

we should find,

$$\cos\theta = \frac{e^{A\theta\sqrt{-1}} + e^{-A\theta\sqrt{-1}}}{2}, \text{ and } \sin\theta = \frac{e^{A\theta\sqrt{-1}} - e^{-A\theta\sqrt{-1}}}{2\sqrt{-1}},$$

in which the quantity A, in the absence of any *determinate* measure of the

The sines and cosines and the measures of angles defined and determined as above, are the only essential elements in a system of trigonometry, and are sufficient for the deduction of all the important formulæ which are required either in algebra

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angle θ, would be perfectly *indeterminate*. It is the *assumption* of the measure of an angle which is mentioned in the text which makes it necessary to replace A by 1.

The knowledge of the exponential expressions for the sine and cosine would furnish us immediately with all the other properties of these transcendents. Thus, if the sines and cosines of two angles be given, we can find the sines and cosines of their sum and difference; and from hence, also, we can find the sine and cosine of any multiple of an angle from the values of the sine and cosine of the simple angle; and also through the medium of the solution of equations the sine and cosine of its submultiples. In fact, as far as the symbolical properties of those transcendents are concerned, it is altogether indifferent whether we consider them to be deduced primarily from the assumed functional equations (1.), (2.), (3.), or from the primitive geometrical definitions of which those equations are the immediate symbolical consequences.

If we should denote the integrals $\int_0^x \frac{dx}{\sqrt{1-x^2}}$ and $\int_0^y \frac{dy}{\sqrt{1-y^2}}$ (commencing from 0 respectively) by θ and θ' respectively, then the integral of the equation

$$\frac{dx}{\sqrt{1-x^2}} + \frac{dy}{\sqrt{1-y^2}} = 0 \qquad (\alpha.)$$

would furnish us with the fundamental equation

$$\sin(\theta + \theta') = \sin\theta \cos\theta' + \cos\theta \sin\theta', \qquad (\beta.)$$

if we should replace x by $\sin\theta$, $\sqrt{1-x^2}$ by $\cos\theta$, y by $\sin\theta'$, and $\sqrt{1-y^2}$ by $\cos\theta'$. If the formulæ of trigonometry were founded upon such a basis, they would require no previous knowledge either of circular arcs considered as the measures of angles, or of the geometrical definitions of the sines and cosines, except so far as they may be ascertained from the examination of the values and properties of the transcendents which enter into the equation (α.). In a similar manner, if we should suppose θ and θ' to represent the integrals of the transcendents $\int_0^x \frac{dx}{\sqrt{(1+x^2)}}$ and $\int_0^y \frac{dy}{\sqrt{(1-y^2)}}$, then the integral of the equation

$$\frac{dx}{\sqrt{(1+x^2)}} + \frac{dy}{\sqrt{(1+y^2)}} = 0 \qquad (\gamma.)$$

would be expressed by the equation

$$h\sin(\theta + \theta') = h\sin\theta \times h\cos\theta' + h\cos\theta \times h\sin\theta', \qquad (\delta.)$$

if we should make $x = h\sin\theta$ (the hyperbolic sine of θ), and $\sqrt{(1+x^2)} = h\cos\theta$ (the hyperbolic cosine of θ), $y = h\sin\theta'$, and $\sqrt{1+y^2} = h\cos\theta'$, adopting the terms which Lambert introduced, and which have been noticed in the note in p. 231; and it is evident that it would be possible from equation (δ.), combined with the assumptions made in deducing it, to frame a system of hyperbolic trigonometry (having reference to the sectors, and not or in its applications to geometry. The terms *tangent, cotangent, secant* and *cosecant,* and *versed sine,* which denote very simple functions of the sine and cosine, may be defined by those functions and will be merely used when they enable us to exhibit formulæ involving sines and cosines, in a more simple form. By adopting such a view of the meaning and origin of the transcendental functions, the relations and properties of which constitute the science of trigonometry, we are at once freed from the necessity of considering those functions as lines described in and about a circle, and as jointly dependent upon the magnitude of the angles to which they correspond and of the radius of the circle itself. It is this last element, which is thus introduced, which is not merely superfluous, but calculated to give erroneous views of the origin and constitution of trigonometrical formulæ and greatly to embarrass all their applications.

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to the arcs of the equilateral hyperbola), whose formulæ would bear a very striking analogy to the formulæ of trigonometry, properly so called.

Abel, in the second volume of *Crelle's Journal,* has laid the foundation, of a species of *elliptic* trigonometry, (if such a term may be used,) in connexion with a remarkable extension of the theory of elliptic integrals. If we denote the elliptic integral of the first species

$$\int_0 \frac{d\psi}{\sqrt{(1 - c^2 \sin^2 \psi)}}$$

by θ, and replace $\sin \psi$ by x, we shall get

$$\theta = \int_0 \frac{dx}{\sqrt{\{(1 - x^2)(1 - c^2 x^2)\}}}$$

or more generally

$$\theta = \int_0 \frac{dx}{\sqrt{\{(1 + e^2 x^2)(1 - c^2 x^2)\}}}.$$

If we now suppose $x = \phi\,\theta$, $\sqrt{(1 - c^2 x^2)} = f\,\theta$ and $\sqrt{(1 + e^2 x^2)} = \mathrm{F}\,\theta$, it may be demonstrated that

$$\phi\,(\theta + \theta') = \frac{\phi\,\theta \,.\, f\,\theta' \,.\, \mathrm{F}\,\theta' + \phi\,\theta' \,.\, f\,\theta \,.\, \mathrm{F}\,\theta}{1 + e^2 c^2 \phi^2 \theta \,.\, \phi^2 \theta'},$$

$$f\,(\theta + \theta') = \frac{f\,\theta \,.\, f\,\theta' - c^2 \phi\,\theta \,.\, \phi\,\theta' \,.\, \mathrm{F}\,\theta \,.\, \mathrm{F}\,\theta'}{1 + e^2 c^2 \phi^2 \theta \,.\, \phi^2 \theta'},$$

$$\mathrm{F}\,(\theta + \theta') = \frac{\mathrm{F}\,\theta \,.\, \mathrm{F}\,\theta' + e^2 \phi\,\theta \,.\, \phi\,\theta' \,.\, f\,\theta \,.\, f\,\theta'}{1 + e^2 c^2 \phi^2 \theta \,.\, \phi^2 \theta'}:$$

or if, for the sake of more distinct and immediate reference to these peculiar transcendents, we denote

$\phi\,\theta$ by ${}_e\sin\theta$ (elliptic sine of θ),

$f\,\theta$ by ${}_e\cos\theta$ (elliptic cosine of θ), and

$\mathrm{F}\,\theta$ by ${}_e\mathrm{sur}\,\theta$ (elliptic sursine of θ), The primitive signs $+$ and $-$, when applied to symbols denoting lines, are only competent to express the relation of lines which are parallel to each other when drawn or estimated in different directions; but the more general sign $\cos\theta + \sqrt{-1}\sin\theta$, which has been noticed in the former part of this Report, when applied to such symbols, is competent to express all the relations of position of lines in the same plane with respect to each other. It is the use of this sign which enables us to subject the properties of rectilinear figures to the dominion of algebra: thus, a series of lines represented in magnitude and position by a_0, $(\cos\theta_1 + \sqrt{-1}\sin\theta_1)\,a_1$, $\{\cos(\theta_1+\theta_2) + \sqrt{-1}\sin(\theta_1+\theta_2)\}\,a_2$, $\ldots\{\cos(\theta_1+\theta_2+\ldots\theta_{n-1}) + \sqrt{-1}\sin(\theta_1+\theta_2+\ldots\theta_{n-1})\}\,a_{n-1}$, will be competent to form a *closed* figure, if the following equations be satisfied:

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then these fundamental equations will become

$$\sin_e(\theta+\theta') = \frac{\sin_e\theta\cos_e\theta'\,\mathrm{surs}_e\,\theta' + \sin_e\theta'\cos_e\theta\,\mathrm{surs}_e\,\theta}{1 + e^2c^2\sin_e^2\theta\sin_e^2\theta'},$$

$$\cos_e(\theta+\theta') = \frac{\cos_e\theta\cos_e\theta' - c^2\sin_e\theta\sin_e\theta'\,\mathrm{surs}_e\,\theta\,\mathrm{surs}_e\,\theta'}{1 + e^2c^2\sin_e^2\theta\sin_e^2\theta'}.$$

$$\mathrm{surs}_e(\theta+\theta') = \frac{\mathrm{surs}_e\,\theta\,\mathrm{surs}_e\,\theta' + e^2\sin_e\theta\sin_e\theta'\cos_e\theta\cos_e\theta'}{1 + e^2c^2\sin_e^2\theta\sin_e^2\theta'}.$$

If we add, subtract and multiply, the elliptic sines, cosines and sursines of the sum and difference of θ and θ' respectively, reducing them, when necessary, by the aid of the fundamental relations which exist amongst these three transcendents, we shall obtain a series of formulæ, some of which are very remarkable, and which degenerate into the ordinary formulæ of trigonometry, when $e = 0$ and $c = 1$: we shall thus likewise be enabled to express $\sin_e n\theta$, $\cos_e n\theta$, $\mathrm{surs}_e\, n\theta$, in terms of $\sin_e\theta$, $\cos_e\theta$, $\mathrm{surs}_e\,\theta$. The inverse problem, however, to express $\sin_e\theta$, $\cos_e\theta$, $\mathrm{surs}_e\,\theta$, in terms of $\sin_e n\theta$, $\cos_e n\theta$, $\mathrm{surs}_e\, n\theta$, is one of much greater difficulty, requiring the consideration of equations of high orders, but whose ultimate solution can be made to depend upon that of an equation of $(n + 1)$ dimensions only. It is in the discussion of these equations that Abel has displayed all the resources of his extraordinary genius.

It would be altogether out of place to enter upon a lengthened statement of the various properties of these elliptic sines, cosines, and sursines; their periodicity, their limits, their roots, and their extraordinary use in the transformation of elliptic functions. My object has been merely to notice the rudiments of a species of elliptic trigonometry, the cultivation of which, even without the aid of a distinct algorithm, has already contributed so greatly to the enlargement of the domains of analysis.

$$a_0+a_1\cos\theta_1+a_2\cos(\theta_1+\theta_2)+\,.\,.\,a_{n-1}\cos(\theta_1+\theta_2+\,.\,.\,\theta_{n-1})=0 \quad (1.)$$

$$a_1\sin\theta_1+a_2\sin(\theta_1+\theta_2)+\,.\,.\,a_{n-1}\sin(\theta_1+\theta_2+\,.\,.\,\theta_{n-1})=0 \quad (2.)$$

$$\theta_1+\theta_2+\,.\,.\,.\,\theta_{n-1}\,.=(n-2\,r)\,\pi \quad (3.)$$

The first two of these equations may be called *equations* of *figure*, and the last the *equation* of *angles*, and all of them must be satisfied in order that the lines in question may be capable of being formed into a figure, along the sides of which if a point be moved it will circulate continually. If the values of θ_1, $\theta_2-\theta_1$, $\theta_3-\theta_2\,.\,.\,\theta_{n-1}-\theta_{n-2}$ be all positive, and if $r=1$, then the equation of angles will correspond to those rectilineal figures to which the corollaries to the thirty-second proposition of the first book of Euclid are applicable, and which are contemplated by the ordinary definitions of rectilineal figures in geometry. If we should suppose $r=2$ or 3 or any other whole number different from 1, the equation would correspond to *stellated* figures, where the sum of the exterior angles shall be 8, 12, or $4\,r$ right angles. The properties of such *stellated* figures were first noticed by Poinsot in the fourth volume of the *Journal de l'Ecole Polytechnique*, in a very interesting memoir on the Geometry of Situation*.

All equal and parallel lines drawn or estimated in the same direction are expressed by the same symbol affected by the same sign, whatever it may be: and it is this infinity of lines, *geometrically* different from each other, which have the same algebraical representation, which renders it necessary to consider the position of lines, not merely with respect to each other, but also with respect to *fixed* lines or axes, through the medium of the *equations* of their generating points. In other words, it is not possible to supersede even rectilineal geometry by means of *affected* symbols only. We are thus led to the consideration of a new branch of analytical science, which is specifically denominated the Application of Algebra to Geometry, and which enables us to consider every relation of points in space and the laws of their connexion with each other, whatever those laws may be. It is not our intention, however, to enter upon the discussion of the general principles of this science, or to notice its present state or recent progress.

A great number of elementary works on trigonometry have been published of late years in this country, many of which are remarkable for the great simplicity of form to which they have reduced the investigation of the fundamental formulæ. Such works are admirably calculated to promote the extension of

* See also Peacock's *Algebra*, p. 448.

mathematical education, by placing this most important branch of analytical science, the very key-stone of all the applications of mathematics to natural philosophy, within the reach of every student who has mastered the elements of geometry and the first principles of algebra.

We have before had occasion to notice the work of the late Professor Vince upon this subject, which was generally used in the Universities of England for some years after the commencement of the present century. Its author was a mathematician of no inconsiderable powers, and of very extensive knowledge, but who was totally destitute of all feeling for elegance in the selection and construction of his formulæ, and who had no acquaintance with, or rather no proper power of appreciating, those beautiful models of symmetry and of correct taste which were presented by the works of Euler and Lagrange. But though this treatise was singularly rude and barbarous in its form, and altogether inadequate to introduce the student to a proper knowledge either of the objects or of the powers of this science, yet it was greatly in advance of other treatises which were used and studied in this country at the period of its publication. Amongst these may be mentioned the treatise on Trigonometry which is appended to Simson's *Euclid*, which was more adapted to the state of the science in the age of Ptolemy than at the close of the eighteenth century*.

The *Plane and Spherical Trigonometry* of the late Professor Woodhouse appeared in 1810, and more than any other work contributed to revolutionize the mathematical studies of this country. It was a work, independently of its singularly opportune appearance, of great merit, and such as is not likely, notwithstanding the crowd of similar publications in the present day, to be speedily superseded in the business of education. The fundamental formulæ are demonstrated with considerable elegance and simplicity; the examples of their application, both in plane and spherical trigonometry, are well selected and very carefully worked out; the uses of trigonometrical formulæ, in some of their highest applications, are exhibited and pointed

* Similar remarks might be applied to treatises upon trigonometry which were published both before and after the appearance of Professor Woodhouse's *Trigonometry*. The author of this Report well recollects a treatise of this kind which was extensively used when he was a student at the University, in which the proposition for expressing the sine of an angle in terms of the sides of a triangle, was familiarly denominated the *black triangle*, in consequence of the use of thick and dark lines to distinguish the primitive triangle amidst the confused mass of other lines in which it was enveloped, for the purpose of obtaining the required result by means of an incongruous combination of geometry and algebra.

out in a very clear and striking form; and, like all other works of this author, it is written in a manner well calculated to fix strongly the attention of the student, and to make him reflect attentively upon the particular processes which are followed, and upon the reasons which lead to their adoption.

The circumstances attending the publication and reception of this work in the University of Cambridge were sufficiently remarkable. It was opposed and stigmatized by many of the older members, as tending to produce a dangerous innovation in the existing course of academical studies, and to subvert the prevalent taste for the geometrical form of conducting investigations and of exhibiting results which had been adopted by Newton in the greatest of his works, and which it became us, therefore, from a regard to the national honour and our own, to maintain unaltered. It was contended, also, that the primary object of academical education, namely, the severe cultivation and discipline of the mind, was more effectually attained by geometrical than by analytical studies, in which the objects of our reasoning are less definite and tangible, and where the processes of demonstration are much less logical and complete. The opposition, however, to this change, though urged with considerable violence, experienced the ordinary fate of attempts made to resist the inevitable progress of knowledge and the increased wants and improving spirit of the age. In the course of a few years the work in question was universally adopted. The antiquated fluxional notation which interfered so greatly with the familiar study of the works of Euler, Lagrange, Laplace, and the other great records of analytical and philosophical knowledge, was abandoned*; the works of the best mathematical writers on the continent of Europe were rapidly introduced into the course of the studies of the University; and the secure foundations were laid of a system of mathematical and philosophical education at once severe and comprehensive, which is now producing, and is likely to continue to produce, the most important effects upon the scientific character of the nation.

Theory of Equations. 1. *Composition of Equations.*—The first and one of the most difficult propositions which presents itself in the theory of equations is to prove "that all equations under a rational form, and arranged according to the method

* The continental notation of the differential calculus was first publicly introduced into the Senate House examinations in 1817. Though the change was strongly deprecated at the time, it was very speedily adopted, and in less than two years from that time the fluxional notation had altogether disappeared.

of Harriott, the significant terms forming one member, and zero the other, are said to be resolvible into simple or quadratic factors." It is only another form of the same proposition to say, "that every equation has as many roots as it has dimensions, and no more; those roots being either real* or imaginary;" that is, being quantities which are expressible by symbols denoting real magnitudes affected by such signs as are recognised in algebra.

We have before said that it is impossible to assign beforehand an absolute limit to the possible existence of signs of affection different from those which are involved in the symbolical values of $(1)^n$ and $(-1)^n$; and when it is said that every equation is resolvable into factors of the form $x - a$, we presume that a is either a real magnitude, or of the form $\alpha + \beta\sqrt{-1}$, where α and β are real magnitudes. If we should fail in establishing this proposition, it would by no means necessarily follow that there might not exist other forms of factors like $x - a$, where a denoted a real magnitude affected by some *unknown* sign different from $+$, $-$, or $\cos\theta + \sqrt{-1}\sin\theta$, which might satisfy the required conditions: at the same time its demonstration will show that our recognised signs are competent to denote all the affections of magnitude which are subject to any conditions which are reducible to the form of an equation.

If we assume in the first instance the composition of equations to be such as we have stated in the enunciation of the fundamental proposition, we can at once ascertain the composition of the several coefficients of the powers of x in the equation

$$x^n - p_1 x^{n-1} + p_2 x^{n-2} - \ldots\ldots \pm p_n = 0,$$

and we can complete the investigation of all those general properties of equations which such an *hypothesis* would lead to. All such conclusions, when established upon such a foundation, are conditional only. It is not expedient, however, to make the fate of any number of propositions, however consistent with each other, and however unquestionable their truth may appear to be from indirect or from *à posteriori* considerations, dependent upon an hypothesis, when it is possible to convert this hypothesis into a necessary symbolical truth. Using such an hypothesis, therefore, as a *suggestion* merely, let us propose the

* It is convenient in the theory of equations, for the purpose of avoiding repetition, to consider symbols denoting *arithmetical* magnitudes and affected with the signs $+$ or $-$, as *real*; and quantities denoted by symbols affected with the sign $\cos\theta + \sqrt{-1}\sin\theta$, as *imaginary*.

following problem, and examine all the consequences to which its solution will lead.

"To find n quantities $x, x_1, x_2, \ldots x_{n-1}$, such that their sum shall be equal to p_1, the sum of all their products *two* and *two* shall be equal to p_2, the sum of all their products *three* and *three* shall be equal to p_3, and so on, until we arrive at their continued product, which shall be equal to p_n."

The quantities $x, x_1, \ldots x_{n-1}$, are supposed to be any quantities whatever, whether real or affected by any signs of affection whether known or unknown. It is our object to show that the only sign of affection required is $\cos\theta + \sqrt{-1}\sin\theta$, taken in its most general sense.

It is very easy to show that the solution of this problem will lead to a general equation, whose coefficients are $p_1, p_2, \ldots p_n$: for if we suppose the first of these quantities x to be omitted, and $P_1, P_2, \ldots P_{n-1}$ to be the quantities corresponding to $p_1, p_2, \ldots p_n$ when there are $(n-1)$ quantities instead of n, then we shall get

$$x + P_1 = p_1,$$
$$x P_1 + P_2 = p_2,$$
$$x P_2 + P_3 = p_3,$$
$$\cdots\cdots\cdots$$
$$x P_{n-2} + P_{n-1} = p_{n-1},$$
$$x P_{n-1} = p_n.$$

If we multiply these equations from the first downwards by the terms of the series $x^{n-1}, x^{n-2}, \ldots x^2, x, 1$, and add the first, third, fifth, &c., of the results together, and subtract the second, fourth, sixth, &c., we shall get the general equation

$$x^n - p_1 x^{n-1} + p_2 x^{n-2} - \ldots + (-1)^n p_n = 0. \qquad (1.)$$

In as much as $p_1, p_2, \ldots p_n$ may represent any real magnitudes whatever, zero included, it is obvious that we may consider this equation as the result of the solution of the problem in its most general form. And in as much as x may represent any one of the n quantities involved in the problem, we must equally obtain the same equation for all those n quantities: it also follows that every general solution of this equation must comprehend the expression of all the roots.

By this mode of presenting the question we are authorized in considering the *symbolical composition* of the coefficients of *every* equation as *known*, though the ultimate *symbolical form* of the roots is *not known*; and our inquiry will now be properly limited to the question of ascertaining whether symbols representing real magnitudes affected by the recognised and *known* signs of affection *only*, are competent, under all circumstances, to answer the required conditions of the problem.

If the value of one root can be ascertained, and that root be *real*, the problem can be simplified, and the dimensions of the equation depressed by unity; for the coefficients of the reduced equation $P_1 . P_2 . P_{n-1}$, *which are also real*, can be successively determined. If more real roots than one can be found, the dimensions of the equation can be depressed by as many *unities* as there are real roots. If the root determined be not real, and if a similar process for depressing the dimensions of the equation be adopted, the coefficients of the new equation would not be real, and the conditions of the problem with respect to the remaining roots would be changed. But if we could ascertain a pair of such roots, such that their sum $= x + x_1$ and their product $= x\,x_1$ should be real, then the dimensions of the equation might be depressed by two unities, without changing the conditions of the problem with respect to the remaining roots; for if we supposed Q_1, Q_2, Q_3, &c., to represent the coefficients of the reduced equation, we should find,

$$x + x_1 + Q_1 = p_1,$$

$$x\,x_1 + (x + x_1)\,Q_1 + Q_2 = p_2,$$

$$x\,x_1\,Q_1 + (x + x_1)\,Q_2 + Q_3 = p_3,$$

$$\cdots\cdots\cdots\cdots\cdots\cdots$$

$$x\,x_1\,Q_{n-4} + (x + x_1)\,Q_{n-3} + Q_{n-2} = p_{n-1},$$

$$x\,x_1\,Q_{n-2} = p_n,$$

from which equations we can determine successively rational values of $Q_1, Q_2, \ldots Q_{n-2}$. It remains to show, therefore, that in all cases we can find *pairs* of roots which will answer these conditions.

If the number of quantities $x, x_1, \ldots x_n$, be odd, it is very easy to *prove* that there is always a *real* value of one of them, x, which will satisfy the conditions of the general equation (1.) *, and that consequently the dimensions of the equation may be depressed by *unity*, and our attention confined therefore to the case where the dimensions of the equation are even. If m, therefore, be any odd number, the form of n may be either $2\,m$, $2^2\,m$, $2^3\,m$, $2^n\,m$, and so on. Let us consider, in the first place, the first of these cases.

The number of combinations of $2\,m$, things taken two and two together, is $m\,(2\,m - 1,)$ and therefore an odd number: these

* This may be easily proved without the necessity of making any hypothesis respecting the composition of the equation. See the Article 'EQUATIONS' in the *Supplement to the Encyclopædia Britannica,* written by Mr. Ivory.

combinations may be either the *sums* of every two of the quantities, $x, x_1, \ldots x_{n-1}$, such as $x + x_1$, $x + x_2$, &c., or their products, such as $x\,x_1$, or other rational linear functions of those quantities, involving two of them only, such as $x + x_1 + x\,x_1$, $x + x_1 + 2\,x\,x_1$, or $x + x_1 + k\,x\,x_1$, where k may be any given number whatsoever. If we take any one of these sets of combinations, we can form rational expressions for their sum, for the sum of their products, two and two, three and three, and so on, in terms of the coefficients $p_1, p_2, \ldots p_n$, of the original equation (1.), by means of the common theory of symmetrical functions *, and consequently, we can form the corresponding equations of $m\,(2\,m - 1)$ dimensions which will have rational and known coefficients. Such equations being of odd dimensions must have at least one *real* root; or, in other words, there must exist at least one real value of one of the sums of two roots, such as $x + x_1$, of one of the products, such as $x\,x_1$, of one of the functions, $x + x_1 + x\,x_1$, or $x + x_1 + k\,x\,x_1$. If the symbols which form the real sum $x + x_1$ are the same with those which form the real value of the product $x\,x_1$, then, under such circumstances, x and x_1 are expressible by real magnitudes affected with the ordinary signs of algebra †. We shall now proceed to show that this must be the case.

If we form the equations successively whose roots are $x + x_1 + k\,x\,x_1$, corresponding to different values of k, we shall have one real root at least in each of them. If we form more than $m\,(2\,m - 1)$, such equations for different values of k, we must at least have amongst them the same combination of x and x_1 forming the real root, in as much as there are only $m\,(2\,m - 1)$ such combinations which are different from each other. Let k and k_1 be the values of k which give such combinations, and let α' and β' be the values of the real roots corresponding; then we must have

$$x + x_1 + k\,x\,x_1 = \alpha'$$
$$x + x_1 + k_1\,x\,x_1 = \beta'$$

* The formation of symmetrical combinations of any number of *symbolical* quantities $x, x_1, \ldots x_{n-1}$, and the determination of their symbolical values in terms of their sums (p_1), their products two and two (p_2), three and three (p_3), and so on, involves no principle which is not contained in the direct processes of algebra, and is altogether independent of the theory of equations. The theorems for this purpose may be found in the first chapter of Waring's *Meditationes Algebraicæ*, in Lagrange's *Traité sur la Résolution des Equations Numériques*, chap. i. and notes 3 and 10, and with more or less detail in nearly all treatises on Algebra.

† If $x + x_1 = \alpha$ and $x\,x_1 = \beta$., where α and β are real magnitudes, then $x = \frac{\alpha}{2} \pm \sqrt{\left\{\frac{\alpha^2}{4} - \beta\right\}}$ the values of which are either real or of the form $(\cos\theta \pm \sqrt{-1}\sin\theta)\sqrt{\beta}$, where the modulus $\sqrt{\beta}$ is real.

and therefore

$$x x_1 = \frac{\alpha' - \beta'}{k - k_1}$$

$$x + x_1 = \frac{k_1 \alpha' - k \beta'}{k_1 - k}.$$

There are therefore necessarily two roots of the equation or two values of the symbols $x, x_1, x_2, \ldots x_{n-1}$, such that $x + x_1$ and $x x_1$ are real; and therefore it is always possible, in an equation whose dimensions are *impariter par*, to depress them by *two* unities, so that the reduced equation may still possess rational coefficients.

If the number of symbols involved in the original problem be $2^2 m$, then the number of their binary combinations must be $2 m (2^2 m - 1)$ or *impariter par*. It will immediately follow, from what we have already proved, that there are two values of the *sum* and *product* of the *same* symbols, which are either real or of the form $\alpha + \beta \sqrt{-1}$; and consequently the symbols themselves will admit of expression under a similar form *.

If the dimensions of the original equation be $2^3 m$ or $2^4 m$, or any one in an ascending series of orders of *parity*, it may be reduced down to the next order of parity in a similar manner: and under all circumstances it may be shown that there must be two roots which are reducible to the form $\alpha \pm \beta \sqrt{-1}$, where α and β real or *zero*; and also in any equation of even dimensions, we can reduce its dimensions successively by two unities, thus producing a series of equations of successive or decreasing *orders of parity*, in which we can demonstrate the existence of successive pairs of roots of the required form until they are all exhausted.

This mode of proving the composition of equations differs chiefly from that which was noticed by Laplace, in his lectures to the *Ecole Normale* in 1795 †, in the form in which the question is proposed. A certain number of symbols, representing magnitudes with *unknown* affections, are required to satisfy

* Let

$$x + x' = r (\cos \theta + \sqrt{-1} \sin \theta)$$

$$x x' = \varrho (\cos \varphi + \sqrt{-1} \sin \varphi)$$

$$\overline{x + x'}|^2 - 4 x x' = \mathrm{R}^2 (\cos 2 \psi + \sqrt{-1} \sin 2 \psi)$$

$$\text{or } x - x' = \mathrm{R} (\cos \psi + \sqrt{-1} \sin \psi)$$

$$x = \frac{r \cos \theta + \mathrm{R} \cos \psi}{2} + \frac{(r \sin \theta + \mathrm{R} \sin \psi)}{2} \sqrt{-1}$$

$$= r' (\cos \chi + \sqrt{-1} \sin \chi)$$

$$x' = r' (\cos \chi - \sqrt{-1} \sin \chi).$$

† *Leçons de l'Ecole Normale*, tom. ii.

certain real conditions: those conditions are found to be identical with those which the unknown quantity, or, in other words, the *root* in an equation of n dimensions, is required to satisfy. The object of the proof above given is to show that it is always possible to find n real magnitudes with *known* affections which are competent to satisfy these conditions; and those quantities, therefore, are of such a kind that the equation, whose roots they are, is always resolvible into real quadratic factors; a most important conclusion, which the greatest analysts have laboured to deduce by methods which have not been, in most cases at least, free from very serious objections.

There are two classes of demonstrations which have been given of this fundamental proposition in the theory of equations. The first class comprehends those in which the form of the roots is determined from the conditions which they are required to satisfy; the second class, those in which the form of the roots is assumed to be comprehended under different values of ρ and θ in the expression $\rho\,(\cos\theta + \sqrt{-1}\sin\theta)$, and it is shown that they are competent to satisfy the conditions of the equation. To the first class belongs the demonstration given above; those given by Lagrange in notes ix. and x. to his *Résolution des Equations Numériques*; the first of those given by Gauss in the *Gottingen Transactions* for 1816*; and by Mr. Ivory in his article on Equations in the *Supplement to the Encyclopædia Britannica.* To the second class belongs the second demonstration given by Gauss in the same volume of the *Gottingen Transactions*; by Legendre in the 14th section of the first Part of his *Théorie des Nombres*; by Cauchy in the 18th cahier of the *Journal de l'Ecole Polytechnique*; and subsequently under a slightly different form in his *Cours d'Analyse Algébrique.*

The first of the demonstrations given by Gauss, like many other writings of that great analyst, is extremely difficult to follow, in consequence of the want of distinct enunciations of the propositions to be proved, and still more from their not always succeeding each other in the natural order of investigation. It requires the aid likewise of principles, or rather of processes, which are too far advanced in the order of the results of algebra to be properly employed in the establishment of a proposition which is elementary in the order of truths, though it may not be so in the order of difficulty. If we may

* There is another demonstration by Gauss, published in 1799, which I have never seen. In his Preface to his *Demonstratio Nova Altera* he speaks of its being founded partly on geometrical considerations, and in other respects as involving very different principles from the second.

be allowed, however, to consider it apart from such considerations, it would appear to be complete and satisfactory, and very carefully guarded against any approach to an assumption of the proposition to be proved, a defect to which most of the demonstrations of this class are more or less liable*. It extends to equations whose dimensions involve different or successive orders of parities, nearly in the same manner as in the demonstration which we have given above.

The demonstration given by Mr. Ivory is different from any other, and the principles involved in it are such as naturally present themselves in such an investigation; and it will be recommended to many persons by its not involving directly the use, or supposing the necessary existence of, imaginary quantities. It is not, however, altogether free from some very serious defects in the form under which it at present appears, though most of them admit of being remedied without any injury to the general scheme of the demonstration, which is framed with great skill, and which exhibits throughout a perfect command over the most refined and difficult artifices of analysis.

Lagrange has devoted two notes to his great work on the Resolution of Numerical Equations to the discussion of the forms of the roots of equations. In the first of these notes, after examining the very remarkable observations of D'Alembert on the forms of imaginary quantities, he proceeds to consider the case of an equation such as $f(x) + V = 0$, where $f(x)$ is a rational function of x; if for different values a and b of the last term of this equation, where $a \angle b$, we may suppose a root which is not real for values of V between those limits, to become real at those limits, he then shows that for values of V between those limits, and indefinitely near to them, the corresponding root of the equation must involve $\sqrt{-1}$, or $\sqrt[4]{-1}$, or $\sqrt[6]{-1}$, and so on; or, in other words, that the roots of the equation in the transition of their values from real to imaginary (whatever may be the affection of magnitude which renders them imaginary), will change in form from α to $m + n\sqrt{-1}$. He subsequently shows that the same result will follow for any values of V between a and b, and consequently,

* I do not venture to speak more decidedly; for though I have read it entirely through several times with great care, I do not retain that distinct and clear conviction of the essential connexion of all its parts which is necessary to compel assent to the truth of a demonstration. It is unfortunately frequently the character of many of the higher and more difficult investigations connected with the general theory of the composition and solution of equations to leave a vague and imperfect impression of their truth and correctness even upon the minds of the most laborious and best instructed readers.

that in every instance, when roots of equations cease to be real, they will assume the form $m + n\sqrt{-1}$.

This demonstration is not merely indirect, but it does not arise naturally from the question to be investigated. It seems likewise to assume the existence of some algebraical form which expresses the value of the root in terms of the coefficients of the equation, an assumption which, as will afterwards be seen, it would be difficult to justify by any *à priori* considerations. The illustrious author himself seems to have felt the full force of these objections, and he proceeds therefore in the following Note to prove that every polynomial of a rational form will admit of rational divisors of the first or second degree. The demonstration which he has given is founded upon the theory of symmetrical functions, and shows that the coefficients of such a divisor may be made to depend severally upon equations all whose coefficients are rational functions of the coefficients of the polynomial dividend. Whatever be the degree of parity of the number which expresses the dimensions of this polynome, he shows the possibility of the coefficients of this quadratic divisor, which is the capital conclusion in the theory. It ought to be observed, however, that the whole theory of the composition of equations is so much involved in the different steps of this investigation, or, at all events, that so little provision is made in conducting it to guard against the assumption of this truth, that we should not be justified in considering this demonstration as perfectly independent or as furnishing an adequate foundation for so important a conclusion. If we view it, however, simply with reference to the problem for exhibiting the nature of the law of dependence which connects the coefficients of the polynomial factor with those of the original polynomial dividend, it must still be considered as an investigation of no inconsiderable importance, as bearing upon the general theory of the solution and depression of equations.

The second of the proofs given by Gauss, the proof of Legendre, and both of those which have been given by Cauchy, belong to the second class of demonstrations to which we have referred above. Assuming the root to be represented by $\rho(\cos\theta + \sqrt{-1}\sin\theta)$, the equation is reduced to the form $P + Q\sqrt{-1}$, or $\sqrt{(P^2 + Q^2)} \,.\, (\cos\phi + \sqrt{-1}\sin\phi)$; and the object of the demonstration is to show that there exist necessarily real values of ρ and θ, which make $P^2 + Q^2 = 0$. This is effected by Gauss by processes which are somewhat synthetical in their form, and such as do not arise very naturally or directly from the problem to be investigated; and the essential part of the demonstration requires a double integration between assigned limits, a process against which serious objections may in this instance be raised, independently of its involving analytical truths and principles of too advanced an order.

The demonstration of Legendre depends upon the possible discovery, by tentative or other means, of values of ϱ and θ, which render P and Q very small; and subsequently requires us, by the application of the ordinary processes of approximation, to find other values of ϱ and θ, subject to repeated correction, which may render P and Q smaller and smaller, and ultimately equal to zero. The objection to this demonstration, if so it may be called, is the absence of any proof of the necessary existence of values of ϱ and θ; and if they should be shown to exist, it seems to fail in showing that the subsequent corrections of their values which this process would assign would really and necessarily increase the required approximations.

The demonstrations of Cauchy are formed upon the general scheme of that which is given by Legendre, at the same time that they seem to avoid the very serious defects under which that demonstration labours: he shows that $(P^2 + Q^2)$ must admit of a *minimum*, and that this *minimum* value must be zero. The second of the demonstrations differs from the first merely in the manner of establishing the existence and value of this *minimum*: they both of them appear to me to be quite complete and satisfactory.

It is not very difficult to establish this fundamental proposition by reasonings derived from the geometrical representation of impossible quantities. This was done, though imperfectly, by M. Argand, in the fifth volume of Gergonne's *Annales des Mathématiques* *, and has been since reconsidered by M. Murey, in a very fanciful work upon the geometrical interpretation of imaginary quantities, which was published in 1827. It seems to me, however, to be a violation of propriety to make such interpretations which are conventional merely, and not necessary, the foundation of a most important symbolical truth, which should be considered as a necessary result of the first principles of algebra, and which ought to admit of demonstration by the aid of those principles alone.

General Solution of Equations.—The solution of equations in its most general sense would require the expression of its roots by such functions of their coefficients as were competent

* In the fourth volume of the same collection there are demonstrations of this fundamental proposition, given by M. Dubourguet and M. Encontre, which do not appear, however, to merit a more particular notice.

to express them, when those coefficients were general symbols, though representing rational numbers. Such functions also must equally express all the roots, in as much as they are all of them equally dependent upon the coefficients for their value; and they must express likewise the values of no quantities which are not roots of the equation.

The problem, in fact, is the inverse of that for the formation of the equation which is required to satisfy assigned conditions. And as we have shown that there always exist quantities expressible by the ordinary signs of algebra which will fulfil the conditions of any equation with rational coefficients, so likewise we might appear to be justified in concluding that there must exist explicable functions of those coefficients which in all cases would be competent to represent those roots.

A very little consideration, however, would show that such a conclusion was premature. In the first place, such a function must be irrational, in as much as all rational functions of the coefficients admit but of one value; and they must be such irrational functions of the coefficients as will successively *insulate* the several roots of the equation,—for they must be equally capable of expressing all the roots,—and they must be capable likewise of effecting this *insulation* without any reference to the specific values of the symbols involved, or to the relation of the values of the roots themselves; for otherwise they could not be said to represent the general solution of any equation whatever of a given degree. The question which naturally presents itself, after the enumeration of such conditions, is, whether we could conclude that any succession of operations which are, properly speaking, algebraical, would be competent to fulfil them.

If it be further considered that those successive operations must be assigned beforehand for every general equation of an assigned degree; that every one of these operations can give one real value only, or at the most two; and that the result of these operations, which must embrace all the coefficients, must express the n roots of the equation and those roots only; it will readily be conceded that the solution of this great problem is probably one which will be found to transcend the powers of analysis.

The solutions of cubic and biquadratic equations have been known for nearly three centuries; and all the attempts which have hitherto been made to proceed beyond them, at least in equations in which there exists no relation of dependence amongst the several coefficients, and no presumed or presumable relation amongst the roots, have altogether failed of success: and if we consider that this great problem has been subjected to the most scrutinizing and laborious examination by nearly all the greatest analysts who have lived in that period, we may be justified in concluding that this failure is rather to be attributed to the essential impossibility of the problem itself than to the want of skill or perseverance on the part of those who have made the attempt. But in the absence of any complete and uncontrovertible proof of this impossibility, the question cannot be considered as concluded, and will still remain open to speculations upon the part of those with whom extensive and well-matured knowledge, and a deep conviction founded upon it, have not altogether extinguished hope.

The different methods which have been proposed for the resolution of cubic and biquadratic equations, and the consequences of the extension of their principles to the solution of equations of higher orders, have been subjected to a very detailed analysis by Lagrange, in the *Berlin Memoirs* for 1770 and 1771, and in the Notes xiii. and xiv. of his *Traité sur la Résolution des Equations Numériques*; and it would be difficult to refer to any investigations of this great analyst which are better calculated to show the extraordinary power which he possessed of referring methods apparently the most distinct to a common principle of a much higher and more comprehensive generality. In the subsequent remarks which we shall make, we shall rarely have occasion to proceed beyond a notice of the general conclusions to which he has arrived, and to show their bearing upon some later speculations upon the same subject.

A very slight examination of the principles involved in the solution of the equations of the third and fourth degrees will show them to be inapplicable to those of higher orders. A notice of a very few of such methods will be quite sufficient for our purpose.

Thus, the ordinary solution of the cubic equation

$$x^3 - 3\,q\,x + 2\,r = 0\text{*}$$

is made to depend upon that of the following problem:

"To find two numbers or quantities such that the sum of their cubes shall be equal to $2\,r$ and their product equal to q."

If we represent the required numbers by u and v, we readily obtain the equation of reduction

$$u^6 - 2\,r\,u^3 + q^3 = 0,$$

* This equation may be considered as equally general with

$$x^3 - \mathrm{A}\,x^2 + \mathrm{B}\,x - \mathrm{C} = 0,$$

in as much as we can pass from one to the other by a very easy transformation; and the same remark may be extended to equations of higher orders. Such a change of form, however, will determine the applicability or inapplicability of many of the methods which are proposed for their solution.

which gives, when solved as a quadratic equation,

$$u^3 = r + \surd(r^2 - q^3),$$

and consequently,

$$u = \{r + \surd(r^2 - q^3)\}^{\frac{1}{3}},$$

and therefore

$$v = \frac{q}{u} = \frac{q}{\{r + \surd(r^2 - q^3)\}^{\frac{1}{3}}}.$$

If we call 1, α, α^2, the three cube roots of 1, or the roots of the equation $z^3 - 1 = 0$, and if we assume a to represent the arithmetical value of u, we shall obtain the following three values of $u + v$, which are

$$a + \frac{q}{a},\ a\,\alpha + \frac{q}{a\,\alpha},\ a\,\alpha^2 + \frac{q}{a\,\alpha^2}.$$

These values, though derived from the solution of an equation of six dimensions *, are only three in number, and form, therefore, the roots of a cubic equation. A little further inquiry will show that they are the roots of the cubic equation

$$x^3 - 3\,q\,x + 2\,r = 0:$$

for it may readily be shown, in the first place, that their sum $= 0$; that the sum of their products two and two $= -3\,q$; and that their continued product $= 2\,r$; or in other words, that they are the roots of an equation which is in every respect identical with the equation in question †.

* There are *six* values of u, in as much as the values of u and v are interchangeable, from the form in which the problem was proposed; but there are only *three* values of $u + v$.

† Since $$\frac{q}{\{r + \surd(r^2 - q^3)\}^{\frac{1}{3}}} = \{r - \surd(r^2 - q^3)\}^{\frac{1}{3}},$$

it is usual to express the roots of the equation $x^3 - 3\,q\,x + 2\,r = 0$, by the formula

$$x = \{r + \surd(r^2 - q^3)\}^{\frac{1}{3}} + \{r - \surd(r^2 - q^3)\}^{\frac{1}{3}}, \qquad (1.)$$

which is in a certain sense incorrect, in as much as it admits of nine values instead of three. The six additional values are the roots of the two equations

$$x^3 - 3\,\alpha\,q\,x + 2\,r = 0,$$
$$x^3 - 3\,\alpha^2\,q\,x + 2\,r = 0,$$

and the formula (1.) expresses the complete solution of the equation

$$(x^3 - 2\,r)^3 - 27\,q^3\,x^3 = 0,$$

which is of 9 dimensions. It is the formula

$$x = u + \frac{q}{u}, \text{ where } u = \{r + \surd(r^2 - q^3)\}^{\frac{1}{3}},$$

and has the same value in both terms of the expression, which corresponds to the equation $$x^3 - 3\,q\,x + 2\,r = 0.$$

This mode of effecting the solution of a cubic equation would altogether fail if the original equation possessed all its terms: and though the absence of the second term of a cubic equation cannot be said, in a certain sense at least, to affect the generality of its character, yet it would lead us to expect that the method which we had followed was of so limited a nature as not to be applicable to general equations of a higher order. Thus, if it was proposed to find two quantities, u and v, the sum of whose n^{th} powers was equal to $2r$, and whose product was equal to q, we should find

$$u = \{r + \sqrt{(r^2 - q^n)}\}^{\frac{1}{n}};$$

$$u + v = \{r + \sqrt{(r^2 - q^n)}\}^{\frac{1}{n}} + \frac{q}{\{r + \sqrt{(r^2 - q^n)}\}^{\frac{1}{n}}},$$

where $u + v$ is the root of the equation

$$x^n - n\,q\,x^{n-2} + \frac{n(n-3)}{1 \,.\, 2}\,q^2\,x^{n-4} - \frac{n(n-3)(n-4)}{1 \,.\, 2 \,.\, 3}\,q^3\,x^{n-6},$$
$$+ \text{ \&c.} = 2r^*.$$

The form of this equation is of such a kind as to prevent its being identified with any *general* equation whatever, beyond a cubic equation wanting the second term; a circumstance which precludes all further attempts, therefore, to exhibit the roots of higher equations by radicals † of this very simple order: but it is possible that there may exist determinate functions of the roots of higher equations (not symmetrical functions of all of them, which are invariable as far as the permutations of the roots amongst each other are concerned,) which may admit of triple values only, and which will be expressible, therefore, by means of a cubic equation, and consequently by the general formula for its solution.

Thus, if x_1, x_2, x_3, x_4, were assumed to represent the roots of a biquadratic equation

* This equation was first solved by Demoivre in the *Philosophical Transactions* for 1737, and it was readily derived from the theorem which goes by his name. It was afterwards shown to be true, by a process, however, not altogether general, by Euler, in the sixth volume of the *Comment. Acad. Petrop.*, p. 226. See also Abel's "Mémoire sur une Classe particulière d'Equations résolubles algébriquement," in *Crelle's Journal*, vol. iv.

† Abel has used the term *radicality* to designate such expressions. To say, therefore, that the root of an equation is expressible by *radicalities*, is the same thing as to say that the equation is solvable *algebraically*. It is used in contradistinction to such transcendental functions, whether of a known or unknown nature, as may, *possibly*, be competent to express those roots, when all general algebraical methods fail to determine them.

$$x^4 - p\,x^3 + q\,x^2 - r\,x + s = 0, \qquad (1.)$$

such functions would be $x_1 x_2 + x_3 x_4$ and $(x_1 + x_2 - x_3 - x_4)^2$, which admit but of three different values, and which may severally form, therefore, the roots of cubic equations, whose coefficients are expressible in terms of the coefficients of the original equation. Such a function also would be $(x_1 + x_2)^2$, if we should suppose p or the coefficient of the second term of equation (1.) to be zero *. The function $(x_1 + x_2)(x_3 + x_4)$ would give three values only under all circumstances. The functions $x_1 + x_2 + x_3$ and $x_1 x_2 x_3$ are capable of four different values, and therefore do not admit of being expressed by a determinable equation of lower dimensions than the primitive equation. Functions of the form $x_1 x_2$ admit of six values, and require for their expression equations of *six* dimensions, which are reducible to *three*, in consequence of being *quasi recurring* equations †. Innumerable functions may be formed which admit of 12 and of 24 values, and one *alternate* function which admits of two values only ‡.

The success of such transformations in reducing the dimensions of the equation to be solved, would naturally direct us to the research of similar functions of the roots of higher equations than the fourth, which admit of values whose number is inferior to the dimensions of the equation. We may presume that, if such functions exist, they are rational functions, for if not, their *irrationality* would increase the dimensions of the reducing equation, and would tend to distribute its roots into cyclical periods; and what is more, it has been very clearly proved that if equations admit of algebraical solution, all the algebraical functions which are jointly or separately involved in the expression of their roots, will be equal to rational

* The first of these transformations involves the principles of Ferrari's, sometimes called Waring's, solution of biquadratic equations; the second that of Euler; and the third that of Des Cartes. See the third chapter of Meyer Hirsch's *Sammlung von Aufgaben aus der Theorie der algebraischen Gleichungen*, which contains the most complete collection of formulæ and of propositions relating to symmetrical and other functions of the roots of equations with which I am acquainted. The combinatory analysis receives its most advantageous and immediate applications in investigations connected with the theory of such functions. See also Peacock's *Algebra*, note, p. 619.

† The form of its roots being u and $\frac{s}{u}$, they are reducible by the same methods as are applied to *recurring* equations.

‡ See Cauchy, *Cours d'Analyse*, chap. iii. and note iv. The use of such alternate functions in the elimination of the several unknown quantities from n simultaneous equations of the first order, involving n unknown quantities, will be noticed hereafter.

functions of these roots; and consequently, if irrational functions of those roots are employed in the formation of the reducing equation, the roots of the equation must enter into the final expression of the required roots, in a form where that irrationality has altogether disappeared *. If we assume, therefore, that such functions are in all cases rational, the next question will be, whether they are discoverable in higher equations than the fourth.

This inquiry was undertaken by Paolo Ruffini, of Modena, in his *Teoria delle Equazione Algebraiche*, published at Bologna in 1799, and subsequently in the tenth volume of the *Memorie della Società Italiana*, in a memoir on the impossibility of solving equations of higher degrees than the fourth. He has demonstrated that the number of values of such functions of the roots of an equation of n dimensions must be either equal to $1 . 2 . 3 \ldots n$, or to some submultiple of it; and that when $n = 5$, there is no such function, the *alternate* function being excluded, which possesses less than 5 values. The process of reasoning which is employed by the author for this pur-

* This proposition has been proved by Abel, in his *Beweis der Unmöglichkeit algebraische Gleichungen von höheren Graden als dem vierten allgemein Aufzulösen*, in the first volume of *Crelle's Journal*: the same demonstration was printed at Paris, in a less perfectly developed form, during his residence in that capital. This proof applies to *algebraical* solutions only, excluding the consideration of the possibility of expressing such roots by the aid of unknown transcendents. After defining the most general form of algebraical functions of any assigned degree and order; and after demonstrating the proposition referred to in the text, and analysing the demonstrations of Ruffini and Cauchy, and showing their precise bearing upon the theory of the solution of equations, he proceeds to show that the hypothesis of the existence of such a solution in an equation of five dimensions will necessarily lead to an equation, one member of which has 120 values and the other only 10; an absurd conclusion. It is quite impossible to exhibit this demonstration in a very abridged form so as to make it intelligible; and though some parts of it are obscure and not perfectly conclusive, yet it is, perhaps, as satisfactory, upon the whole, as the nature of the subject will allow us to expect.

It is impossible to mention the name of M. Abel in connexion with this subject, without expressing our sense of the great loss which the mathematical sciences have sustained by his death. Like other ardent young men, he commenced his career in analysis by attempting the general solution of an equation of five dimensions, and was for some time seduced by glimpses of an imagined success; but he nobly compensated for his error by furnishing the most able sketch of a demonstration of its impossibility which has hitherto appeared. His subsequent discoveries in the theory of elliptic functions, which were almost simultaneous with those of Jacobi, have contributed most materially to change the whole aspect of one of the most difficult branches of analytical science, and furnish everywhere proofs of a most vigorous and inventive genius. He died of consumption, at Christiania in Norway, in 1827, in the 27th year of his age.

pose is exceedingly difficult to follow, being unnecessarily encumbered with vast multitudes of forms of combination, and requiring a very tedious and minute examination of different classes of cases; and it was, perhaps, as much owing to the necessary obscurity of this very difficult inquiry as to any imperfection in the demonstration itself, that doubts were expressed of its correctness by Malfatti * and other contemporary writers. The subject, however, has been resumed by Cauchy in the tenth volume of the *Journal de l'Ecole Polytechnique,* who has fully and clearly demonstrated the following proposition, which is somewhat more general than that of Ruffini: "That the number of different values of any rational function of n quantities, is a submultiple of $1 \,.\, 2 \,.\, 3 \ldots n$, and cannot be reduced below the greatest prime number contained in n, without becoming equal to 2 or to 1." If we grant, therefore, the truth of this proposition, it will be in vain to seek for the reduction of equations of higher dimensions than the fourth, by transformations dependent upon rational functions of the roots.

The establishment of this proposition forms an epoch in the history of the progress of our knowledge of the theory of equations, in as much as it so greatly limits the objects of research in attempts to discover the general methods for their solution. And if the demonstration of Abel should be likewise admitted, there would be an end of any further prosecution of such inquiries, at least with the views with which they are commonly undertaken.

Lagrange, in his incomparable analysis of the different methods which have been proposed for the solution of biquadratic and higher equations, has shown their common relation to each other, and that they all of them equally tend to the formation of a reducing equation, whose root is

$$x_1 + \alpha\, x_2 + \alpha^2\, x_3 + \alpha^3\, x_4 + \text{\&c.}$$

where x_1, x_2, x_3, &c., are the roots of the primitive equation, and where α is a root of the equation

$$\alpha^{n-1} + \alpha^{n-2} + \alpha^{n-3} + \ldots \alpha + 1 = 0,$$

where n expresses the dimensions of the equation to be solved.

He then reverses the inquiry, and assuming this form as correctly representing the root of the *reducing* equation, he seeks to determine its dimensions. The beautiful process which he has employed for this purpose is so well known † that it is quite unnecessary to describe it in this place; and the result,

* *Memorie della Soc. Ital.*, tom. xi.

† *Résolution des Equations Numériques,* Note xiii.

as might be expected, perfectly agrees with the conclusions which are derived from more direct, and, perhaps, more general considerations. If n, or the number of roots x_1, x_2, x_3, &c., be a prime number, then the dimensions of the final reducing equation will be $1 \,.\, 2 \ldots (n-2)$; and if n be a composite number $= m\,p$, then the dimensions of the final reducing equation will be

$$\frac{1 \,.\, 2 \ldots n}{(m-1)\, m \,.\, (1 \,.\, 2 \ldots p)^m} \text{ or } \frac{1 \,.\, 2 \ldots n}{(p-1)\, p \,.\, (1 \,.\, 2 \ldots m)^p},$$

according as we arrive at it, by grouping the terms of the expression

$$x_1 + \alpha\, x_2 + \alpha^2\, x_3 + \text{ \&c.}$$

into m periods of p terms, or into p periods of m terms. It thus appears, that for an equation of 5 dimensions, the final reducing equation is of 6 dimensions; for an equation of 6 dimensions, the final reducing equation is of 10 dimensions in one mode of derivation and 15 in the other; and the higher the dimensions of the equation are, the greater will be the excess of the dimensions of the final reducing equation. And in as much as there exist no periodical or other relations amongst the roots of these reducing equations, it is obvious that the application of this process, and therefore also of any of those primary methods which lead to the assumption of the form of the roots of the reducing equation, must increase instead of diminishing the difficulties of the solution which was required to be found.

It was the imagined discovery of a cyclical period amongst the roots of this reducing equation which induced Meyer Hirsch, a mathematician of very considerable attainments, to believe that he had discovered methods for the general solution of equations of the fifth and higher degrees. Amongst the different methods which Lagrange has analysed in the *Berlin Memoirs* is that which Tschirnhausen proposed in the *Acta Eruditorum* for 1683. It proposed to exterminate, by means of an auxiliary equation, all the terms of the original equation except the first and the last, and thus to reduce it to a binomial equation. Thus, in order to exterminate the second term of $x^2 + a\,x + b = 0$, we must employ the auxiliary equation $y + \mathrm{A} + x = 0$, and then eliminate x. To exterminate simultaneously the second and third terms of the cubic equation $x^3 + a\,x^2 + b\,x + c = 0$, we must employ the auxiliary equation $y + \mathrm{A} + \mathrm{B}\,x + x^2 = 0$, and then eliminate x; and more generally, to destroy all the intermediate terms of an equation of n dimensions,

$$x^n + a_1\, x^{n-1} + a_2\, x^{n-2} + \ldots a_n = 0,$$

we must employ the auxiliary equation

$$y + A + A_1 x + A_2 x^2 + \ldots x^{n-1} = 0,$$

whose dimensions are less by 1 than those of the given equation.

Such a process is apparently very simple and uniform and equally applicable to all equations; and so it appeared to its author. But it will be found that the equations upon which the determination of A, A_1, A_2, depend, in an equation of the fourth degree, will rise to the sixth degree, which are subsequently reducible to others of the third degree; and that for an equation of the fifth degree, it will be impossible to reduce them below the sixth degree. Such was the decision of Lagrange, who has subjected this process to a most laborious analysis, and who has actually calculated one of the coefficients of the final reducing equation, and shown the mode in which the others may be determined*.

Meyer Hirsch, however, though fully adopting the conclusions of Lagrange to this extent, attempted to proceed further; and, deceived by the form which he gave to his *types* of combination, imagined that he had discovered cyclical periods amongst the roots of this final equation, by which it might be resolved into two equations of the third degree. If such a distribution of the roots was practicable in the case of the final equation corresponding to equations of the fifth degree, it would be practicable in that corresponding to equations of higher degrees. But some consequences of this discovery, and particularly the multiplicity of solutions which it gave, would have startled an analyst whose prudence was not laid asleep by the excitement consequent upon the expected attainment of a memorable advancement in analysis, which had eluded the grasp even of Lagrange. Its author, however, was too profound an analyst to continue long ignorant at once of the consequences of his error and of the source from which it sprung. In the Preface to his *Integraltafeln*, an excellent work, which was published in 1810, within two years of the announcement of his discovery, he acknowledges with great modesty and propriety, that he had not succeeded in effecting general solutions of equations in the sense in which the problem was understood by Euler, Lagrange, and the greatest analysts.

The well known Hoëne de Wronski, in a short pamphlet published in 1811, announced a method for the general resolution of equations. He assumes hypothetical expressions for the roots of the given equation in terms of the n roots of 1, and of the $(m-1)$

* In the *Berlin Memoirs* for 1771, p. 170: it forms a work of prodigious labour, such as few persons would venture to undertake or to repeat.

roots of a reduced equation of $(n-1)$ dimensions, and employs in the determination of the coefficients of this reduced equation n^{n-1} *fundamental* equations, designated by the Hebrew letter $\aleph$, and n^{n-2} others designated by the Greek letter Ω. It is unnecessary, however, to enter upon an examination of the truth of processes which the author who proposes them has left undemonstrated; and in as much as the application of his method to an equation of 5 dimensions would require the formation of 625 fundamental equations of the class Aleph and 125 of the class Omega, and the determination of the greatest common measure of 2 polynomials of 24 and 30 dimensions respectively, it was quite clear that M. Wronski might in perfect safety retire behind an intrenchment of equations and operations of this formidable nature. And this was the position which he took in answer to M. Gergonne, who, in the third volume of the *Annales de Mathématiques*, in the modest form of *doubts*, showed that the form of the roots which he had assumed was not essentially different from those which Waring, Bezout, and Euler, had assumed, and which Lagrange had shown to be incompatible with the existence of a final reducing equation of the dimensions assigned to it*.

The process given by Lagrange for determining the dimensions and nature of the final reducing equation has been the touchstone by which all the methods which have been hitherto proposed for the solution of equations have been tried, and will probably continue to serve the same purpose for all similar attempts which may be hereafter made. Its illustrious author, however, hesitated to pronounce a decisive opinion respecting the possibility of the problem, contenting himself with demonstrating it to be so, with reference to every method which had been suggested, or which could be shown to arise naturally out

* The works of Hoëne de Wronski were received with extraordinary favour in Portugal, where the Baron Stockler, a mathematician of considerable attainments, and other members of the Academy of Sciences became converts to his opinions. There is, in fact, a bold and imposing generality, and apparent comprehensiveness of views in his speculations, which are well calculated to deceive a reader whose mind is not fortified by the possession of an extensive and well digested knowledge of analysis. In the year 1817, the Academy of Sciences at Lisbon proposed as a prize, "The demonstration of Wronski's formulæ for the general resolution of equations," which was adjudged in the following year to an excellent refutation of their truth by the academician Evangelista Torriani: it chiefly consists in showing, and that very clearly, that the coefficients of the reducing equation of $(n-1)$ dimensions, assuming the form of the roots of the equation which Wronski assigned to them, cannot be symmetrical functions of those roots, and therefore cannot be expressed by the coefficients of the primitive equation, whatever be the number, nature and derivation of the fundamental equations $\aleph$ and Ω which are interposed.

of the conditions of the problem itself. But even if we should assume the impossibility of the problem, to the full extent of Abel's demonstration, it is still possible that there may exist solutions by means of undiscovered transcendents. It is, in fact, quite impossible to attempt to limit the resources of analysis, or to demonstrate the nonexistence of symbolical forms which may be competent to fulfil every condition which the solution of this problem may require. In conformity with such views, we may consider the numerical roots of equations as the only discoverable values of such transcendental functions; but it is quite obvious that such values will in no respect assist us in determining their nature or symbolical form, in the absence of any knowledge of the course of successive operations upon all the coefficients of the equation which were required for their determination.

Though we may venture to despair, at least in the present limited state of our knowledge of transcendental functions, of ever effecting the general resolution of equations, in the large sense in which that problem is commonly proposed and understood, yet there are large classes of equations of all orders which admit of perfect algebraical solution. The principal properties of the roots of the binomial equation $x^n - 1 = 0$, had long been ascertained by the researches of Waring and Lagrange, and its general *transcendental* solution had been completely effected. Its *algebraical* solution, however, had been limited to values of n not exceeding 10; and though Vandermonde in some very remarkable researches *, which were contemporary with those of Lagrange, had given the solution of the equation $x^{11} - 1 = 0$, as a consequence of his general method for the solution of equations, and had asserted that it could be extended to those of higher dimensions, yet his solution contained no developement of the peculiar theory of such binomial equations, and was so little understood, that his discovery, if such it may be termed, remained a barren fact, which in no way contributed to the advancement of our analytical knowledge.

The appearance of the *Disquisitiones Arithmeticæ* of the

* *Mémoires de l'Académie de Paris* for 1771. The result only of this solution was given, the steps of the process by which it was obtained being omitted. This result has been verified by Lagrange in Note xiv. to his *Traité sur la Résolution des Equations Numériques*. Poinsot, in a memoir on the solution of the *congruence* $x^n - 1 = M(p)$, which will be noticed in the text, has attempted to set up a prior claim in favour of Vandermonde for Gauss's memorable discovery; in doing so, however, he appears to have been more influenced by his national predilections in favour of his countrymen, than by a strict regard to historical truth and justice.

celebrated Gauss, in 1801, gave an immense extension to our knowledge of the theory and solution of such binomial equations. It was well known that the roots of the equation $\frac{x^n-1}{x-1}=0$, where n is a prime number, could be expressed by the terms of the series

$$r + r^2 + r^3 + \ldots r^{n-1},$$

where r represented any root *whatever* of the equation, and where, consequently, the first term r might be replaced by any term of the series. But in this form of the roots there is presented no means of distributing them into cyclical periods, nor even of ascertaining the existence of such periods or of determining their laws. It was the happy substitution of a geometrical series formed by the successive powers of a primitive root* of n, in place of the arithmetical series of natural numbers, as the indices of r, which enabled him to exhibit not merely all the different roots of the equation $\frac{x^n - 1}{x - 1} = 0$, but which also made manifest the cyclical periods which existed amongst them. Thus, if a was a primitive root of n, and $n - 1 = m\,k$, then in the series

$$r, r^a, r^{a^2}, r^{a^3}, \ldots r^{a^{k-1}}, \ldots r^{a^{mk-1}},$$

the m successive series which are formed by the selection of every k^{th} term, beginning with the first, the second, the third, and so on successively, or the k successive series which are formed in a similar manner by the selection of every m^{th} term, are periodical; and if the number m or k of terms in one of those periods be a composite number, they will further admit of resolutions into periods in the same manner with the complete series of roots of the equation. The terms of such periods will be reproduced in the same order, if they are produced to any extent according to the same law, it being understood that the multiples of n which are included in the indices which successively arise, are rejected, for the purpose of exhibiting their values and their laws of formation in the most simple and obvious form. If two or more periods also are multiplied together, the product will be composed of complete periods or of 1, or of multiples of them, the rules for whose determination are easily

* There are as many primitive roots of n as there are numbers less than $n - 1$ which are prime to it. Euler, who first noticed such primitive roots as determined by Fermat's theorem, determined them by an empirical process. Mr. Ivory, in his admirable article on Equations, in the Supplement to the *Encyclopædia Britannica*, has given a rule for finding them directly.

framed *; and it arises from the application of such rules that we are enabled to determine the coefficients of an equation of which those periods are the roots, and thus to depress the original binomial equation to one whose dimensions are the greatest prime number, which is a divisor of $n - 1$.

It follows, therefore, that if the highest prime factor of $n - 1$ be 2, the resolution of the binomial equation $x^n - 1 = 0$ will be made to depend upon the solution of quadratic equations only, and consequently to depend upon constructions which can be effected by combinations of straight lines and circles, and therefore within the strict province of plane geometry: this will take place whenever n is equal to $2^k + 1$ and is also a *prime* number. Thus, if $k = 4$ we have $n = 17$, a prime number, and therefore the solution of the equation $x^{17} - 1 = 0$ will be reducible to that of four quadratic equations. Similar observations apply to the equations

$$x^{2^8 + 1} - 1 = 0 \text{ and } x^{2^{16} + 1} - 1 = 0.$$

The same principles which enable us to solve algebraically binomial equations, under the circumstances above noticed, will admit of extension to other classes of equations, whose roots admit of analogous relations amongst each other. Gauss † has stated that the principles of his theory were applicable to functions dependent upon the transcendent $\int \frac{dx}{\sqrt{(1 - x^4)}}$, which defines the arcs of the lemniscata, as well as to various species of congruencies; and he has also partially applied them to certain classes of equations dependent upon angular sections, though in a form which is very imperfectly and very obscurely developed. Abel, however, in a memoir ‡ which is remarkable for the generality of its views and for its minute and careful analysis, has not merely completed Gauss's theory, but made most important additions to it, particularly in the solution of extensive classes of equations which present themselves in the theory of elliptic transcendents §. Thus he has given the complete

* Symmetrical functions of these periods will be multiples of the sum (-1) of these periods and of 1. This conclusion follows immediately from the replacement of the arithmetical by the geometrical series of indices, according to the general process of Lagrange, without any antecedent distribution of the roots into periods. See Note xiv. to the *Résolution des Equations Numériques*. It follows from thence that the coefficients of the reducing equations will be whole numbers.

† *Disquisitiones Arithmeticæ*, pp. 595, 645.

‡ "Sur une Classe particulière d'Equations résolubles algébriquement,"—*Crelle's Journal*, vol. iv. p. 131.

§ *Crelle's Journal*, vol. iv. p. 314, and elsewhere.

algebraical resolution of an equation whose roots can be represented by

$$x,\ \theta\, x,\ \theta^2\, x,\ \ldots\ \theta^{\mu-1}\, x,$$

where $\theta^{\mu}\, x = x$, and where θ is a rational function of x and of known quantities; and also of an equation where all the roots can be expressed rationally in terms of one of them, and where, if $\theta\, x$ and $\theta_1\, x$ express any other two of the roots, we have likewise

$$\theta\, \theta_1\, x = \theta_1\, \theta\, x.$$

It is impossible, however, within a space much less than that of the memoir itself, to give any intelligible account of the process followed in the demonstration of these propositions, and of many others which are connected with them. We shall content ourselves, therefore, with a slight notice of their application to circular functions.

If we suppose $a = \frac{2\pi}{\mu}$, the equation whose roots are $\cos a$, $\cos 2a$, $\cos 3a, \ldots \cos \mu a$ is

$$x^{\mu} - \frac{1}{4} \cdot x^{\mu-2} + \frac{1}{16} \cdot \frac{\mu(\mu-3)}{1\,.\,2}\, x^{\mu-4} \ldots = 0 \qquad (1.)$$

which may be easily shown to possess the required form and properties;—for, in the first place, $\cos m\, a = \theta(\cos a)$, where θ is, as is well known, a rational function of $\cos a$ or x; and, in the second place, if $\theta\, x = \cos m\, a$ and $\theta_1\, x = \cos m_1\, a$, then likewise $\theta\, \theta_1\, x = \cos m\, m_1\, a = \cos m_1\, m\, a = \theta_1\, \theta\, x$, which is the second condition which was required to be fulfilled.

Let us suppose $\mu = 2n + 1$, when the roots of the equation (1.) will be

$$\cos \frac{2\pi}{2n+1},\ \cos \frac{4\pi}{2n+1},\ \ldots\ \cos \frac{4n\pi}{2n+1},\ \cos 2\pi,$$

of which the last is 1, and the n first of the remainder equal to the n last. The equation (1.) may be depressed, therefore, to one of n dimensions, which is

$$x^n + \frac{1}{2}\, x^{n-1} - \frac{1}{4}(n-1)\, x^{n-2} - \frac{1}{8}(n-2)\, x^{n-3}$$

$$+ \frac{1}{16} \cdot \frac{(n-2)(n-3)}{1\,.\,2}\, x^{n-4} + \frac{1}{32} \cdot \frac{(n-3)(n-4)}{1\,.\,2}\, x^{n-5} - \&c. = 0 \quad (2.)$$

whose roots are

$$\cos \frac{2\pi}{2n+1},\ \cos \frac{4\pi}{2n+1},\ \ldots\ \cos \frac{2n\pi}{2n+1}.$$

If $\cos \frac{2\pi}{2n+1} = x = \cos a$, and $\cos \frac{2m\pi}{2n+1} = \theta x = \cos m a$, then these roots are reducible to the form

$$x, \theta x, \theta^2 x, \ldots \theta^{n-1} x,$$

or,

$$\cos a, \cos m a, \cos m^2 a, \ldots \cos m^{n-1} a:$$

and if we suppose m to be a primitive root to the modulus $2n+1$, then all the roots

$$\cos a, \cos m a, \cos m^2 a, \ldots \cos m^{n-1} a$$

will be different from each other, and $\cos m^n a = \cos a$; consequently it will follow, since the roots of the equation (2.) are of the form

$$x, \theta x, \theta^2 x, \ldots \theta^{n-1} x,$$

where $\theta^n x = x$, they will admit, in conformity with the preceding theorems, of algebraical expression.

Abel has given the general form of the expression for these roots, which in this case are all real; and their determination will involve the division of a circle into $2n$ equal parts, the division of an assigned or assignable arc into $2n$ equal parts, and the extraction of the square root of $2n+1$; a conclusion to which Gauss had also arrived, though he has not given the steps of the process which he followed for obtaining it*. If we suppose $2n = 2^{\omega}$, we shall get the case of regular polygons of $2^{\omega+1} + 1$ sides, which admit of indefinite inscription in circles by purely geometrical means. It will follow from the same result that the inscription of a heptagon will depend upon that of a hexagon, the trisection of a given angle, and the extraction of the square root of 7.

Poinsot† has given a very remarkable extension to the theory of the solution of the binomial equation $x^n - 1 = 0$, by showing that its imaginary roots may be considered in a certain sense as the analytical representation of the whole numbers which satisfy the *congruence* or equation

$$x^n - 1 = \mathrm{M}(p),$$

whose *modulus* (a prime number) is p: thus, the imaginary cube roots of 1, or the imaginary roots of $x^3 - 1 = 0$, are $\frac{-1+\sqrt{-3}}{2}$, $\frac{-1-\sqrt{-3}}{2}$, and the whole numbers 4 and 2,

* *Disquisitiones Arithmeticæ*, p. 651.

† *Journal de l'Ecole Polytechnique*, cahier 18.

which satisfy the congruence

$$x^3 - 1 = \mathrm{M} \times 7,$$

whose modulus is 7, are expressed by $\dfrac{-1+7+\sqrt{-3+7}}{2}$ and $\dfrac{-1+7-\sqrt{-3+7}}{2}$, which arise from adding 7 to the parts without and beneath the radical sign.

The principle of this transition from the root of the equation to that of the congruence is sufficiently simple. We consider the roots of $x^n - 1 = 0$ as resulting from the expression for those of the congruence $x^n - 1 = \mathrm{M}(p)$, when $\mathrm{M} = 0$; and we thus are enabled to infer, in as much as $\mathrm{M}(p)$, its multiples and powers, are involved in those formulæ, whether without or beneath the radicals, and disappear, therefore, when $\mathrm{M} = 0$, that some such multiples, to be determined by trial, or otherwise, are to be added when $\mathrm{M}(p)$ is restored, or when 1 is replaced by $1 + \mathrm{M}(p)$. When the congruence admits of integral values of x, which are less than p, then they can be found by trial: when no such integral values exist, then, amongst the irrational values which thus arise, those values will present themselves which will satisfy the congruence algebraically, though they can only be ascertained by a tentative process.

The equation of Fermat,

$$x^{p-1} - 1 = \mathrm{M}(p),$$

where p is a prime number, will be satisfied by all the natural numbers 1, 2, 3, . . as far as $(p - 1)$: and it follows, therefore, that all the rational roots of the equation

$$x^n - 1 = \mathrm{M}(p)$$

will be common to the equation

$$x^{p-1} - 1 = \mathrm{M}(p),$$

the number of them being equal to (d), the greatest common divisor of n and of $p - 1$. If d be 1, then all the roots except 1 are irrational. If we suppose the equation to be

$$x^p - 1 = \mathrm{M}(p),$$

then all the roots will be equal to each other and to 1. It is unnecessary, however, to enter upon the further examination of such cases, which are developed with great care and singular ingenuity in the memoir referred to.

These views of Poinsot are chiefly interesting and valuable as connecting the theory of indeterminate with that of ordinary equations. It has, in fact, been too much the custom of analysts to consider the theory of numbers as altogether separated from that of ordinary algebra. The methods employed have generally been confined to the specific problem under consideration, and have been altogether incapable of application when the known quantities employed were expressed by general symbols and not by specific numbers. It is to this cause that we may chiefly attribute the want of continuity in the methods of investigation which have been pursued, and the great confusion which has been occasioned by the multiplication of insulated facts and propositions which were not referable to, nor deducible from, any general and comprehensive theory.

Libri, in his *Teoria dei Numeri,* and in his *Mémoires de Mathématique et de Physique,* has not merely extended the views of Poinsot, but has endeavoured to comprehend all those conditions in the theory of numbers, by means of algebraical or transcendental equations, which were previously understood merely, and not symbolically expressed. He has shown that problems which have been usually considered as *indeterminate* are really more than *determinate,* and he has thus been enabled to explain many anomalies which had formerly embarrassed analysts, by showing the necessary existence of an *equation of condition,* which must be satisfied, in order that the problem required to be solved may be possible. By the aid of such principles the solutions of indeterminate equations, at least within finite limits, may be found directly, and without the necessity of resorting to merely tentative processes.

A great multitude of new and interesting conclusions result from such views of the theory of numbers; but the limits and object of this Report will not allow me to discuss them in detail, or to point out their connexion with the general theory of equations, and with the properties of circular and other functions. The reader, however, will find, in the second of the memoirs of Libri above referred to, a general sketch of the nature and consequences of these researches, which is unfortunately, however, too rapid and too imperfectly developed to put him in full and satisfactory possession of all the bases of this most important theory.

On the Solution of Numerical Equations.—The resolution of numerical equations formed the subject of a truly classical work by Lagrange, in which this problem, one of the most important in algebra, is not only completely solved, but the imperfections of all the methods which had been proposed for this purpose by other authors are pointed out with that singular distinctness and elegance which always distinguish his reviews of the progress and existing state of the different branches of the mathematical sciences. In the following report we shall commence by a general account of the state in which the problem was left by him, and of the practical difficulties which attend the use of his methods, and we shall then proceed to notice the important labours of Fourier and other authors, with a view to bring its solution within the reach of arithmetical processes which are at once general and easy of application.

The resolution of numerical equations involves two principal objects of research: the first of them concerns the separation of the roots into real and imaginary, positive and negative, and the determination of the limits between which the real roots are severally placed; the second regards the actual numerical approximation to their values, when their limits and nature have been previously ascertained. Many different methods have been proposed for both these objects, which differ greatly from each other, both in their theoretical perfection and in their practical applicability. We shall begin with a notice of the first class of methods, which have been proposed for the separation of the roots.

If the coefficients of an equation be whole numbers or rational fractions, their real roots will be either whole numbers or rational fractions, or otherwise irrational quantities, which will be generally *conjugate** to each other and which will generally present themselves, therefore, in pairs. The method of divisors which Newton proposed, and which Maclaurin perfected, will enable us to determine roots of the first class, and they are also determined immediately and completely by nearly all methods of approximation. It will be to roots of the second class, therefore, that our methods of approximation will require to be applied, though such methods will never enable us to assign them under their finite irrational form, nor would our knowledge of their existence under such a form in any way aid us, unless in a very small number of cases, in the determination of their approximate numerical values.

The *equal* roots of equations, if any exist, may be detected by general methods; and the factors corresponding to them may be completely determined, and the dimensions of the equa-

* An irrational real root may be *conjugate* to the *modulus* of a pair of impossible roots; and there may exist, therefore, as many irrational real roots which have no corresponding conjugate *real* roots as there are pairs of impossible roots in the equation. It is not true, therefore, generally, as is sometimes asserted, that such irrational roots enter equations by pairs. It would not be very difficult to investigate the different circumstances under which roots present themselves, and the different conditions under which they can be conjugate to each other; but the inquiry is not very important, in as much as the knowledge of their form would not materially influence the application of methods for approximating to their values.

tion depressed by a number of units equal to the number of such factors. We might suppose, therefore, in all cases, that the roots of the equation to be solved were unequal to each other; but if it should not be considered necessary to perform the previous operations which are required for the detection and separation of the equal roots, the failure of the methods of approximation or other peculiar circumstances connected with the determination of the limits of the roots, would indicate their existence, and at once direct us to the specific operations upon which their determination depends.

If we suppose, therefore, the equal roots to be thus separated from the equation to be solved, and if we assume a quantity Δ which is less than the least difference of the unequal roots, then the substitution of the terms of the series

$$k\,\Delta,\ (k-1)\,\Delta,\ \ldots\ 2\,\Delta,\ \Delta,\ 0,\ -\Delta,\ -2\,\Delta,\ \ldots\ -k_1\,\Delta,$$

where $k\,\Delta$ is greater than the greatest root, and $-k_1\,\Delta$ less than the least root*, will give a series of results, amongst which the number of changes of sign from + to − and from − to + will be as many as the number of real roots, and no more; and where the pairs of consecutive terms of the series of multiples of Δ which correspond to each change of sign are limits to the several real roots of the equation. This is the principle of the method of determining the limits of the real roots which was first proposed by Waring, and which has been brought into practical operation by Lagrange and Cauchy. It remains to explain the different methods which have been proposed for the purpose of determining the value of Δ.

Waring first, and subsequently Lagrange, proposed for this purpose the formation of the equation whose roots are the squares of the differences of the roots of the given equation. If we subsequently transform this equation into one whose roots are the reciprocals of its roots, and determine a limit l greater than the greatest root of this transformed equation†, then $\frac{1}{\sqrt{l}}$

* A negative root is always considered as less than a positive root, unless the consideration of the signs of affection is expressly excluded.

† Newton proposed for this purpose the formation of the equation whose roots are $x - e$, and where e is determined by trial of such a magnitude that all the coefficients of the equation may become positive. In such a case e is the limit required. Maclaurin proved that the same property would belong to the greatest negative coefficient of the equation increased by 1. Cauchy, in his *Cours d'Analyse*, Note iii., and in his *Exercices des Mathématiques*, has shown that if the coefficients of the equation, without reference to their sign, be $A_1\ A_2,\ \ldots\ A_m$, and if n be the number of such coefficients which are different from zero, then that the greatest of the quantities

$$n\,A_1,\ (n\,A_2)^{\frac{1}{2}},\ (n\,A_3)^{\frac{1}{3}},\ \ldots\ (n\,A_m)^{\frac{1}{m}}$$

will be less than the least difference of any two of the real roots of the primitive equation, and will consequently furnish us with such a value of Δ as will enable us to assign their limits. The extreme difficulty, however, of forming the equation of differences, which becomes nearly impracticable in the case of equations beyond the fourth degree*, renders it nearly, if not altogether, useless for the purposes for which this transformation was intended by the illustrious analysts who first proposed it; in other words, it is only in a theoretical sense that it can be said to furnish the solution of the problem of determining the limits of the real roots of an equation.

Cauchy has succeeded in avoiding the necessity of forming the equation of the squares of the differences of the roots, by showing that a value of Δ may be determined from the last term of this transformed equation, combined with a value of a limit greater than the greatest root of the primitive equation. If we suppose H to represent this term, k to be the superior limit required, and a and b to represent any two roots of the equation, whether real or imaginary, then he has shown that their difference $a - b$, or the *modulus* of their difference, will be

will be a superior limit to the roots. An inferior limit (without reference to algebraical sign) may be readily found by the same process by the formation of the equation whose roots are the reciprocals of the former.

M. Bret, in the sixth volume of Gergonne's *Annales des Mathématiques*, has investigated other superior limits of the roots of equations, which admit of very easy application, and which likewise give results which are generally not very remote from the truth. One of these limits is furnished by the following theorem: "If we add to *unity* a series of fractions whose numerators are the successive negative coefficients, taken positively, and whose denominators are the sums of the positive coefficients, including that of the first term, the greatest of the resulting values will be a superior limit of the roots of the equation." Thus, in the equation

$$2x^7 + 11x^6 - 10x^5 - 26x^4 + 31x^3 + 72x^2 - 230x - 348 = 0,$$

the number 4, which is equal to the greatest of the quantities

$$1 + \frac{10}{13},\ 1 + \frac{26}{13},\ 1 + \frac{230}{116},\ 1 + \frac{348}{116},$$

is a superior limit required; and if we change the signs of the alternate terms, we shall have $1 + \frac{11}{2}$, or 7, a superior limit of the roots of the resulting equation: it will follow, therefore, that all the real roots of the first equation will be included between 4 and -7. Other methods are proposed in the same memoir which are not equally new or equally simple with the one just given, and which I do not think it necessary to notice.

* Waring, as is well known, gave the transformed equation of the 10th degree, whose roots were the squares of the differences of the roots of a general equation of the fifth degree, wanting its second term: it involves 94 different combinations of the coefficients of the original equation, many of them with large numerical coefficients.

greater than $\frac{H^{\frac{1}{2}}}{(2k)^{\frac{n(n-1)}{2}-1}}$, if n denote the dimensions of the equation; and in as much as H is necessarily, when the coefficients are whole numbers, either equal to or greater than 1, it will follow that $\frac{1}{(2k)^{\frac{n(n-1)}{2}-1}}$ will furnish a proper value of Δ, where k has been determined by the methods described above, or in any other manner. The chief objection to the use of a value of Δ thus determined arises from its being generally much too small, and from the consequent necessity of making a much greater number of trials for the discovery of the limits of the roots than would otherwise be necessary.

Lagrange has proposed different methods of determining the value of Δ, which, though much less laborious, at least for equations of high orders, than the equation of the squares of the differences, are still liable to great objections, in consequence of their being indirect, difficult of application, and likely to give values of Δ so small and so uncertain as greatly to multiply the number of trials which are necessary to be made*. It is for this and other reasons that such methods have never been reduced to such a form as to be competent to furnish the required limits by means of processes which are expressible in the form of arithmetical rules, like those which are given for the extraction of the square and cube root in numbers. In this respect, therefore, they have failed altogether in satisfying the great object proposed to be attained by their author, who considered the resolution of numerical equations as properly constituting a department of common arithmetic, the demonstration of whose rules of operation must be subsequently sought for in the general theory of algebraical equations†.

The basis of all methods of solution of numerical equations must be found in the previous separation of the roots; and the efforts of algebraists for the last two centuries and a half have been directed to the discovery of rules for this purpose. The methods, however, which have been proposed have been chiefly directed to the separation of the roots into classes, as positive and negative, real and imaginary, and not to the determination of the successive limits between which they are severally placed. The celebrated theorem of Des Cartes‡ gave a limit to the number of positive and negative roots, but failed in deter-

* *Résolution des Equations Numériques,* Note iv.

† *Ibid.,* Introduction.

‡ The proper enunciation of this theorem is the following: "Every equation has at *least* as many changes of sign from + to — and from — to + as it has real and positive roots, and at *least* as many continuations of sign

mining the absolute number either of one class or of the other, in the absence of any means of ascertaining the number of imaginary roots. If the roots of the equation were all of them real, and could be shown to be so by any independent test, it would be easy to determine the limits between which the roots were severally placed; for the number of changes of sign which are lost upon the substitution of $x + e$ for x would show the number of roots which are included between 0 and e; and if, therefore, we should assume a succession of values of e, whether positive or negative, such as to destroy one change of signs and *no more*, upon the substitution of any two of these successive values, we should be enabled to obtain the limits of every root of the equation.

It was chiefly with a view to this consequence of Des Cartes's theorem that De Gua investigated and assigned the conditions of the reality of all the roots of an equation. If we suppose $X = 0$ to be the equation, and X^{i}, X^{ii}, X^{iii}, X^{iv}, X^{v}, &c., to denote the successive differential coefficients of X, then, if all the roots of $X = 0$ be real, the roots of the several derivative equations $X^{i} = 0$, $X^{ii} = 0$, $X^{iii} = 0$, &c., must be real likewise; and if the roots of any one of these equations $X^{(r)} = 0$ be substituted in $X^{(r-1)}$ and $X^{(r+1)}$, it will give results affected with different signs. If we form, therefore, a succession of equations in y by eliminating successively x from the equations $y = X^{(n)} \,.\, X^{(n-2)}$ and $X^{(n-1)} = 0$,

$$y = X^{(n-1)} \,.\, X^{(n-3)} \text{ and } X^{(n-2)} = 0, \; \ldots \ldots$$

$$y = X^{i}\, X^{iii} \text{ and } X^{ii} = 0, \; y = X\, X^{ii} \text{ and } X^{i} = 0,$$

the coefficients of all these equations must be positive, forming

from + to + and from — to — as it has real and negative roots." It is very doubtful, notwithstanding the assertions of some authors, whether Des Cartes himself was aware of the necessary limitation of the application of this theorem, which is required by the possible or ascertained existence of imaginary roots.

The demonstration which was given by De Gua of this theorem in the *Mémoires de l'Académie des Sciences* for 1741, founded upon the properties of the limiting equation or equations, has been completed by Lagrange with his usual fullness and elegance, in Note viii. to his *Résolution des Equations Numériques*. The most simple and elementary, however, of all the demonstrations which have been given of it, and the one, likewise, which arises most naturally and immediately from the theory of the composition of equations, is that which was given by Segner in the *Berlin Memoirs* for 1756. The few imperfections which attach to this demonstration, as far as the classification of the forms which algebraical products may assume is concerned, have been completely removed in a demonstration which Gauss has published in the third volume of *Crelle's Journal*.

This theorem is included as a corollary to Fourier's more general theorem for the separation of the roots, as we shall have occasion to notice hereafter.

a collection of conditions of the reality of the roots of an equation of n dimensions which are $\frac{n(n-1)}{2}$ in number*.

These speculations of De Gua were well calculated to show the importance of examining the succession of signs of these derivative equations, with a view to the discovery of their connexion with the nature of the roots of the primitive equation. The changes in the succession of signs of the coefficients of the equations which resulted from the substitution of $x + a$ and $x + b$, gave no certain indications of the nature and number of the roots included between a and b, unless it could be shown that all the roots of the primitive equation were real, a case of comparatively rare occurrence, and which left the general problem of the separation of the roots, as preparatory to their actual calculation, nearly untouched. It was the conviction that all attempts to effect the solution of this problem by the aid of Des Cartes's theorem would necessarily fail, which led Fourier, one of the most profound and philosophical writers on analysis and physical science in modern times, to the examination of the

* *Résolution des Equations Numériques,* Note viii. The equation of the squares of the differences of the roots of an equation will indicate the reality of all the roots, if its coefficients have $\frac{n(n-1)}{2}$ changes of sign, or be alternately positive and negative. The succession of signs of the coefficients very readily furnishes the indications of the number of impossible roots in all equations as far as five dimensions, as has been shown by Waring and Lagrange.

The number of conditions of the reality of the roots of an equation of five dimensions would appear from the formula in the text to be 10; but some of these conditions, as Lagrange has intimated, may, and indeed are, included in the system of the others, so as to reduce them to a smaller number. Lagrange has assigned two conditions (not three) of the reality of the roots of a cubic equation; but the first of these is necessarily included in that of the second, so as to reduce the essential conditions to one. Similar consequences are found to present themselves in the examination of these conditions for an equation of the fourth degree, which are three in number, and not six, as the formula would appear to indicate.

Cauchy, in the 17th cahier of the *Journal de l'Ecole Polytechnique,* has succeeded, by a combined examination of the geometrical properties of the curve whose equation is $y = X$ (where X is a rational function of x of the form $x^n + p_1 x^{n-1} + \dots p_n$), and of their corresponding analytical characters, in the discovery of general methods, not merely for the determination of the number of real roots, but likewise of the number of positive and negative roots, as distinguished from each other. These methods are equally applicable to literal and numerical equations. He has applied his method to general equations of the first five degrees, and the results are in every respect, as far at least as they have been examined in common, equivalent to those which are derived from the equation of the squares of the differences. It is impossible, however, in the space which is allowed to me in this Report, to give any intelligible account of this most elaborate and able memoir, and I must content myself, therefore, with this general reference to it.

succession of signs of the function X and its derivatives, upon the substitution of different values of x. The conclusions which have resulted from this examination, which we shall now proceed to state, have completely succeeded in effecting the practical solution of this most difficult and important problem, as far, at least, as real roots are concerned.

If we suppose

$$X = x^m + a_1 x^{m-1} + a_2 x^{m-2} + \ldots a_m = 0,$$

and if we write X and its derivatives in the following order,

$$X^{(m)}, X^{(m-1)}, X^{(m-2)}, \ldots X^{ii}, X^{i}, X,$$

then the substitution of $\frac{1}{0}$ and $-\frac{1}{0}$, will give two series of results, the terms of the first series being all of them positive, and those of the second being alternately positive and negative. The same will be the case if, in the place of $\frac{1}{0}$, we put any limit (α) greater than the greatest root of the equation $X = 0$, and if in the place of $-\frac{1}{0}$ we substitute any negative value of x ($-\beta$) (to be determined by trial or otherwise) which will make the first terms of X, X^{i}, X^{ii}, &c., considered with regard to numerical value only, severally greater than the sum of all those which follow them. In the course of the substitution of values of x intermediate to those extreme values $-\beta$ and α, all the m changes of sign of X and its derivatives, from + to − and from − to +, will disappear, in conformity with the following theorems, which are capable of strict demonstration.

1st. If, upon the substitution of any value of x, one or more changes of signs disappear, those changes are not recoverable by the substitution of any greater value of x.

2nd. If upon the substitution of two values a and b of x, one change of signs disappears, there is one real root and no more included between a and b. If under the same circumstances an odd number $2p + 1$ of changes of sign have disappeared, there must be at least one, and there may be $2p' + 1$ (where p' is not greater than p) real roots between a and b; but if an even number $2p$ of signs have disappeared in the interval, there *may* be $2p - 2p'$ real roots, and p' pairs of imaginary roots corresponding to it, where p' is not greater than p. If no change of sign disappears, upon the successive substitution of a and b, then no root whatever of the equation $X = 0$ can be found between the limits a and b.

3rd. If the substitution of a value a of x makes $X = 0$, then a is a root of the equation. If the substitution of the same value of x makes at the same time $X = 0$ and $X^{i} = 0$, then there are two real roots equal to a; and generally, as many of the final functions X, X^{i}, X^{ii}, &c., will disappear, under the same circumstances, as there are roots equal to a.

4th. If the substitution of a value of a makes one intermediate function $X^{(r)}$ equal to 0, and one only, and if the result 0 be placed between two signs of the same kind, whether + and + or − and −, then there will be one pair of imaginary roots corresponding to this occurrence; but if 0 be placed between two unlike signs, + and − or − and +, then there will be no root corresponding to it, unless at the same time $X = 0$. If the substitution of a makes any number of consecutive derivative functions equal to 0, then, if there be an even number $2p$ of consecutive zeros, there will be p or $(p - 1)$ pairs of imaginary roots corresponding, according as they are placed between the same or different signs; and if there be an odd number $2p + 1$ of consecutive zeros, then there will be $p + 1$ or p pairs of imaginary roots corresponding, according as they are placed between the same or different signs *.

The preceding propositions may be easily shown to include the theorem of Des Cartes; for it is obvious that the substitution of 0 for x in X and its derivatives will give a succession of signs identical with those of the successive coefficients of X, deficient terms being replaced by 0. If the extreme values α and $-\beta$ be substituted, there will be m permanences in one case and m changes in the second; it will follow therefore that the number of real and therefore positive roots between α and 0 cannot exceed the number of changes of sign corresponding to $x = 0$, or amongst the successive coefficients of the equation; and that the number of real and therefore negative roots between $-\beta$ and 0 cannot exceed the number of permanences corresponding to $x = 0$, or of changes between 0 and $-\beta$, which is also identical with the number of successive permanences of sign amongst the coefficients of the equation.

* I have stated this rule differently from Fourier, whose *rule of the double sign* appears to me to be superfluous. If we consider the zeros as possessing arbitrary signs, the nature and extent of the ambiguity which they produce will always be determined by the circumstances of their position with respect to the preceding and succeeding sign.

The rule of the double sign, when one of the derivative functions X^{i}, X^{ii}, X^{iii}, &c., becomes equal to zero, is made use of in a memoir by Mr. W. G. Horner, in the *Philosophical Transactions* for 1819, upon a new method of solving numerical equations. This memoir, though very imperfectly developed, and in many parts of it very awkwardly and obscurely expressed, contains many original views, and also a very valuable arithmetical method of extracting the roots of *affected* equations. It makes also a very near approach to Fourier's method of separating the roots of equations. It is proper to state that Fourier's proposition was known to him as early as 1796 or 1797, as very clearly appears from M. Navier's Preface to his *Analyse des Equations Déterminées*, a posthumous work, which appeared in 1831.

In order to render the preceding propositions more easily intelligible, we will apply them to two examples.

Let $X = x^4 - 4x^3 - 3x + 23 = 0$, and underneath X^{iv}, X^{iii}, X^{ii}, X^{i}, X, let us write down the signs of the results of the substitution of 0, 1, 2, 3, 10, in the place of x, in conformity with the following scheme:

	X^{iv},	X^{iii},	X^{ii},	X^{i},	X,
(0)	+	−	0	−	+
(1)	+	0	−	−	+
(2)	+	+	0	−	+
(3)	+	+	+	−	−
(10)	+	+	+	+	+

For $x = 0$, there is a result 0 placed between two similar signs; there is therefore a pair of imaginary roots corresponding to it. Every value of x less than 0 will give results alternately + and −, and there is therefore no real negative root.

For $x = 1$, there is a result 0 placed between two dissimilar signs: there is therefore no pair of imaginary roots corresponding; and since there is no loss of changes of sign in passing from 0 to 1, there is no real root between those values.

For $x = 2$, there is a result 0 placed between two dissimilar signs; there is therefore no pair of imaginary roots corresponding, and there is no root between 1 and 2.

For $x = 3$, there is a loss of one change of sign, and there is therefore one real root between 2 and 3.

For $x = 10$, there is a loss of one change of signs and all the resulting signs are positive; there is therefore one real root between 3 and 10.

The limits of the real roots are thus completely determined, and the substitution of the successive whole numbers, from 3 upwards, will show the nearest whole numbers 3 and 4, between which the greatest root is situated.

Let $X = x^6 - 12x^5 + 60x^4 + 123x^2 + 4567x - 89012 = 0$

	X^{vi},	X^{v},	X^{iv},	X^{iii},	X^{ii},	X^{i},	X,
(−10)	+	−	+	−	+	−	+
(−1)	+	−	+	−	+	−	−
(0)	+	−	+	0	+	+	−
(1)	+	−	+	+	+	+	−
(10)	+	+	+	+	+	+	+

All the real roots of the equation are included between the extreme values − 10 and 10.

One change of sign is lost in the transition from -10 to -1, and there is therefore one real root between them; the sign of the last term is therefore necessarily changed from $+$ to $-$.

For $x = 0$, there is a result 0 between two similar signs; there is therefore a pair of imaginary roots corresponding, and consequently a loss of two changes of sign.

There is no root of the equation between 0 and 1.

There is a loss of three changes of sign in the transition from 1 to 10, and therefore there are three roots corresponding, one or all of which may be real: the application of a subsequent rule will show that two of them are imaginary.

It is obvious, in a series of derivatives, $X^{(m)}, X^{(m-1)}, \ldots X^{(r)} \ldots X$, that $X^{(m)}, X^{(m-1)}$ may be considered as the derivatives of the $(m-r-1)^{\text{th}}$ and $(m-r-2)^{\text{th}}$ order from $X^{(r)}$, as well as the m^{th} and $(m-1)^{\text{th}}$ derivatives from X, and that the same rules may be applied to the separation of the roots of these derivatives when they become equations, whether they be considered as belonging to the inferior or to the superior order. The substitution, therefore, of a and b successively for x, will show the number of roots of the successive derivative equations which are found in this interval, which will be equal successively to the number of changes of sign which have disappeared in the transition from one value of x to the other. If we now place under the several results of the substitution of a and b, a series of zeros or numbers as *indices* to signify that no change, or an indicated number of changes of signs, have disappeared, then in passing from the left to the right, we shall find first zero, and subsequently, whether immediately or not, the numbers, 1, 2, &c., which will indicate the number of roots which must be sought for, in that interval, in the derivative or other functions, considered as equations, which are severally placed above them. Thus, if $X = x^4 - x^3 + 4x^2 + x - 4 = 0$, then from the scheme

	X^{iv},	X^{iii},	X^{ii},	X^{i},	X,
(-10)	$+$	$-$	$+$	$-$	$+$
	0	0	0	0	1
(-1)	$+$	$-$	$+$	$-$	$-$
	0	0	0	1	0
(0)	$+$	$-$	$+$	$+$	$-$
	0	1	2	2	3
(1)	$+$	$+$	$+$	$+$	$+$

we infer that there is one root of $X = 0$, and no root of any of the several derivative equations situated between -10 and -1;

that there is one root of $X^{i} = 0$, and no root of $X = 0$, between -1 and 0; that there is one root of $X^{iii} = 0$, two roots of $X^{ii} = 0$, two roots of $X^{i} = 0$, and three roots of $X = 0$, situated between 0 and 1. It remains to determine whether these three roots are all of them real, or two of them imaginary, and also to assign the limits, in the first case*, between which they are placed.

In the first place, if imaginary roots exist in the derived, they will exist also in the primitive equation. The converse of this proposition is not necessarily true.

If the succession of indices be 0, 1, 2, then the succession of signs corresponding to

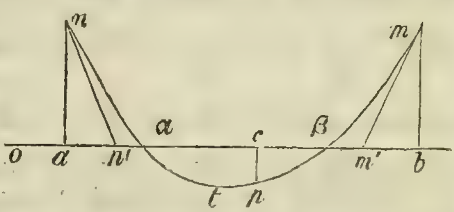
$$X^{ii},\ X^{i},\ X, \text{ or } X^{(r+1)},\ X^{(r)},\ X^{(r-1)},$$

will be

(*a*)	$+$	$-$	$+$	or	$-$	$+$	$-$
	0	1	2		0	1	2
(*b*)	$+$	$+$	$+$		$-$	$-$	$-$

There will be one real root between a and b in the equation $X^{i} = 0$ or $X^{(r)} = 0$, and two roots, whether real or imaginary, corresponding to this interval, in $X = 0$ or $X^{(r-1)} = 0$.

In the first case, if there be two real roots between a and b, then the curve whose equation is $y = X = f(x)$, where $o\,a = a$, $o\,b = b$, $a\,n = f(a)$, $b\,m = f(b)$, will cut the axis at the points α and β between a and b. The curve will have no point of inflection between a and b, since X^{ii} preserves the same sign, whether $+$ or $-$; and there will be a point t, where the tangent is parallel to the axis, since X^{i}, in the same interval, changes from $+$ to $-$, or conversely, and therefore becomes equal to *zero* between those limits. In this case, the sum of the subtangents (considered without regard to algebraical signs) will be necessarily less than $a\,b$; and if the interval $a\,b$ be subdivided sufficiently, so as to furnish new limits a' and b', then one or both of these points will sooner or later be found between the points of intersection α and β, and therefore $f(a')$ and $f(b')$ will one or both of them change their signs. The analytical expression of those geometrical conditions, and therefore of the existence of two real roots, will be, that the sum of the subtangents or quotients $\dfrac{f(a)}{f'(a)}$

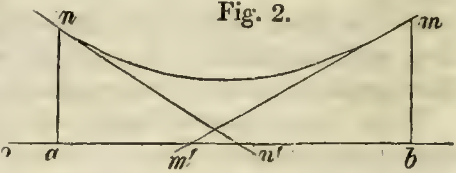


* We seek for the limits of the real roots only; we have no concern with those of the imaginary roots or of their *moduli*.

$+ \dfrac{f(b)}{f'(b)} \angle\, b - a$, when no regard is paid to the sign of $f'(a)$ and $f'(b)$. In this case new limits must be taken successively, intermediate to a and b, until $f(a')$ and $f(b')$ one or both of them change their sign.

In the second case, if there be two imaginary roots corresponding to the interval between a and b, then the curve whose equation is $y = \mathrm{X}$ though similar in its other geometrical properties to fig. 1, will not cut the axis between a and b. In this case the sum of the subtangents $a\,n'$ and $b\,m'$ will either exceed the interval $a\,b$, or will ultimately exceed it, when the interval $a\,b$ is sufficiently diminished. The corresponding analytical character will be that $\dfrac{f(a)}{f'(b)} + \dfrac{f(b)}{f'(b)}$ is either greater than $b - a$, or that it may ultimately be made to exceed it *.

Fig. 2.



Thus, in the example referred to above, p. 332, write down the following scheme:

	X^{iv},	$\mathrm{X}^{\mathrm{iii}}$,	X^{ii},	X^{i},	X,
		6	8		
(0)	+	−	+	+	−
	0	1	2	2	3
(1)	+	+	+	+	+
		18	14		

and place above and below the indices 1 and 2, in the succession of indices 0, 1, 2, the values of $\mathrm{X}^{\mathrm{iii}}$ and X^{ii} respectively, without regard to sign, corresponding to $x = 0$ and $x = 1$; then we shall find $\dfrac{8}{6} > 1$ and, *à fortiori*, therefore $\dfrac{8}{6} + \dfrac{14}{18}$, also greater than 1, which is the interval between which the roots required are to be sought for: it consequently follows that two of the roots corresponding to this interval are imaginary, and there remains, therefore, only one real root between 0 and 1.

If we suppose

$$\mathrm{X} = x^5 + x^4 + x^3 - 2x^2 + 2x - 1 = 0,$$

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* The new values a' and b' of a and b may be made $a + \dfrac{f(a)}{f'(a)}$ and $b - \dfrac{f(b)}{f'(b)}$, which are $0\,n'$ and $0\,m'$ respectively: a second trial will generally succeed.

the corresponding scheme will be as follows:

	X^{v},	X^{iv},	X^{iii},	X^{ii},	X^{i},	X,
		96	42			
(-1)	+	−	+	−	+	−
		36	9			
$\left(-\frac{1}{2}\right)$	+	−	+	−	+	−
	0	1	2	2	2	2
(0)	+	+	+	−	+	−
		24	6	4	2	
	0	0	0	1	2	2
$\left(\frac{1}{2}\right)$	+	+	+	+	+	−
				36	10	
(1)	+	+	+	+	+	+

If we take the interval from (-1) to 0, we find two roots included within it; but since $\frac{42}{96} + \frac{6}{24}$ is less than the interval, no certain conclusion can be drawn with respect to the nature of the corresponding roots. If we now consider the interval from $-\frac{1}{2}$ to 0, which includes the same roots, we shall find $\frac{9}{36} + \frac{6}{24} = \frac{1}{2}$, a quantity equal to the whole interval, and we are consequently authorized in concluding that the corresponding roots are imaginary. In a similar manner, we find the indication of the existence of two roots between 0 and $\frac{1}{2}$; and in as much as $\frac{2}{4} = \frac{1}{2} =$ the whole interval, we at once conclude that the two roots in question are imaginary*.

It thus appears that we are enabled, by the processes just described, to separate all the real roots of an equation and to

* When we speak of the existence of imaginary roots between two limits, we do not mean that such limits comprehend the *moduli* of these roots, but merely that the real roots which would be found between those limits, if certain conditions were satisfied, are wanting, and that there are as many imaginary roots of the equation which may be said to correspond to them which are sufficient to complete the required number of changes of sign which are lost. The theory of Fourier as given in his work, determines nothing concerning the values or limits of the *moduli*, or of the peculiar nature of the signs of affection, of such imaginary roots.

assign their limits, and thus to prepare them for the certain application of methods of approximation. They constitute a most important element in the theory of numerical equations; and though they do not enable us to assign the limits of the *moduli* of the pairs of impossible roots nor to determine their signs of affection, yet they at once indicate both their existence and their number, and thus form the proper preparation, at least for the application of methods, whether tentative or not, for the determination of their values.

Lagrange, in the fifth chapter of his ***Résolution des Equations Numériques***, has shown in what manner the equation of the squares of the differences may be applied to the determination of these imaginary roots; and the methods which thence arise are equally complete, in a theoretical sense, with those which are made use of, by the aid of the same equation, for the determination of the limits of the real roots; and Legendre, also, has furnished tentative methods of approximating to their values. But all such methods are more or less nearly impracticable for equations of high orders; and the invention of a ready and certain method of separating the imaginary roots of equations, as the basis of processes for approximating to their values, must still be considered as a great *desideratum* in algebra.

The method of approximating to the roots of numerical equations, when their limits are assigned, which Lagrange has given, by means of continued fractions, is so well known that it is quite unnecessary to enter upon a detailed examination of its principles. If there is only one real root, included between two consecutive whole numbers, there will be only one positive root in the several transformed equations, which is greater than 1, and methods which are certain and sufficiently rapid may be applied to the determination of the several quotients which form the converging fractions. If, however, there are two or more roots included between two consecutive whole numbers, there will be two or more roots of the first transformed equation, and possibly, likewise, of the transformed equations which follow which are greater than 1, and which may be placed between two consecutive whole numbers. The separation of such roots may be effected by the methods of Fourier, which have been explained above; but when we have once arrived at a transformed equation which has two or more roots greater than 1, no two of which are included between two consecutive whole numbers, then we shall find the same number of sets of successive transformed equations, which will furnish the several sets of quotients to the continued fractions, which represent the roots of the primitive equation, which are included between two limits which are consecutive whole numbers. The formation, however, of these transformed equations, and the determination of the next inferior integral limit of their roots, even when no further separation of the roots is required, is excessively laborious, and Lagrange has pointed out methods by which the operations required for both these objects may be greatly simplified. Legendre also, in the 14th section of the first part of his Theory of Numbers, has given a considerable practical extension to these methods of Lagrange. If we combine their processes for finding the nearest inferior limit of the root with the theorems of Budan * for the formation of the transformed equations, we shall probably have arrived at the greatest simplification which the practical solution of numerical equations, by means of continued fractions, is capable of receiving.

Lagrange has pointed out the principal defects of the method of approximation to the roots of numerical equations which was given by Newton †. It is only under particular conditions that it is competent to attain the object proposed, and in no case does it immediately furnish a measure of the accuracy of the approximation. But notwithstanding these objections to this method, in the form under which it has been commonly applied, it is unquestionably that which most naturally arises out of the analytical conditions of the problem, and which is also capable of the most immediate and most simple application in almost every department of analysis. Lagrange had demonstrated that this method could only be applied with safety to find the greatest and least roots of an equation, and in those cases only in which the *moduli* of the imaginary roots, if any existed, were included in value between such roots. But Fourier has shown, by considering the superior and inferior limits of every real root, and by a proper examination of certain conditions which those limits may be made to satisfy, and by instituting the approximation simultaneously with respect to both those limits, that all sources of ambiguity may be removed and the accuracy of the approximation determined ‡. We shall now proceed to give a short notice of these researches.

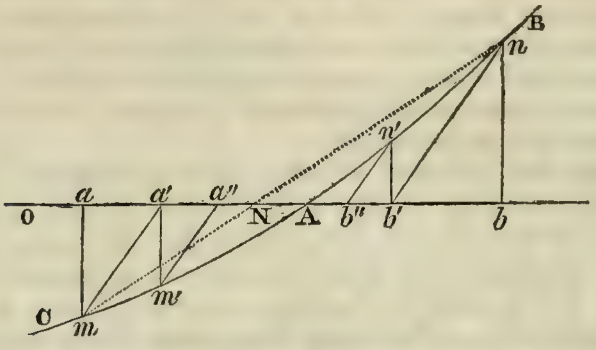
* *Nouvelle Méthode pour la Résolution des Equations Numériques.* It contains the exposition of exceedingly simple and rapid rules for the formation of the transformed equation whose unknown quantity is $x - e$, where e is any integral or decimal number. In other respects, however, this publication, though announced with great pomp and circumstance, is a very superficial production, and is only remarkable for having received the charitable notice and approbation of Lagrange.

† *Résolution des Equations Numériques,* Note v.

‡ *Analyse des Equations déterminées,* livr. ii., *Calcul des Racines.*

1. If $f(x) = 0$, or $X = 0$ be the equation, $f'(x)$, $f''(x)$, or X', X'' its first and second derivatives, then the limits a and b of one of the roots will be sufficiently near for the application of this method of approximation, if the three last *indices* (p. 332) be 0, 0, 1. If this be not the case, the interval between a and b must be further subdivided until this last condition is satisfied.

Under such circumstances there will be no root of the equations $f'(x) = 0$ and $f''(x) = 0$, included between a and b: and if we suppose $y = f(x)$ to be the equation of a parabolic curve C A B, where O $a = a$, O $b = b$, $a\,m = f(a)$, $b\,n = f(b)$, then



there will be no point of inflection between a and b, and no tangent parallel to the axis. The analytical conditions above mentioned would show that $f(a)$ and $f(b)$ must necessarily have different signs.

2. If we suppose b to represent the superior limit of the root (α), then the Newtonian approximation gives us the new superior limit $b' = b - \frac{f(b)}{f'(b)}$; a new inferior limit will be found to be $a' = a - \frac{f(a)}{f'(b)}$: these limits are still superior and inferior limits of the root α, and are both of them nearer to it than the primitive limits b and a.

If the same operation be repeated by replacing b and a in $f(b)$ and $f(a)$ by b' and a', nearer limits will be obtained, and it is obvious that the same process may be repeated as often as may be thought necessary. And in as much as we obtain both the inferior and superior limits corresponding to each operation, the difference between them will always be greater than the error of each approximation. If we refer to the above figure, and suppose $n\,b'$ to be a tangent to the curve at n, and $a'\,m$ to be drawn parallel to $n\,b'$, then $b\,b' = \frac{f(b)}{f'(b)}$, and $a\,a' = \frac{f(a)}{f'(b)}$, since $f'(b) = \tan n\,b'\,b = \tan m\,a'\,a$. It follows, therefore, that

O b' and O a' are the new limits b' and a': and if ordinates b' n' and a' m' be drawn to the curve, and n' b'' be drawn a tangent, and m' a'' parallel to n' b'', then O b'' and O a'' will be the new values b'' and a'' of b' and a'. The progress of the approximation, upon the continued repetition of this process, will now be sufficiently manifest.

3. If we consider the different arrangements of the signs of $f''(x)$, $f'(x)$, $f(x)$, in the transition from the inferior limit a to the superior limit b, they will be found to be the following, it being kept in mind that the sign of $f(x)$ *alone* changes from $+$ to $-$, or conversely.

		$f''(x)$	$f'(x)$	$f(x)$
(1)	a	$+$	$+$	$-$
	b	$+$	$+$	$+$
(2)	a	$-$	$-$	$+$
	b	$-$	$-$	$-$
(3)	a	$+$	$-$	$+$
	b	$+$	$-$	$-$
(4)	a	$-$	$+$	$-$
	b	$-$	$+$	$+$

In the first two cases, the formulæ of approximation are $b - \frac{f(b)}{f'(b)}$ and $a - \frac{f(a)}{f'(b)}$, and commence therefore with the superior limit. In the last two cases, the formulæ of approximation are $a - \frac{f(a)}{f'(a)}$ and $b - \frac{f(b)}{f'(a)}$, and commence therefore with the inferior limit. In other words, that limit must in all cases be selected which gives the same sign to $f''(x)$ and $f(x)$, whether $+$ or $-$. The construction of the portions of the corresponding parabolic curves included between a and b in these several cases, will at once make manifest the reason of the selection of the superior or inferior limit and likewise the progress of the approximation itself*.

* If, in the figure p. 338, we join the extremities m and n of the ordinates $a\,m$ and $b\,n$ by the chord m N n, which cuts the axis of x in the point N, we shall find $\text{O N} = a - \frac{f(a)\ (b-a)}{f(b) - f(a)} = b - \frac{f(b)\ (b-a)}{f(b) - f(a)}$, which gives a new approximate inferior limit in the first two cases considered in the text, and a new superior limit in the last two. Other constructions are noticed by Fourier, which give similar results.

In the *Mémoires de l'Académie Royale de Bruxelles* for 1826, there is a memoir on the resolution of numerical equations by Dandelin, in which the analytical conditions which must be satisfied by the limit, towards which the

4. In the application of these rules some precautions are occasionally necessary. Thus, if $f''(x)$ and $f(x)$ have a common measure $\varphi(x)$, and if a root (α) of $\varphi(x) = 0$ be included between a and b, then there is a point of inflection of the parabolic arc between a and b at the point of its intersection with the axis. Under such circumstances, the method of approximation must be applied to the equation $\varphi(x) = 0$, and not to the primitive equation $f(x) = 0$, for the purpose of determining the value of α. Again, if there exists a common measure of $f'(x)$ and $f(x)$, which becomes equal to zero, for a value of x between a and b, then there are two or more equal roots of $f(x) = 0$ in that interval, and the final succession of *indices* is no longer 0, 0, 1. Other precautions connected with the subdivision of the interval $b - a$ are sometimes required, which the limits of this Report will not allow me to notice in detail.

It remains to add a few remarks upon the rapidity of the approximation, and upon the means by which it may be ascertained. If we express the primary and secondary intervals $b - a$ and $b' - a'$ by i and i', it may be very easily proved that

$$i^1 = i^2 \cdot \frac{f''(a \ldots b)}{2f'(b)},$$

where $f''(a \ldots b)$ denotes some value which $f''(x)$ assumes when we substitute for x a quantity between a and b: and if we form the quotient (C) which arises from dividing the greatest value of $f''(a)$ and $f''(b)$* by the least value of $2f'(a)$ and $2f'(b)$, and suppose k the order of the greatest articulate or subarticu-

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approximation in Newton's method must be made, are established by a combination of analytical and geometrical considerations, and in which also the new limits b' and a' are respectively found by what he terms the *rule* of *tangents* in one case, and by the *rule* of *chords* in the other. The first is the subtraction of the subtangent $b\,b'$ or $\frac{f(b)}{f'(b)}$ from b, as involved in the ordinary Newtonian approximation when the proper limit is selected. The second is the determination of the value of O N, or $a - \frac{f(a)\ (b-a)}{f(b) - f(a)}$, or $\frac{a\,f(b) - b\,f(a)}{f(b) - f(a)}$, by the method taught at the beginning of this Note. It is evident that these conclusions involve all that is important in Fourier's researches upon this part of the subject.

This memoir of M. Dandelin, which contains a very full and a very clear exposition of the whole theory of the Newtonian method of approximation, preceded by five years the publication of M. Fourier's work.

* Since no root of $f'''(x) = 0$ is included between a and b, it follows that either $f''(a)$ or $f''(b)$ will be the greatest value of $f''(a \ldots b)$: the same remark applies likewise to $f'(a)$ and $f'(b)$.

late number * immediately greater than this quotient, and n the order of the articulate or subarticulate number which is not less than the difference of the limits $b - a$, then if we divide $f(b)$ by $f'(b)$, and continue the operation as far as the $(2n + k)^{\text{th}}$ decimal, and increase the last digit by 1, the quotient which arises being subtracted from or added to, b, according as $f(b)$ and $f'(b)$ have the same or different signs, will give a result which will differ from the true value of the root by a quantity less than $\left(\frac{1}{10}\right)^{2n+k}$. And if the same operations be repeated, forming successively new limits by means of the results thus obtained, we shall obtain a series of limits which are correct as far as the $(4n+3k)^{\text{th}}$, the $(8n+7k)^{\text{th}}$, &c., decimal place †.

The processes of approximation which have been described above, as well as those which belong to all other methods, require divisions and other operations with numbers which are sometimes beyond the reach of logarithmic tables, and which it is extremely important to abbreviate as much as possible, consistently with the determination of the accurate digits of the results which are required to be found. Such processes were taught by Oughtred and other algebraists of the seventeenth century, but both their theory and applications have been greatly and, perhaps, undeservedly, neglected in later times. The consideration, however, of such methods has been partially revived by Fourier and some other writers, the first of whom has given examples of what he terms *ordinate division* (*division ordonnée*,) the principle of which is to conduct the division by the employment of a small number of the first digits of the divisor only, and to correct the successive remainders, augmented by the successive digits of the original dividend, in such a manner as to bring into operation the successive digits of the divisor when they are required for the determination of the correct digit of the quotient, and not before. Such processes, however, are incapable of being briefly described, and we can only refer to the original work ‡ for the developement of the rule and for examples of its application.

* An articulate number is one of the series 1, 10, 200, 7000, &c., where the first digit is followed by zeros only. A subarticulate number is one of the series ·1, ·02, ·003, &c., and the number which designates the place of the first significant digit is supposed to be negative.

† The course of the approximation, in order to be perfectly regular and rapid, would require that $2n + k$ should be greater than n, or that n should be greater than $-k$, a circumstance which might occur if k or n was negative. In such a case it will be necessary, or rather expedient, to subdivide the interval $b - a$, until the difference of the two limits does not exceed $\left(\frac{1}{10}\right)^n$, where n is equal to, or greater than, $1 - k$.

‡ *Analyse des Equations determinées*, livr. ii. p. 188.

Similar processes, also, have been investigated and applied with remarkable ingenuity and success by Mr. Holdred *, Mr. Horner †, and Mr. Nicholson ‡. The first of these writers, a mathematician in humble life, who had formed his taste upon the study of the older algebraical writers of this country, gave very ingenious rules for finding the roots of numerical equations. The method proposed by Mr. Horner was founded upon much more profound views of analysis and of the relation which exists between the processes of algebra and arithmetic, and he has not only succeeded in making a very near approximation to the true principles upon which the limits of the roots of numerical equations are assigned, but by considering the rules for extracting the roots of numbers and of affected numerical equations as founded upon common principles, he has reduced the rules for these purposes to a form which admits of very rapid and effective, though not perhaps of very easy, application. Mr. Nicholson, by a combination of the methods of Mr. Holdred and Mr. Horner, has greatly simplified them both, and reduced them to the form of practical rules, which are not much more complicated than those which are commonly given for the extraction of the cube and higher roots of numbers.

The Newtonian method of approximation, which we have hitherto considered, may be termed *linear*, in as much as the equations of a straight line combined with the general equation of the parabolic curve are competent to express all the circumstances which characterize it. But methods of approximation of higher orders than the first, involving the second or higher powers of the unknown quantity to be determined, have likewise been considered by Fourier and other writers. That of the second order, viewed with reference to the properties of curve lines, may be said to result from the contact of arcs of a conical parabola. The superior and inferior limits, thus determined, converge with great rapidity, the error corresponding to each operation being the product of a constant factor with the cube of the preceding error. Such methods, however, if viewed with reference to the facility of their practical applications, are incomparably less useful than those which are founded upon linear approximations; but there is much which is instructive in their theory, and particularly as furnishing the means of determining immediately the nature of two roots of an equation included in a given interval, which the application of the methods for the

* This method is particularly noticed in Mr. Nicholson's *Essay on Involution and Evolution*. I have never seen the original tract published by Mr. Holdred.

† *Philosophical Transactions* for 1819.

‡ *Essay on Involution and Evolution*. 1820.

separation of the roots which we have previously described may have left in the first instance uncertain. We refer to the end of the second book of Fourier's *Analyse des Equations determinées*, for a very complete examination of the theory of such approximations *.

It has been a question agitated on more than one occasion, whether the tests of the reality of the roots of equations of finite dimensions which De Gua established, or rather the principles of the much more general theorem of Fourier, were applicable likewise to transcendental equations. In a discussion of the transcendental equation

$$y = 1 - \theta + \frac{\theta^2}{2^2} - \frac{\theta^3}{2^2 \cdot 3^2} + \frac{\theta^4}{2^2 \cdot 3^2 \cdot 4^2} - \&c.,$$

which presents itself in the expression of the law of propagation of heat in a solid cylinder † of infinite length, Fourier ventured to apply the principles in question to show that all its roots were real; but M. Poisson ‡ has disputed the propriety of such an application, both in this case and in others: thus, if we suppose

$$X = e^x - b\, e^{a\,x},$$

we shall find

* The rule for the determination of the nature of two roots included in a given interval, which is given in page 333, is merely the expression of a consequence of the application of the method of linear approximation to the distinction of those roots; and whatever difficulties in certain extreme cases may attend the successful application of that rule, will necessarily present themselves likewise in the application of the linear approximation under the same circumstances. This character, however, is not confined to the Newtonian or linear method of approximation. If the interval of the roots be determined, by the application of Fourier's theorem of the succession of signs of the original function X and its derivatives, so that no more than two roots may be said to exist in that interval, whose nature is unknown, whether real or imaginary, then the application of the method of continued fractions, as well as of other equivalent modes of approximation, will be competent to determine the values of those roots when real, and their nature, when imaginary. Such, at least, is the assertion of Fourier, who refers to the third book of his work on equations for its demonstration. It is unfortunate, however, that only two books of this work, which is full of such remarkable researches upon the theory of equations, were fully prepared for publication at the time of his death. Our knowledge of the contents of the other five books, which were left unfinished, is derived from an *Exposé Synoptique* prefixed to those which are published, and which contains a general review and analysis of their principal contents. It is to be hoped, however, that the materials which he has left behind him will be found to be sufficient at least for their partial, if not for their complete restoration.

† *Théorie de la Chaleur*, p. 372.

‡ *Journal de l'Ecole Polytechnique*, cahier xix. p. 381; *Mémoires de l'Institut*, tom. ix. p. 92.

$$\frac{d^n X}{d x^n} = e^x - b a^n e^{ax},$$

$$\frac{d^{n+1} X}{d x^{n+1}} = e^x - b a^{n+1} e^{ax},$$

$$\frac{d^{n+2} X}{d x^{n+2}} = e^x - b a^{n+2} e^{ax},$$

where n is any whole number, or zero. If we now suppose

$$\frac{d^{n+1} X}{d x^{n+1}} = 0,$$

and eliminate, by means of this equation, e^x, we shall get

$$\frac{d^n X}{d x^n} = - b (1 - a) a^n e^{ax},$$

$$\frac{d^{n+2} X}{d x^{n+2}} = b (1 - a) a^{n+1} e^{ax},$$

and therefore

$$\frac{d^n X}{d x^n} \cdot \frac{d^{n+2} X}{d x^{n+2}} = - b^2 (1 - a)^2 a^{2n+1} e^{2ax},$$

a quantity which is negative for every real value of x. The conclusion which should be drawn, in conformity with Fourier's principles, is, that all the roots of the equation $e^x - b e^{ax} = 0$ are real, as well as those of its successive derivatives; whilst the fact is, that each of those equations has one real root, and an infinite number of imaginary roots, which are included under the formula

$$x = \frac{\log b a^n + 2 i \pi \sqrt{-1}}{1 - a}.$$

In reply to this objection, it has been urged by Fourier that Poisson has not very accurately stated the terms of the proposition in question as applicable to such a case*, and also that he has neglected to take into consideration all the roots of the equation. For if we suppose that the substitution of two limits a and b, in a function $f(x)$ and its derivatives, gives results which present the same succession of signs between $f^{(n+r)}(x)$ and $f^{(n)}(x)$, then those extreme derivative functions, and those

* This inaccuracy of statement is rather chargeable upon Fourier himself than upon Poisson, who has certainly failed to notice the necessary limitation of this proposition upon the occasion which gave rise to its application in page 373 of the *Théorie de la Chaleur*.

also which are included between them, when considered as equations, will contain the *same number of roots, or none*, between those limits. This proposition is true, whether the number of derivative functions be finite, as in the case of algebraical equations, or infinite, as in the case of transcendental equations. In the first case, however, it admits of absolute application, in consequence of our arriving at a final derivative, from which the comparison of the signs of the two series of results commences. In the second case we can draw no conclusion, in the absence of any difference in the signs of the series of results, in the transition from one derivative function to another, with respect to the number of roots of any of those functions which are included between the given limits: thus, if $f(x) = \sin x$, we shall have the same series of signs of $\sin x$ and of its derivatives, however far continued, upon the substitution of the limits a and $a + 2\pi$, although it is manifest that there are two real roots of $\sin x = 0$ between those limits. The general proposition, therefore, will, in such a case, authorize us in concluding *merely* that whatever number of roots the equation $\sin x = 0$ includes between the limits a and $a + 2\pi$, will be possessed likewise by all its derivative equations between the same limits*.

There is another point of view, likewise, in which the objection advanced by Poisson may be considered as not altogether applicable to the example which he puts forward. In considering the roots of the derivative functions $\frac{d^n X}{d x^n}, \frac{d^{n+1} X}{d x^{n+1}}, \frac{d^{n+2} X}{d x^{n+2}}$, he has not included those of the factor e^x, which those functions severally involve. Since $e^x = \left(1 + \frac{x}{\infty}\right)^{\infty} = 0$, it follows that there are an infinite number of equal roots (where $x = -\infty$) of $e^x = 0$, which equally reduce three or any number of consecutive derivative functions to zero, and to which, therefore, the test of De Gua is no longer applicable. It would follow, therefore, that the existence of imaginary roots in the equation $X = 0$ is no longer contradictory to Fourier's proposition, even

* If the transcendental function denoted by $f(x)$ be a determinate function, it will always be possible to assign an interval δ, such that the derivative function $f^n(x) = 0$ contains no root, or a determinate number of roots, between a and $a + \delta$. If such an interval or succession of intervals can be determined for any one derivative function, such as $f^{(n)}(x)$, it will become a point of departure for the determination of the number and nature of the roots corresponding to the same interval or intervals for all the other derivative functions which form the superior or inferior terms of the series. In the case of algebraical functions, the point of departure is that derivative function which is a constant quantity.

admitting the correctness of that form of it which Poisson ha. assigned*.

* If we transform e^x by replacing x by $-\frac{1}{x'^2}$, we shall get the expression $e^{-\frac{1}{x'^2}}$, which may be easily shown as above, and also by other means, to be equal to *zero* when x' is equal to *zero*, and equal to 1 when x' is equal to infinity.

Professor Hamilton of Dublin, in a paper in the *Irish Transactions* for 1830, has quoted the expression $e^{-\frac{1}{x'^2}}$ as possessing some very peculiar properties, which are inconsistent with the universality of a very commonly received principle of analysis. It is commonly assumed that "if a real function of a positive variable x approaches to *zero* with the variable, and vanishes along with it, then that function can be developed in a real series of the form

$$A\,x^{\alpha} + B\,x^{\beta} + C\,x^{\gamma} + \&c. \qquad (1.)$$

where α, β, γ, &c., are constant and positive, A, B, C, &c., constant, and all those coefficients different from zero: but if we put the equation under the form

$$x^{-\alpha}\,e^{-\frac{1}{x^2}} = A + B\,x^{\beta-\alpha} + C\,x^{\gamma-\alpha} + \&c.,$$

supposing α the least of the several indices α, β, γ, &c., then if $x = 0$, we shall find $x^{-\alpha}\,e^{-\frac{1}{x^2}} = 0$ or A equal to *zero*; for if we replace $\frac{1}{x}$ by y, we shall get

$$\frac{1}{x^{-\alpha}\,e^{-\frac{1}{x^2}}} = x^{\alpha}\,e^{\frac{1}{x^2}} = y^{-\alpha}\,e^{y^2}$$

$$= y^{-\alpha} + y^{2-\alpha} + \frac{y^{4-\alpha}}{1\,.\,2} + \frac{y^{6-\alpha}}{1\,.\,2\,.\,3} + \&c.,$$

all whose terms are positive, and which, when $x = 0$ or $y = \infty$, will necessarily become equal to *infinity*: it follows, therefore, that the function $e^{-\frac{1}{x^2}}$ is not capable of developement in a series of the assumed form (1.). The same expression, as has been remarked by Professor Hamilton, has been noticed by Cauchy as an example of the vanishing of a function and of all its differential coefficients, for a particular value of the variable, without the function vanishing for other values of the variable, thus forming an exception to another principle generally received in analysis. In his *Leçons sur le Calcul Infinitesimal,* Cauchy has produced this last anomaly as a sufficient reason for not founding the principles of the differential calculus upon the developement of functions, as effected by or exhibited in, the series of Taylor.

It is possible that more enlarged views of the analytical relations of *zero* and *infinity,* and of the interpretation of the circumstances of their occurrence, as well as of the principles and applications of Taylor's series, may enable us to explain these and other anomalies, and to show that they arise naturally and necessarily out of the very framework of analysis; but it must be confessed that there are many other difficulties, which are yet unexplained, which are connected with the developement of e^x when x is negative or ima-

Another method of approximation to the roots of equations by means of recurring series was proposed by Daniel Bernoulli *, and very extensively illustrated and applied by Euler †. If we write down m arbitrary numbers to form the first m terms of the series, and if we assume, for the scale of relation, the coefficients of an equation of m dimensions, and form by means of it and the assumed terms the other terms of the series which may be indefinitely continued, and if we also form a series of quotients by dividing each succeeding term (after the arbitrary terms) by that which precedes it, then the terms of the series of quotients which thence arise, will converge continually towards the value of the greatest root of the equation; and if we form the equation whose roots are the reciprocals of those of the original equation, and proceed in a similar manner, we shall obtain a series of quotients which will converge to the greatest root of this equation, whose reciprocal will be the least root of the original equation, considered without reference to its algebraical sign.

Lagrange, in the 6th Note to his *Résolution des Equations Numériques*, has analysed the principles of this method, and has shown that its success will depend upon the greatest real root, without reference to algebraical sign, being greater than the *modulus* of any of the imaginary roots. If this condition be not satisfied, the quotients will not approximate to the value of any root of the equation, a consequence which Euler had also pointed out.

The recurring series which is formed by dividing the first derivative function $f'(x)$ by $f(x)$, which is equal to

ginary. Some of these have been noticed in the note to p. 267, in connexion with our observations upon Mr. Graves's researches upon the theory of logarithms; another is noticed by M. Clausen of Altona, in the second volume of *Crelle's Journal*, p. 287; it is stated as follows:—Since $e^{2n\pi\sqrt{-1}} = 1$, when n is a whole number, we get $e^{1+2n\pi\sqrt{-1}} = e$ and therefore $e^{(1+2n\pi\sqrt{-1})^2} = e^{1+2n\pi\sqrt{-1}} = e = e^{1+4n\pi\sqrt{-1}-4n^2\pi^2}$, and consequently $e^{-4n^2\pi^2} = 1$, whenever n is a whole number,—a conclusion which M. Clausen characterizes as *absurd*. Its explanation involves no other difficulty than that which is included in the equation $e^{2n\pi\sqrt{-1}} = 1$, and must be sought for in the circumstances which accompany the transition from a function to its equivalent series, when a strict arithmetical equality does not exist between them. It must be confessed, however, that these difficulties are of a very serious nature, and are in every way deserving of a more careful examination and analysis than they have hitherto received.

* *Comment. Acad. Petrop.*, vol. iii.

† *Introductio in Analysim Infinitorum*, vol. i. cap. xvii.

$$\frac{1}{x-\alpha} + \frac{1}{x-\beta} + \frac{1}{x-\gamma} + \&c.,$$

when α, β, γ, &c., are the roots of the equation, whether real or imaginary, has been shown by Lagrange to be the series furnished by this method which is most easily formed, and to be likewise that which converges most rapidly and certainly to a geometrical series in the case of equal roots. In every case the terms of the series of quotients are alternately greater and less than the root to be determined, and consequently furnish a measure of the accuracy of the approximation.

This method of approximation is generally less rapid and certain than those which have already been considered, and, as commonly stated, is extremely limited in its application. It is true, as has been shown by Lagrange, that a knowledge of the limits of the roots would enable us to apply it to the determination of all the real roots by means of a series of transformed equations equal to their number, such as is required in the Newtonian method of approximation, and also in that of Lagrange; but under such circumstances, and with such data, it is more convenient and more expeditious to employ those methods in preference to the one which we are now considering.

Fourier has shown in what manner this method may be applied to determine all the roots of an equation, whether imaginary or real. Let us suppose a, b, c, d, e, &c., to represent the roots of the equation arranged in the order of magnitude, the magnitudes of imaginary roots being estimated by the magnitudes of their *moduli*; and let A, B, C, D, E, &c., be the terms of the recurring series, whose quotients furnish the value of the greatest root, when that root is real. Form, in the second place, a series whose terms are $AD - BC$, $BE - CD$, $CF - DE$, &c., which is also a recurring series, whose quotients may be easily shown to approximate to the sum of the two first roots $a + b$. Again, form a series whose terms are $AC - B^2$, $BD - C^2$, $CE - D^2$, &c., which is also a recurring series, whose successive quotients will approximate to the value of the greatest product $a\,b$. In a similar manner, we may deduce from the primitive recurring series three other recurring series, the terms of the convergent series formed by whose quotients will form, in the first series, the sum $a + b + c$ of the three first roots; in the second, the sum of their products two or two, or $a\,b + a\,c + b\,c$; and in the third their continued product $a\,b\,c$: and similarly for four or a greater number of roots. If, therefore, we suppose the first root a to be imaginary, the first series will give no result; but the values of $a + b$ and of $a\,b$, which are given by the two first recurring series derived from the primitive recurring series, will enable us to determine their separate values: in both cases the series of quotients is convergent.

If the third root be real, the third series of derived quotients is convergent; if not, the fourth series will be so, and so on as far as we wish to proceed.

These propositions have been merely announced by Fourier in his Introduction. The chapter of his work, which contains the demonstrations, has not yet been published.

If the root of an equation be determined approximately, the equation may be depressed, and the general processes of solution or of approximation may be applied to find the roots of the quotient of the division. Thus, in the equation

$$x^3 - 3\,x + 2{\cdot}0000001 = 0,$$

one of the roots is very nearly equal to 1, if we divide the equation by $x - 1$, and neglect the small remainder which results from the division, we shall get the quotient

$$x^2 - x - 2 = (x - 1)\,(x + 2) = 0,$$

whose roots are 1 and -2; or we may suppose one of the roots to be $1{\cdot}0001$, the second $\cdot 9999$, and the third -2; or we may suppose two of the roots to be imaginary, namely, $1 \pm \cdot 0001\,\sqrt{-1}$. All these roots are approximate values of the roots of the equation, which different processes, whether tentative or direct, may determine: and it is obvious that when two roots are equal, or nearly so, an inaccuracy of the approximation to those roots which are employed in the depression of the primitive equation may convert real roots into imaginary, or conversely. Such consequences will never follow when the limits and nature of the roots are previously ascertained, and every root is determined independently of the rest; but it is not very easy to prevent their occurrence when methods of approximation are applied without any previous inquiries into the nature and limits of the roots, though the resulting conversion of imaginary roots into real, and of real roots into imaginary, may not deprive them of the character of true approximations to the values of the roots which are required to be determined.

If the limits of the roots of an equation $\mathrm{F}\,x = 0$ be assigned, and if the Newtonian method of approximation be applied continually to one of these limits a, we should obtain, for the value of the root, the series*

$$a - a'\,\mathrm{F}\,a + \frac{a''}{1\,.\,2}(\mathrm{F}\,a)^2 - \frac{a'''}{1\,.\,2\,.\,3}(\mathrm{F}\,a)^3 + \&c.,$$

* Lagrange, *Résolution des Equations Numériques*, Note xi.

where

$$a' = \frac{1}{F' a}$$

$$a'' = -\frac{a' F'' a}{(F' a)^2} = -\frac{F'' a}{(F' a)^3}$$

$$a''' = -\frac{a' F''' a}{(F' a)^3} + \frac{3\, a' (F'' a)^2}{(F a)^4}$$

$$= -\frac{F''' a}{(F' a)^4} + \frac{3\, (F'' a)^2}{(F' a)^5}.$$

This series was first assigned by Euler, and the observations which we have had occasion to make in the preceding pages upon linear approximations will at once explain the circumstances under which it may be safely applied: it cannot be viewed, however, in any other light than as the analytical expression for the result of the application of such linear approximations, repeated as many times as there are terms of the series succeeding the first.

The celebrated theorem of Lagrange, which is so extensively used in the solution of the transcendental equations which present themselves in physical and plane astronomy, will enable us to assign, likewise, a series for the *least* root, or for any function of the least root of an equation in terms of its coefficients. Mr. Murphy, in a very able memoir in the *Transactions of the Philosophical Society of Cambridge* for 1831, has shown the mode in which such series may be determined, by means of a very simple rule, which admits of very rapid and very extensive application. The rule is as follows:

"To find the series for the least root of the equation $\varphi(x) = 0$, divide the equation by x, and take the Napierian logarithm of the quotient which arises; then the coefficient of $\frac{1}{x}$ with its sign changed is the series which expresses the least root required."

Thus, to find the series for the least root of the quadratic equation

$$x^2 + a\,x + b = 0,$$

find the coefficient, with its sign changed, of $\frac{1}{x}$ in $\log \frac{(x^2 + a\,x + b)}{x}$ or $\log \left(1 + \frac{x + \frac{b}{x}}{a}\right)$, which is

$$\left\{\frac{b}{a} + \frac{b^2}{a^3} + \frac{4}{2} \cdot \frac{b^3}{a^5} + \frac{6\,.\,5}{2\,.\,3} \cdot \frac{b^4}{a^7} + \frac{8\,.\,7\,.\,6}{2\,.\,3\,.\,4} \cdot \frac{b^5}{a^9} + \&c.\right\},$$

and therefore identical with that which arises from the developement of $-\frac{a}{2}+\sqrt{\left(\frac{a^2}{4}-b\right)}$. If b be greater than $\frac{a^2}{4}$, the roots of the equation are impossible, and the series becomes divergent, and gives no result.

Any function $f(x)$ of the least root of an equation $\phi(x)=0$ may be found "by subtracting from $f(0)$ the coefficient of $\frac{1}{x}$ in $f'(x)\log\frac{\phi(x)}{x}$." This more general theorem evidently includes the former.

"The sum of any assigned number (m) of roots of the equation $\phi(x)=0$ is equal to the coefficient, with its sign changed, of $\frac{1}{x}$ in $\log\frac{\phi(x)}{x^m}$."

The expression for the sum of m roots of an equation which is thus obtained gives the arithmetical value of the sum of the *m least* roots. In estimating the order of magnitude of such roots no regard is paid to their signs of affection.

Mr. Murphy has shown in what manner the same general proposition which is employed in the deduction of the results just given may be applied to the investigation of some of the most general theorems which have been employed in analysis for the developement and transformation of functions. Amongst many others the following very remarkable theorem seems to merit particular notice.

If $x_1, x_2, x_3, \ldots x_m$ be the *m least* roots of the equation

$$(x-a)^m - h\,F(x) = 0,$$

then,

$$f(x_1)+f(x_2)+f(x_3)+\ldots f(x_m)$$
$$= m f(a) + h\,\frac{d^{m-1}\{f'(a)\,F(a)\}}{1.2\ldots(m-1)\,d a^{m-1}}$$
$$+\frac{h^2}{1.2}\cdot\frac{d^{2m-1}\{f'(a)\,(F(a))^2\}}{1.2\ldots.(2m-1)\,d a^{2m-1}} + \&c.$$

If in this very general theorem we make $m=1$, it becomes the theorem of Lagrange; and if we make m equal to the dimensions of the equation, or greater than any power of x involved in $F(x)$, then it becomes the theorem which Cauchy has given, without demonstration, in the ninth volume of the new series of the *Memoirs of the Institute*, for the expression of the sum of the different values of $f(x)$, when x is successively replaced by every root of the equation.

The preceding conclusions, so very remarkable for their great generality, and for the very simple means employed in their derivation, will be sufficient to direct the attention of the reader to the other contents of this very original and valuable memoir.

There are some other most important departments of the general theory of equations which it was my intention to have noticed, and without which no report upon the present state and recent progress of algebra can be said to be complete. Amongst these may be particularly mentioned the theory of elimination and the solution of simultaneous equations, and also the theory of the solution of literal and of implicit equations. The very undue length, however, to which this Report has already extended, and the arrangements which have been made connected with the publication of this volume, compel me, though most reluctantly, to omit them. I venture to indulge a hope, however, that I may be allowed upon some future occasion to add a short supplemental Report upon this extensive department of analysis, in which I may be enabled to supply some of the numerous deficiencies of the preceding sketch.

ERRATA IN THE FOREGOING REPORT.

Page 197, line 21, *dele* not
— 215, — 3, *for* $\Gamma(r)\ \Gamma(1\ r)$ *read* $\Gamma(r)\ \Gamma(1-r)$
— 215, — 11 from the bottom, *for* $x^2 + 2 + x\ 1$ *read* $x^2 + 2x + 1$
— 221, — 15, *for* $\cos\ {}^1 \frac{n}{\varrho}$ *read* $\cos^{-1}\frac{n}{\varrho}$
— 226, — 2 from the bottom, *for* $\frac{\cos^{-1} a}{\sqrt{a^2+b^2}}$ *read* $\cos^{-1}\frac{a}{\sqrt{a^2+b^2}}$
— 234, — 7, *for* (0) *read* $(0)^n$
— 240, — 18, *for* In this last case $(a-b)^n$ *read* If we suppose n to be an even whole number or a fraction in its lowest terms with an even numerator, then $(a-b)^n$
— 240, — 10 from the bottom, *for* fraction *read* function
— 248, — 6, for *diventical* read *diacritical*
— 251, — 11, *for* $\cos\left\{x - \frac{(a+b)}{2} r\right\}$ *read* $\cos\left\{x - \frac{(a+b)}{2}\right\} r$
— 259, — 23, *dele* or the *greatest* of the two quantities α and β
— 261, — 14, *for* $\frac{4}{-}$ *read* $\frac{4}{\pi}$
— 263, — 15, *for* X^1 *read* X
— 316, — 29, *for* $x'' - 1 = 0$ *read* $x^{11} - 1 = 0$.

Professor Oersted's Apparatus for the Compression of WATER.

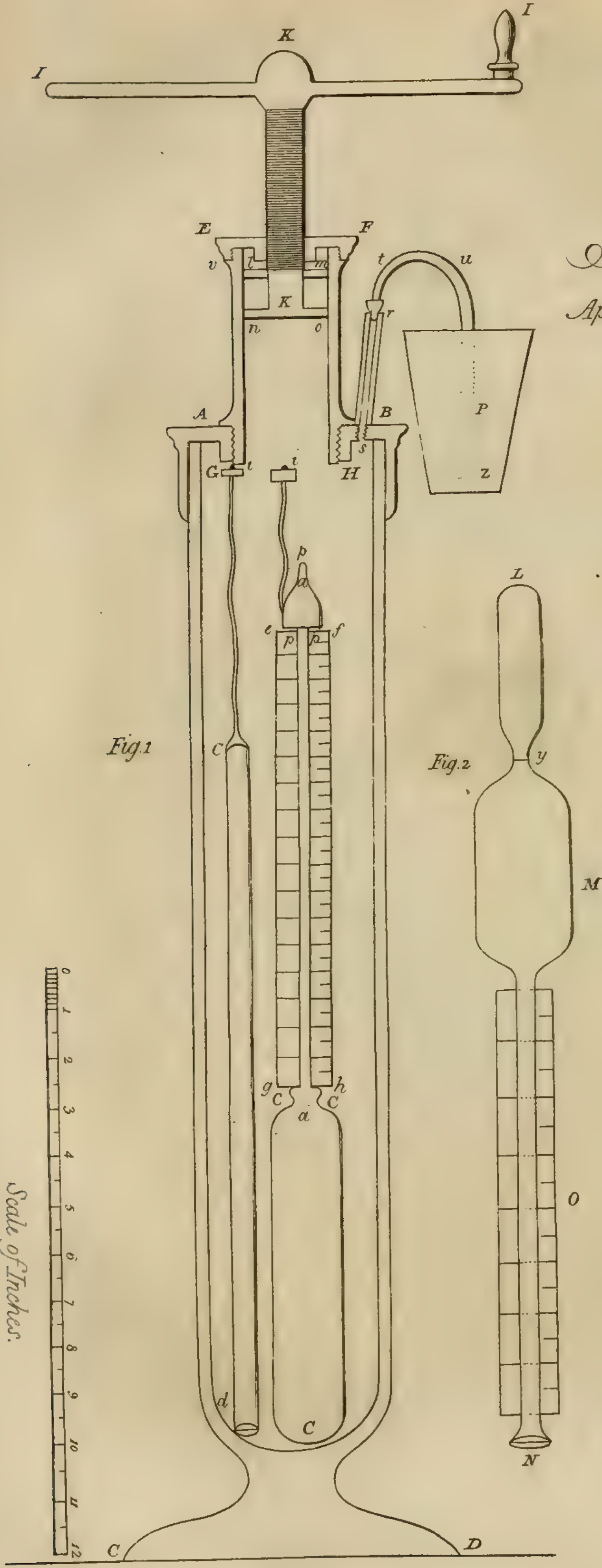

Fig.3

W.Brown sculp.

TRANSACTIONS OF THE SECTIONS.

1. MATHEMATICS AND PHYSICS.

On the Compressibility of Water. By Professor Œrsted. (*From a Letter to the Rev.* William Whewell, *dated Copenhagen, June* 18, 1833.)

[With an Engraving.]

"Were I not withheld by official duties, I should certainly not omit so excellent an opportunity of renewing the very interesting and useful acquaintance I made during my last visit to England and Scotland, and of forming new ones with those distinguished scientific characters that I was not fortunate enough to meet with at that time, or such as have risen to eminence of late years. But though I must now forgo this advantage, I will not let this opportunity pass without giving the illustrious assembly some mark of my high esteem, and of my desire to keep up the friendly intercourse which I have maintained with the British philosophers since my acquaintance with your happy country.

"You are, perhaps, aware that I have published several notices upon the compressibility of water, the first as early as 1818, and the first description of the improved method in 1822. Since that time I have still gone on improving my methods, and am now preparing a paper on the subject for the *Transactions of the Royal Society of Sciences at Copenhagen.* I will endeavour to give you a succinct account of my method and its results. It has been found that the apparatus for compressing water, a description of which I published in 1822, can give very accurate results; so that the results it has given in the hands of philosophers in different countries, have agreed more than might have been expected. Next to the accuracy of the measurements, however, one of the most important requisites of such an apparatus is, that the experiments be performed with the greatest celerity possible. When the experiment is protracted, the change of temperature produces great variations in the volume of the water, $\frac{1}{100}$ of the thermical measure (1° centigrade *) causing at high temperatures the volume of the water to vary more than the pressure of 3, 4, or even 5 atmospheres.

"The improved apparatus is represented in the diagram fig. 1. Its principal parts are the same as in the earlier; in each of them, however, some change is introduced. A B C D is a strong glass cylinder, having at the top a cylinder E F G H, containing a piston *l m n o*, moved by a screw K K, as in the first apparatus; but the handle I I is now arranged in such a manner that the screw can be turned without interruption; by this means the effect is accelerated, and subitaneous strokes avoided. The bottle *c c c*, with its capillary tube *a a*, is different from the earlier only so far, that the tube is not soldered to the bottle, but merely adjusted by grinding. This alteration is not necessary except when solid bodies are to be compressed. The scale *e f g h* is divided into parts of $\frac{1}{40}$ inch. In order to exclude the water with which the large cylinder is filled, from communication with that of the bottle, the top of the tube *a a* is covered with a small diving-bell, or rather diving-cap, *p p p*, whose conical shape has the advantage of preventing the water from reaching the top of the tube *a a*, even when the air is compressed to a tenth or twelfth part of its first volume. Its margin is loaded with a ring of lead or brass. *c d* is a glass tube with proper divisions, containing air, whose compression measures the pressure; its inferior part is loaded with some lead or a ring of brass. *t u z* is a siphon; P, a vessel containing water; *i i* are two buoys of cork for lifting up the bottle and the glass tube *c d*; *s r* is a tube of brass, which can be stopped by a screw. In the beginning and at the conclusion of the experiment it serves to introduce water into the space E F G H, or to get it out again. Before the experiments the calibre of the two tubes must be exactly ascertained, and the relative capacities of the bottle and its capillary tube determined by the quantities of mercury they can admit. I have had some tubes in which $\frac{1}{40}$ of an inch (making one division) held only 2 millionths of the capacity of the bottle, in others they have held more, in some even as much as 7 millionths. The capacity of the bottle was not less than $1\frac{1}{2}$ pound, often 2 pounds of mercury. It is next filled with water, which must be boiled in the bottle in order to expel the air, which might be suspected of having a great influence in these experiments, though Canton

* The unit of thermical measurement is the distance between the freezing and the boiling point. I think that the most natural expression for the temperatures would be this unit and its fractions. Thus, the temperature 0·50 would be the same as 50° centigrade, 19·30 the same as 1930° centigrade. I will mark this metrical measure by Th. If this innovation should not please, I wish that it might be suppressed, and centigrade degrees put in the place, which is an easy change.

has already observed that this is not the case. When the large cylinder is filled with water, the bottle is to be immersed in it. If the tube *a a* is full of water, a little of it must be expelled, which can be done by heating it gently with the hand, or better, by introducing a wire into it. As the bottle may be considered as a water thermometer, it is easy to ascertain whether it is in thermical equilibrium with the water in the cylinder. The air in the tube *c d* must likewise be brought to the same temperature with the water, before it is ultimately immersed. When the pumping cylinder shall be placed in its box, the piston must be at G H. If the large cylinder is full of water, part of it will be expelled through the siphon *t u z*. Now the piston is to be lifted up by means of the screw, whereby the pumping cylinder is filled with water. When this is done, the siphon is taken away, and the tube *r s* is stopped by a screw appertaining to it. The experiment is most conveniently performed by three persons; one turning the screw, the second observing the height of the water in the tube *a a*, and the third observing the volume of the air in the tube *c d*: the last writes down the numbers observed. Now, the point where the water stands in the tube of the bottle is to be noted. The descending piston having reduced the volume of the air in the tube *c d* to the point desired, the observer of it takes hold of the handle of the screw, and keeps the volume unchanged until the other observer has settled the point to which the water is brought down, and writes down the observation. When the piston is lifted up to its first place, the screw at *r* is to be opened, and the state of the water in the capillary tube again noted down. I commonly make ten or more such observations, one after another, which is performed in less than ten minutes when the operators are accustomed to work together. An example will illustrate the use of the observations.

Height of the Water in the Capillary Tube.

Before the pressure.	Mean.	When the pressure has ceased.	Length of descent in the capillary tube.	Point to which the pressure has driven the water down.
248·9	248·95	249·0	50·35	198·6
249·0	249·2	249·4	50·20	199·0
249·4	249·7	250·0	49·90	199·8
250·0	250·5	251·0	50·10	200·4
251·0	251·5	252·0	49·90	201·6
252·0	252·65	253·3	49·85	202·8
253·3	254·1	254·9	49·90	204·2
254·9	255·5	256·1	49·70	205·8
256·1	256·95	257·8	49·95	207·0
257·8	258·4	259·0	49·80	208·6
259.0	259·5	260·0	49·70	209·8
Mean of descents = 49·96.				

"The height of the mercury in the barometer, reduced to the freezing point, was at the same time 332·36 French lines. The volume of the air was in each experiment reduced to 5·264, or the pressure added to that of the atmosphere was 4·264 atmospheres. The pressure reduced to lines of mercury is thus, $332·36 \times 4·264 = 1417·18$; yet this reduction was not produced by the united pressure of the atmosphere and the piston alone, but was aided by a pressure of 40 lines of water, whose effect is equal to that of 2·94 lines of mercury, which is to be deducted, leaving then a pressure of 1414·24. Now, when a pressure of 1414·24 produces a descent of 49·96 parts, a pressure of 336 must produce a descent of nearly 11·87 parts. Each part makes in the instrument here employed 3·497 millionths of the whole capacity. $11·87 \times 3·497$ gives ultimately 41·51. The temperature of the water was at the beginning 0·20 Th., at the end 0·2025 Th., by the thermometer. The water stood 10·1 parts, about 35 millionths, higher at the end of the experiments than at the beginning. This gives 0·202 Th., which is as perfect an agreement as could be desired, the difference being only 0·0005 of the thermical measure, or 0·09 degree of the scale of Fahrenheit. During the last three months I have not made use of the tube *c d* for measuring the compression of air, but I have employed a glass tube L M N (fig. 2.), whose shape is better seen in the diagram than it can be described. The capacity of the part above the line *y*, and that of the whole, are measured by weights of mercury. When the instrument is sunk in the water, the liquid mounts in the tube which has the scale O, whose parts are likewise measured by mercury. This has the double advantage of giving a more accurate measure, and of showing whether or not the volume of air has changed. In the series of experiments above mentioned this measure has been employed. By a considerable number of experiments, I have found that the compressibility of water is not so great in high temperatures as in lower. Canton had already obtained this result, but some doubts might remain, because his experiments were made by means much more troublesome to make use of, and at a time when all instruments were less perfect. Here, as well as in the whole research into the compressibility of water, the new experiments prove the great skill and acute judgement of this distinguished philosopher. My experiments are much more numerous than his, and have been extended to a greater range of temperatures. Their results may be expressed by supposing that the pressure of one atmosphere equivalent to 336 French lines' height of mercury develops a heat 0·00025 Th. = 0·045° Fahr. In calculating this I have made use of the tables of Professor Stampfer at Vienna, who finds the highest contraction of water at 0·0375 Th., or 38·75° Fahr.* At this point the recession of water by the pressure of one atmosphere is 46·77 millionths. At 0·09125 Th. the volume of the water augments 71·75 millionths, having its temperature augmented 0·01 Th. The heat developed by the pressure thus augments its volume $0 \cdot 00025 \, . \, \frac{71 \cdot 75}{0 \cdot 01} = 1 \cdot 79$ millionth, or the recession is $46 \cdot 77 - 1 \cdot 79 = 44 \cdot 98$ millionths. Actual experiments have given it 44·89, or 0·09 millionth

* PART OF STAMPFER'S TABLE.

Temperatures according to the centigrade thermometer.	Volumes of the water.	Differences.
−3	1·000373	
2	1·000269	104
1	1·000182	87
0	1·000113	69
+1	1·000061	52
2	1·000025	36
3	1·000005	20
3·75	1·000000	5
4	1·000001	1
5	1·000012	11
6	1·000038	26
7	1·000079	41
8	1·000135	56
9	1·000205	70
10	1·000289	84
11	1·000387	98
12	1·000497	110
13	1·000620	123
14	1·000757	137
15	1·000906	149
16	1·001066	160
17	1·001239	173
18	1·001422	183
19	1·001617	195
20	1·001822	205
21	1·002039	217
22	1·002265	226
23	1·002502	237
24	1·002749	247
25	1·003005	256
26	1·003271	266
27	1·003545	274
28	1·003828	283
29	1·004119	291
30	1·004418	299

greater. The coincidence is often less perfect. At 0·1775 Th. the quantity calculated is 42·65, the quantity given by experiment 43·03, a difference of 0·38 millionth. The experiment mentioned above gave a recession = 41·51 at 0·20125 Th. (mean of the temperatures of the beginning and end of the series). The calculation gives 41·63, or a difference of 0·12 millionth. At 0·005 Th. the change of volume produced by one 0·01 is 60·5 millionths, but inversely, as the water at low temperature loses in volume by augmented heat; thus an addition is to be made equal to $\frac{60 \cdot 5}{0 \cdot 01} \cdot 0 \cdot 00025 = 1 \cdot 5$. Now $46 \cdot 77 + 1 \cdot 5$ gives 48·27, experiment 48·02. At 0·019 the quantity calculated is 47·72, that given by experiment 47·97. I have not yet finished the tedious discussion of all the experiments, but as far as I have proceeded the agreement of the hypothesis with facts is satisfactory. Messrs. Colladon and Sturm have in the calculation of their experiments introduced a correction founded upon the supposition that the glass of the bottle in which the water is compressed should suffer a compression so great as to have an influence upon the results. Their supposition is, that the diminution of volume produced by a pressure on all sides can be calculated by the change of length which takes place in a rod during longitudinal traction or pression. Thus, a rod of glass, lengthened by a traction equal to the weight of the atmosphere as much as 1·1 millionth, should by an equal pression on all sides lose 3·3 millionths, or, according to a calculation by the illustrious Poisson, 1·65 millionth. As the mathematical calculation here is founded upon physical suppositions, it is not only allowable, but necessary, to try its results by experiment. Were the hypothesis of this calculation just, the result would be, that most of the solids were more compressible than mercury. For this purpose I have procured cylinders of glass, of lead, and of tin, which filled the greater part of a cylinder, to which a stopple of glass, perforated by a capillary tube, was adjusted by grinding. I have not yet exactly discussed all the experiments on this subject, but the numbers obtained are such as to show that the results are widely different from those calculated after the supposition above mentioned. The quantity assigned by this calculation to the glass is very small indeed, yet the experiment gives it much less. Lead, which extends, according to Tredgold, 20·45 millionths by a weight equal to that of the atmosphere, and thus much more by the pressure on all sides, does not change one millionth. Tin is not more compressible. The inverse experiment is, perhaps, still more striking. I published it some years ago: however, as I have now repeated these experiments, and as they appear hitherto not to have satisfied philosophers, I shall here mention, that in all my experiments upon the subject, I have invariably found that the recession of the water in the capillary tube is about 1·5 millionth greater in bottles of lead or tin than in those of glass. Supposing the compressibility of the solid bodies to be so small that it cannot be observed in those experiments, yet the heat developed by the compression, feeble as it is, produces a small augmentation of the recession of the water in the capillary tube. If the dilatation of a rod of glass by 1 Th. is 0·0009, its cubical dilatation is 0·0027, and the dilatation by an increase of 0·00025 is 0·000000675, or nearly 7 ten millionths. The dilatation of lead is about 3 times greater, and the bottle containing it must get an increase of 0·00000225, which exceeds the former by more than 1·5 millionth. The dilatation of tin should give only one millionth more than glass, but it seems to give a little more, yet the quantity is not great. After all this, I think that the true compressibility of water is about 46·1 millionths, and that the apparent compressibility depends upon the effect of the heat developed by the compression, by which the liquid and the bottle are dilated.

"My continued experiments have confirmed my earlier result, that the differences of volume in the compressed water are proportionate to the compressing power. I do not know if the method I have made use of to try the effects of high compression has been published in England. These experiments cannot be made in a cylinder of glass; one of metal is required. As, in this case, the opacity prevents direct observations being made, an index, nearly like that in Six's register-thermometer, is placed in the capillary tube of the bottle. This tube is dilated a little at the top, so as to form a minute funnel. Some drops of mercury are poured into it, which being pressed, pushes the index forward; thus the recession may be seen when the bottle is taken out of the large cylinder. The compression of the air is measured in another way: a bent tube, of the form shown in fig. 3, is fixed in a glass vessel F G H I containing mercury, and exposed to the pressure together with the bottle. The pressure of the piston upon the water in the cylinder is communicated to the mercury, and pushes it into the wide part of the tube, as far as the resistance of the air will permit. The weight of the mercury driven into the wide part A B C D, together with that which has filled D E, and which may be computed, compared with the weight of mercury which the whole tube can admit, gives the volume of the air compressed. By this kind of experiment I have found that the decrease of volume produced by pressure preserves the same proportion to the pressing power as far as the pressure of 65 atmospheres, and probably much further; but how far, I have not hitherto been able to try, my apparatus not having resisted a greater pressure.

"I have thus given you a short abstract of my researches into the compressibility of water. They may be considered as a continuation of those of Canton. I should feel much flattered if they should obtain the approbation of the philosophers of the country where the first good experiments upon the subject have been made."

On some Results of the View of a Characteristic Function in Optics. By WILLIAM R. HAMILTON, *M.R.I.A., Royal Astronomer of Ireland.*

The author gave a statement of some optical results, deduced from the view which he had explained in the preceding year at Oxford.

His general method, for the study of optical systems, consists in expressing the properties of any optical combination by the form of ONE CHARACTERISTIC FUNCTION, one central or radical relation. In order to investigate the properties of the systems of rays, produced by any object-glass, or atmosphere, or other optical instrument, or combination of surfaces and media, ordinary or extraordinary, he has proposed, as *a fundamental problem*, to express for any such combination, *the laws of dependence* of the *final and initial directions* of a linear path of light on the *final and initial positions* or points, and on the colour. And the solution which he has offered for this fundamental problem consists, 1st, in reducing by uniform methods (analogous to the methods of discussing the equation of a curve or surface,) these *several laws* of dependence (of the four extreme angles of direction of a curved or polygon ray on the six extreme coordinates and on the colour,) to that *one law*, different for different combinations, according to which his one characteristic function depends on the same seven variables. And 2ndly, in establishing uniform processes for the research of the form of this function, namely, the action or time of propagation of the light, for any proposed combination.

For example, in the case of a single plane mirror, supposed to coincide with the plane of $x\, y$, we may propose to determine the laws of the two extreme directions of the linear path by which light goes to an eye $(x\, y\, z)$ from an object $(x'\, y'\, z')$, or (expressing the same thing more fully,) to determine the final cosines $\alpha\, \beta\, \gamma$, and the initial cosines $\alpha'\, \beta'\, \gamma'$, of the inclinations of this bent path to the positive semiaxes of coordinates, as functions of $x\ y\ z$, $x'\ y'\ z'$, that is, of the six extreme coordinates themselves, the colour being here indifferent. And Mr. Hamilton's general solution, for this and for all other questions respecting combinations of ordinary reflectors,—a solution which is itself a particular case of a more general result, extending to all optical combinations,—is expressed by the following equations;

$$\left.\begin{array}{l} \alpha = \dfrac{\delta V}{\delta x}, \ \beta = \dfrac{\delta V}{\delta y}, \ \gamma = \dfrac{\delta V}{\delta z}, \\ \alpha' = -\dfrac{\delta V}{\delta x'}, \ \beta' = -\dfrac{\delta V}{\delta y'}, \ \gamma' = -\dfrac{\delta V}{\delta z'}, \end{array}\right\} \qquad (1.)$$

the characteristic function V representing, in all questions respecting combinations of reflectors, the length of the bent path of the light, and being for the present mirror of the form

$$V = \sqrt{(x - x')^2 + (y - y')^2 + (z + z')^2}, \qquad (2.)$$

but being different in other cases. Thus, for a reflecting sphere, or for a Newtonian telescope, the length of a bent path of light would depend differently on the extreme points of that path, and we should have a different form for the *characteristic function* V; but by substituting this new form in the equations (1.), we should still deduce the connected forms of the six *direction-functions* or direction-cosines, $\alpha\,\beta\,\gamma$, $\alpha'\,\beta'\,\gamma'$, and so might deduce all the other properties of the telescope; at least, all the properties connected with its effects upon systems of rays.

It may be perceived from what has been said, that Mr. Hamilton divides mathematical optics into two principal parts: one part proposing to *find* in every particular case the form of the characteristic function V, and the other part proposing to *use* it: as in algebraical geometry, it is one class of problems to *determine* the equations of curves or surfaces which satisfy assigned conditions; and it is another class of problems to *discuss* these equations when determined. The investigations which the author has printed in the fifteenth, sixteenth, and seventeenth volumes of the *Transactions of the Royal Irish Academy*, contain examples of both these inquiries, although they relate chiefly to the second part, or second class of problems, namely, to the *using* of his function, supposed found. He has endeavoured to establish, for such using, a system of general formulæ, and has deduced many general consequences and properties of optical systems, independent of the particular shapes and positions and other peculiarities of the surfaces and media of any optical combination. A few results less general than these, and yet themselves extensive, may not improperly, perhaps, be mentioned here.

When we wish to study the properties of any object-glass, or eye-glass, or other instrument *in vacuo*, symmetric in all respects, about one axis of revolution, we may take this for the axis of z, and we shall still have the equations (1.), the *characteristic function* V being now a function of the five quantities, $x^2 + y^2$, $x\,x' + y\,y'$, $x'^2 + y'^2$, z, z', involving also, in general, the colour, and having its form determined by the properties of the instrument of revolution. Reciprocally, these properties of the instrument are included in the form of the characteristic function V, or in the form of this other connected function,

$$\mathrm{T} = \alpha\,x + \beta\,y + \gamma\,z - \alpha'\,x' - \beta'\,y' - \gamma'\,z' - \mathrm{V}, \tag{3.}$$

which may be considered as depending on only three independent variables besides the colour; namely, on the inclinations of the final and initial portions of a luminous path to each other and to the axis of the instrument. Algebraically, T is in general a function of the colour and of the three quantities, $\alpha^2 + \beta^2$, $\alpha\,\alpha' + \beta\,\beta'$, $\alpha'^2 + \beta'^2$; and it may *usually* (though not in every case) be developed according to ascending powers, positive and integer, of these three latter quantities, which in most applications are small, of the order of the squares of the inclinations. We may therefore in most cases confine ourselves to an approximate expression of the form

$$\mathrm{T} = \mathrm{T}^{(0)} + \mathrm{T}^{(2)} + \mathrm{T}^{(4)}, \tag{4.}$$

in which $\mathrm{T}^{(0)}$ is independent of the inclinations: $\mathrm{T}^{(2)}$ is small of the second order, if those inclinations be small, and is of the form

$$\mathrm{T}^{(2)} = \mathrm{P}\,(\alpha^2 + \beta^2) + \mathrm{P}_{\prime}\,(\alpha\,\alpha' + \beta\,\beta') + \mathrm{P}'\,(\alpha'^2 + \beta'^2); \tag{5.}$$

and $\mathrm{T}^{(4)}$ is small of the fourth order, and is of the form

$$\left.\begin{aligned}\mathrm{T}^{(4)} = {}&\mathrm{Q}\,(\alpha^2+\beta^2)^2 + \mathrm{Q}_{\prime}\,(\alpha^2+\beta^2)\,(\alpha\,\alpha'+\beta\,\beta') + \mathrm{Q}'(\alpha^2+\beta^2)\,(\alpha'^2+\beta'^2)\\ &+ \mathrm{Q}_{\prime\prime}\,(\alpha\,\alpha'+\beta\,\beta')^2 + \mathrm{Q}'_{\prime}\,(\alpha\,\alpha'+\beta\,\beta')\,(\alpha'^2+\beta'^2) + \mathrm{Q}''\,(\alpha'^2+\beta'^2)^2;\end{aligned}\right\} \tag{6.}$$

the nine coefficients, $\mathrm{P}\;\mathrm{P}_{\prime}\;\mathrm{P}'\;\mathrm{Q}\;\mathrm{Q}_{\prime}\;\mathrm{Q}'\;\mathrm{Q}_{\prime\prime}\;\mathrm{Q}'_{\prime}\;\mathrm{Q}''$, being either constant, or at least only functions of the colour. The optical properties of the instrument, to a great degree of approximation, depend usually on these nine coefficients and on their chromatic variations, because the function T may in most cases be very approximately expressed by them, and because the fundamental equations (1.) may rigorously be thus transformed;

$$\left.\begin{aligned} x - \frac{\alpha}{\gamma} z &= \frac{\delta T}{\delta \alpha}, & y - \frac{\beta}{\gamma} z &= \frac{\delta T}{\delta \beta}, \\ x' - \frac{\alpha'}{\gamma'} z' &= -\frac{\delta T}{\delta \alpha'}, & y' - \frac{\beta'}{\gamma'} z' &= -\frac{\delta T}{\delta \beta'}. \end{aligned}\right\} \quad (7.)$$

The first three coefficients, $P\ P_{\prime}\ P'$, which enter by (5.) into the expression of the term $T^{(2)}$, are those on which the focal lengths, the magnifying powers, and the chromatic aberrations depend: the spherical aberrations, whether for direct or inclined rays, from a near or distant object, at either side of the instrument (but not too far from the axis), depend on the six other coefficients, $Q\ Q_{\prime}\ Q'\ Q_{\prime\prime}\ Q'_{\prime}\ Q''$, in the expression of the term $T^{(4)}$. Here, then, we have already a new and remarkable property of object-glasses, and eye-glasses, and other optical instruments of revolution; namely, that all the circumstances of their *spherical aberrations*, however varied by distance or inclination, depend (usually) on the values of SIX RADICAL CONSTANTS OF ABERRATION, and may be deduced from these six numbers by uniform and general processes. And as, by employing general symbols to denote the constant coefficients or elements of an elliptic orbit, it is possible to deduce results extending to all such orbits, which can afterwards be particularised for each; so, by employing general symbols for the six constants of spherical aberration, suggested by the foregoing theory, it is possible to deduce general results respecting the aberrational properties of optical instruments of revolution, and to combine these afterwards with the peculiarities of each particular instrument by substituting the numerical values of its own particular constants. The author proceeds to mention some of the general consequences to which this view has conducted him, respecting the aberrational properties of optical instruments of this kind.

When a luminous point is placed on the axis of an object-glass, or eye-glass, or other instrument of revolution, and when its rays are not refracted or reflected so as to converge exactly to, or diverge exactly from, one common focus, they become, as it is well known, all tangents to one caustic surface of revolution, and they all intersect the axis, at least when they are prolonged, if necessary, behind the instrument. But if the luminous point be anywhere out of the axis, the arrangement of the final rays becomes less simple than before. They are not now all tangents to the meridian of a surface of revolution, nor do they all intersect the axis of the instrument; they become, by another known theorem, the tangents to *two caustic* *surfaces,* and to *two sets of caustic curves,* and compose *two series of developable pencils,* or ray surfaces; so that each ray of the final system may be considered as having, in general, *two foci,* or points of intersection with other rays, indefinitely near. The theorem here alluded to, namely, that of the general existence of *two foci* for each ray of a system proceeding from any surface according to any law, was first discovered by Malus. Mr. Hamilton also obtained it independently, but later, in 1823. It appears to be, as yet, but little known; but it is, he thinks, essential to a correct view of the arrangement of rays in space, for which the analogy of rays in a plane seems quite inadequate. Combining this theorem of the two foci with his view of the characteristic function, and of the six constants of spherical aberration, for the final system produced by oblique incidence on an instrument of revolution, the author has found that the two foci of a ray of this final system do not in general close up into one, except for TWO PRINCIPAL RAYS, having each its own PRINCIPAL FOCUS. The interval between the two foci of any other ray is proportional, very nearly, to the product of the sines of its inclinations to the two principal rays; and the tangent planes of the two developable pencils, passing through any variable ray, bisect (very nearly) the two pairs of supplemental dihedrate angles formed by the two planes which contain this variable ray and are parallel to the two principal rays; in such a manner that all the rays of any developable pencil of one set have (very nearly) one common *sum,* and all the rays of any developable pencil of the other set have (very nearly) one common *difference,* of inclinations to the same two principal rays, or *axes of the final system.* These latter axes always intersect each other, and their plane is either the diametral plane of the instrument (containing the luminous point or focus of incident rays), or a plane perpendicular to that diametral plane, according to the sign of a certain quantity, which vanishes when the two axes happen to coincide in one principal ray, round which the whole final system has then a very perfect symmetry; and, in general, the angle of the two principal rays, whether in or out of the diametral plane of the instrument, is bisected (very nearly) by a certain intermediate ray in that plane, which may be called the CENTRAL RAY of the system, because the other final rays are disposed about it with a certain symmetry of arrangement, less perfect than the symmetry about an axis of revolution, but resembling that of the normals to an ellipsoid about one of its three axes, when unequal; and accordingly the author finds that the final rays from an instrument of revolution (when the incident rays are oblique) are very nearly normals to a portion of such an ellipsoid, having the central ray for one of its three unequal axes, and having the two principal rays for its two umbilical normals, at two out of the four points where the ellipsoid has complete contact of the second order with an osculating sphere. The centres of the two osculating spheres at these two points are the two principal foci of the system; and the centres of the two extreme osculating spheres at any other point of the ellipsoid are the two foci of the corresponding ray, or the points at which that ray touches the two caustic surfaces. These latter surfaces are, in the present approximation, the surfaces of centres of curvature of the ellipsoid: they have a curve of intersection with each other, which contains the two principal foci; every point upon the curve, except these two, being the first focus of one ray and the second focus of another. A plane may be drawn perpendicular to the central ray, and passing through the two principal foci; and this plane will cut the two caustic surfaces in sections which compose a kind of little lozenge, consisting (very nearly) of two curvilinear equilateral triangles, having the principal foci for two common corners: the quadrature of these curvilinear triangles, and of the other sections of the caustic surfaces, depending on elliptic integrals. In all the foregoing remarks, it is supposed, for greater generality, that the aberrations do not vanish with the obliquity of the incident rays; but when the instrument is aplanatic for direct incident rays, it is easy to apply the same theory of the characteristic function and the six radical constants of aberration, and to determine, for this particular case, the components of spherical aberration which arise from obliquity only.

This theory of the aberrations of oblique rays, for an optical instrument of revolution, may admit of practical applications. For the mathematical symmetry of arrangement of the final rays about the central ray of their system, and the intensity of the two principal foci, may perhaps affect our sight, and have some appreciable influence on the practical performance of an instrument; but of this Mr. Hamilton speaks with diffidence, because experiments directed expressly to the question appear to be required for its decision. If the mathematical properties which he has determined by theory in the arrangement and aberrations of a system, shall be found in practice to have any sensible influence on the phænomena of oblique vision, it will become necessary to alter some of the received rules for the construction of telescopes and microscopes; or, at least, it will be possible to improve those rules by following the indications of this theory. A new track seems to be opened thus to mathematical and practical opticians.

The principle of the characteristic function, from which have been deduced the foregoing results, among others not yet published, respecting optical instruments of revolution, may be applied to every part of mathematical and perhaps of physical optics; and an analogous function and method may be introduced in other sciences, especially in dynamical astronomy*. But the author confines himself to mentioning the application which he has made of the principle to the study of the laws of extraordinary refraction in the crystals called biaxal. The general laws of reflection and refraction, ordinary and extraordinary, at any point of any surface, are expressed by his function as follows, when the normal to the reflecting or refracting surface at the point of incidence is taken for the axis of z:

$$\Delta \frac{\delta V}{\delta x} = 0; \; \Delta \frac{\delta V}{\delta y} = 0: \qquad (8.)$$

and in the language of the undulatory theory they may be enunciated by saying, that if the normal slowness of propagation of a luminous wave, at any point of incidence on any reflecting or refracting surface, be decomposed in any direction parallel to this surface at this point, *the component of normal slowness is not altered by reflection or refraction.* In the case of ordinary refraction, this comes to saying, that if on the incident ray prolonged, and on the refracted ray, we measure from the point of incidence lengths represented by the indices of the first and second media, those lengths will have one common projection on the refracting surface or on its tangent plane; which is a form for the law of Snellius. For extraordinary refraction, we must in general construct the normal slowness of a wave by a variable length not always coinciding with the ray; but the two lengths thus substituted for the two successive indices will still have one common projection on the refracting face of the crystal, if plane, or on its tangent plane, if it be curved. If now we seek the locus of the end of the line, which represents in length and direction the normal slowness of a wave, for all possible directions of this slowness, we get for ordinary media a sphere, but for extraordinary media (on Fresnel's principles) a certain double surface, which is not the

* See the *Dublin University Review* for October 1833. Mr. Hamilton has since developed the dynamical application of his principle, in an essay *On a General Method in Dynamics*, which has been presented to the Royal Society, and ordered to appear in the *Philosophical Transactions* for 1834.

same as Fresnel's curved wave-surface, propagated in all directions from a point, but is connected therewith by several remarkable relations of reciprocity, and may be called the *surface of components*, since its coordinates are themselves the components of normal slowness of propagation. They are equal to the partial differential coefficients of the first order of the author's characteristic function V, and are connected by a partial differential equation of the form

$$0 = \Omega \left(\frac{\delta V}{\delta x}, \frac{\delta V}{\delta y}, \frac{\delta V}{\delta z} \right), \tag{9.}$$

which may be regarded as the equation of the surface. And the general equations of reflection or refraction (8.), when put under the form

$$\frac{\delta V}{\delta x} + \Delta \frac{\delta V}{\delta x} = \frac{\delta V}{\delta x'}, \quad \frac{\delta V}{\delta y} + \Delta \frac{\delta V}{\delta y} = \frac{\delta V}{\delta y'}, \tag{10.}$$

express that the corresponding points on the two surfaces of components, before and after any reflection or refraction, ordinary or extraordinary, are on one common ordinate to the reflecting or refracting surface, or to its tangent plane; which gives a new and general construction for the direction of a reflected or refracted wave, and therefore for that of a reflected or refracted ray, simpler in many cases than the construction proposed by Huygens. Thus, if it were required to determine by this new construction the direction and the undulatory velocity of an extraordinary ray, refracted in Iceland spar, being given the direction of the incident ray in air, we should have to construct first the two successive surfaces of components, which would be here a sphere for the air, and a spheroid (not the Huygenian) for the crystal, the common centre of both being at the point of incidence; and then, after determining the point of the hemispheroid within the crystal, which is on the same ordinate to the refracting face as the point where the incident ray prolonged meets its own interior hemisphere, we should only have to draw a tangent plane to the spheroid at the point thus determined, and to let fall a perpendicular on this plane from the point of incidence; for this perpendicular is, in length and direction, the radius vector of the Huygenian spheroid, and therefore represents the undulatory velocity and the direction of the extraordinary ray. And other more complicated cases may be treated in a similar manner, either by using a construction of this kind, or by the equivalent formulæ derived from the characteristic function.

When the author proceeded to apply this general method to

Fresnel's principles respecting biaxal crystals, he arrived at the curious result that the surface of components, in such a crystal, has not at every point a determined tangent plane, but that at each of *four cusps*, opposite, two by two, it is touched by an infinite number of such planes, or by *a tangent cone*; and hence he immediately concluded, by his general method, that if a ray in air fall so upon a biaxal crystal as to make the point upon the air-sphere correspond (by the rule already explained) to one of those cusps on the surface of components of normal slowness in the crystal, his construction would give no unique refracted ray, nor even a pair or other finite number of such rays within the crystal, but *an infinite number of refracted rays*, namely, all the perpendiculars which can be let fall from the point of incidence on the tangent cone at the cusp. The author saw also that these rays must terminate in some *curve of plane contact on Fresnel's double wave*, in the whole extent of which curve the wave must be touched by one plane, and that there must be four such curves, which he afterwards found to be *circles*; a curious property of this wave, which Fresnel himself had not noticed. But the most remarkable part of this result was, the new and delicate experimental test which it offered for Fresnel's principles, since the INTERNAL CONICAL REFRACTION which it indicated, for certain cases of incidence on a biaxal crystal, had not only not been hitherto observed, but seemed contrary to all former analogies of observation; so that if this theoretical consequence of Fresnel's principles, which he had not himself perceived, should be verified by subsequent experiment, the principles would receive a new and striking confirmation; and if, on the contrary, after all due care employed in experiments directed expressly to the question, the small but finite conical dispersion in biaxal crystals, which the author had thus theoretically concluded, should not be found in fact to take place, the principles themselves would require to be abandoned or modified. Professor Lloyd was applied to by the author to undertake this experimental inquiry. After some unsuccessful trials with crystals of insufficient size and purity, he obtained a fine piece of arragonite from Mr. Dollond, and at length completely succeeded in exhibiting the phænomenon which Mr. Hamilton had expected. The rays of the internal cone emerged, as they ought, in a cylinder from the second face of the crystal; and the size of this nearly circular cylinder, though small, was decidedly perceptible, so that with solar light it threw on silver paper a little luminous ring, which seemed to remain the same at different distances of the paper from the arragonite. Professor Lloyd describes the appearance as very beautiful when he employed a lamp, and received the emergent rays on a lens: he seemed to see the two points of light, which the double refraction usually produced, spread out on a sudden, when the critical incidence was obtained, into a ring of gold viewed on a dark ground. His account is contained (with Professor Hamilton's theoretical investigation) in the First Part of the seventeenth volume of the *Transactions of the Royal Irish Academy*; a shorter statement was also published in the numbers of the *London and Edinburgh Philosophical Magazine* for the months of February and March 1833.

From the connexion of the surface of components with the wave-surface propagated from a point, the author saw that the existence of four conoidal cusps on the one surface in Fresnel's theory involved the existence of four such cusps upon the other, namely, at the points of intersection of Fresnel's circle and ellipse in the plane of the extreme axes of elasticity: and thus he was led to expect an EXTERNAL CONICAL REFRACTION, corresponding to the internal incidence of a *cusp-ray* when emerging into air from a crystal with two axes. On this point also he requested Professor Lloyd to undertake a series of experiments; and on this point also (indeed, somewhat sooner than on the other,) he obtained a complete verification. His experimental determinations of the size and position of this emergent cone, as of the former emergent cylinder, and of the laws of polarization in each, for the same large piece of arragonite, agreed with the theoretical results deduced from the principles of Fresnel by the method of the Characteristic Function.

Although this method appears likely to be adopted by analysts at some future time in the researches of theoretical optics, the author does not pretend that its results cannot be obtained in other ways; and with respect to the two kinds of conical refraction, in particular, Mr. MacCullagh (F.T.C.D.) has published, in the *London and Edinburgh Philosophical Magazine* for the months of August and September in the present year, an elegant geometrical investigation, together with some account of the progress of his thoughts upon the subject. The surface which Professor Hamilton has called the *surface of components*, (of normal slowness of propagation,) and to which he was conducted some years ago, as constructing a fundamental equation between the partial differential coefficients of his Characteristic Function, occurred to Mr. MacCullagh also, as he has informed the author, independently from considerations of a geometrical kind. The same important surface presented itself to M. Cauchy, likewise, in his dynamical researches respecting a system of attracting or repelling points.

On Conical Refraction. By the Rev. H. Lloyd, *Professor of Natural and Experimental Philosophy in the University of Dublin.*

Professor Lloyd gave a brief account of the experiments by which he established the existence of conical refraction in biaxal crystals, in conformity with the theoretical anticipations of Professor Hamilton.

The substance employed in these experiments was arragonite, which was selected chiefly on account of the magnitude of its biaxal energy. The specimen was one of remarkable purity, procured by Mr. Dollond. Its thickness was ·49 of an inch, and its parallel faces were perpendicular to the line bisecting the optic axes, being cleavage planes of the crystal.

The first case of conical refraction examined by the author was that called by Professsor Hamilton *external conical refraction.* It was expected to take place when a single ray passes within the crystal in the direction of the line connecting two opposite cusps on the wave-surface. When this is the case, Professor Hamilton has shown that there should be a cone of rays without, the magnitude of which will depend on the biaxal energy of the crystal. In the case of arragonite, the angle of the cone, calculated from the elements of the crystal as determined by M. Rudberg, amounts to 3° very nearly.

A thin metallic plate, perforated with a minute aperture, was placed on each face of the crystal, and these were adjusted in such a manner that the line connecting the apertures should coincide nearly with one of the optic axes. The flame of a lamp was then brought near the first surface of the crystal, and in such a position that the central part of the beam converging to the aperture should have an incidence of between 15° and 16°. When the adjustment was completed, there appeared, on looking through the aperture on the second surface, a brilliant luminous circle with a small dark space around its centre; and in this central dark space were two bright points, separated by a well defined dark line. When the plate on the second surface was slightly shifted, so that the line connecting the two apertures no longer coincided accurately with the line joining the cusps on the wave-surface, the phænomena rapidly changed, and ultimately resolved themselves into the two pencils into which a single ray is divided under ordinary circumstances.

The incident converging cone was sometimes formed by a lens of short focus, placed at the distance of its focal length from the surface. In this case the lamp was removed to a considerable distance, and the plate on the first surface dispensed with. The same experiment was repeated with the sun's light, instead of that of a lamp, and the emergent cone of rays thus formed was of sufficient intensity to be reflected from a screen at some distance. In this manner the section of the cone was observed at various distances from its summit.

When these phænomena were examined in detail, and compared with the results of theory, they appeared to differ in two important particulars. In the first place, the observed cone was very nearly a solid cone of rays, while that of theory was but a conical surface. Secondly, the two cones differed widely in magnitude, the angle of the experimental cone being nearly double of that of the theoretical one. This discordance between the results of experiment and those of theory, the author conceived to arise from the rays which were inclined to the optic axis at small angles, and which were transmitted through the aperture on the second surface in consequence of its sensible magnitude. To examine this point he proceeded in the next place to try the effects of apertures of various dimensions. The effects of these variations in the resulting phænomena corresponded exactly with his preconceived views. The rays which in the first experiments filled the whole of the conical space, parted in the centre, when the aperture was much diminished; and the section of the cone, instead of a complete luminous circle, was reduced to a luminous annulus, whose breadth diminished with the aperture. Simple theoretical considerations showed that the angle of the true cone in this case must be, very nearly, half the sum of the angles of the observed interior and exterior cones; and when this correction was applied to the measurements, the resulting angle agreed, as nearly as could be expected, with that deduced from theory.

A remarkable variation of the phænomenon was obtained by substituting a narrow linear aperture for the small circular one, in the plate next the lamp in the first-mentioned mode of performing the experiment; and by adjusting it so that the plane passing through it and the aperture on the second surface should coincide with the plane of the optic axes. In this case, according to the received theory, all the rays transmitted through the two apertures should be refracted doubly in the plane of the optic axes, so that no part of the line should appear enlarged in breadth on looking through the aperture on the second surface. But if Professor Hamilton's conclusion be physically exact, the ray which proceeds in the direction of the line joining two opposite cusps on the wave-surface should be refracted in every plane. This was accordingly found to be the case. In the neighbourhood of each of the optic axes the luminous line swelled out on either side of the plane of the axes in an oval curve. This curve is the conchoid of Nicomedes, whose asymptot is the line on the first surface; and its variations of form, as the plane passing through the two apertures deviated from the plane of the optic axes, were highly curious and remarkable.

Examining the state of polarization of the rays composing the emergent cone, Mr. Lloyd discovered that they observed the following law, namely, that "the angle between the planes of polarization of any two rays of the cone is half the angle contained by the planes passing through the rays themselves and its axis." This law, it is easy to show, is in perfect accordance with theory.

Internal conical refraction should take place, according to Professor Hamilton, when a single ray has been incident externally upon a biaxal crystal in such a manner that one of the refracted rays may coincide with an optic axis. The ray, in this case, ought to be divided into a cone of rays within the crystal, the angle of which in the case of arragonite is 1° 55′. The rays forming this cone will be refracted at the second surface of the crystal in directions parallel to the ray incident on the first; so that they will form a small cylinder of rays in air, whose base is the section of the cone formed by the surface of emergence.

The minuteness of this phænomenon, and the perfect accuracy required in the incidence, render it much more difficult of detection than the former. A very fine ray of light proceeding from a distant lamp was suffered to fall upon the crystal, and the position of the latter altered with extreme slowness, so as to change the incidence very gradually. When the required position was attained, the two rays suddenly spread into a continuous circle, whose diameter was apparently equal to their former distance. The same experiment was repeated with the sun's light, and the emergent cylinder received on a small screen of paper at various distances from the crystal. No sensible enlargement of the section was visible on increasing the distance.

The magnitude of the angle of the cone of rays within the crystal was ascertained experimentally, and agreed within very narrow limits with that deduced from theory. The rays composing the cone were all polarized in different planes, and the law connecting the planes of polarization was the same as that already found in the case of external conical refraction*.

On the Absorption of Light by coloured Media, viewed in connexion with the undulatory Theory. By Sir John F. W. Herschel, F.R.S.

The absorption of light by coloured media having been of late regarded as offering peculiar difficulties when attempted to be reconciled with the undulatory theory, the object of this paper is to point out considerations which appear capable of reconciling most of the phænomena with that theory, at least in so far as to show that they involve nothing contrary to sound dynamical principles and obvious analogies when regarded as particular cases of the general doctrine of undulatory motion.

The extinction of light in general is first considered. In the corpuscular hypothesis, light, to be extinguished, must be annihilated, or transformed either into caloric or some other form of matter. On the undulatory hypothesis, the extinction of light resolves itself into the more general and highly interesting dynamical question, "What becomes of *Motion?*" This question is therefore considered, and by tracing the progress of an undulation through a body imperfectly elastic or a system of bodies placed in communication, it is shown that a continual subdivision of the original undulation, and a perpetual internal reflection of the subdivided portions, will speedily have the effect of agitating every molecule of the system at one and the same instant, with vibratory motions, in every possible phase and direction. The dynamical state of a molecule so agitated is identical with a state of perfect rest, so that the general fact of the extinction or absorption of light within a medium imperfectly transparent is so far from being repugnant to dynamical principles, that it is one of their immediate and most natural consequences; imperfect transparency, in this view of the subject, consisting in the juxtaposition of parts unequally elastic, or of portions of the etherial medium *unequally loaded*, or restrained from their natural aptitude to motion by the gross particles of material bodies with which they are connected.

This connexion of the etherial particles with the grosser molecules which constitute our solids, liquids, and gases, may be considered as giving rise to compound vibrating systems, each of which, it is easy to conceive, may have a greater apti-

* A full account of these experiments is given in a memoir read before the Royal Irish Academy, on the 28th January 1833, and ordered to be published in the 17th volume of its *Transactions*.

tude to propagate vibrations of some determinate degree of frequency, than of a greater or less degree, as being in unison, or an approach to unison, with that in which they themselves would vibrate if existing alone, and agitated by an external impulse. But this greater disposition to propagate some vibrations does not render them *incapable* of transmitting others. In illustration of this position Sir John Herschel exhibited an experiment where the column of air in a closed pipe, being maintained in a state of forced vibration by two tuning-forks held over it, differing materially in pitch, yielded at the same instant both their sounds, being actually out of unison with itself, and uttering distinct *beats**. It is on this principle of *forced vibrations* that, according to the view here taken of the subject, the phænomena of absorption, or, as it should rather be called, of obstructed transmission, depend. The degree of obstruction which an undulation experiences from a medium will be in proportion to the *number* and *extent* of its subdivisions, in traversing the molecular systems of which that medium consists, and the latter will be greater the more completely out of unison those systems happen to be with the vibrations thus forced through them. To assign the law of degradation may be difficult, but it is easy to satisfy ourselves that it may depend on relations quite sufficiently varied and different in differently constituted molecules, to give rise to all the apparently capricious phænomena of absorption†.

On the Dispersive Powers of the Media of the Eye, in connexion with its Achromatism. By the Rev. BADEN POWELL, *M.A., F.R.S., Sav. Prof. of Geometry, Oxford.*

The principal experimental results bearing on the question of the achromatism of the eye are as follows:

At distances *less* than the limit of distinct vision the image of a luminous point is coloured; or if a small opake object be interposed, its shadow is coloured.

To near-sighted eyes, a small bright object on a dark ground appears edged with colour, and the effect is rendered more conspicuous by using a blue glass, which allows the extreme rays of the spectrum to pass, and stops or weakens the middle rays.

* This experiment, which was published by Sir John Herschel in his *Essay on Sound*, was then supposed by him to be new. It appears, however, to have been previously made by Mr. Wheatstone. See a very ingenious paper by that gentleman, "On the Resonance of Columns of Air," *Quarterly Journal of Science*, N.S. No. 5.

† The paper, of which the above is an abstract, has since been communicated to the *London and Edinburgh Philosophical Magazine and Journal*, vol. iii. p. 401.

Thus, when from *any cause* the rays do not converge accurately upon the retina, the dispersion is sensible.

To some eyes, however, it is scarcely appreciable, and in all cases is but small.

So long as direct centrical pencils have their mean rays converging accurately *upon* the retina, *no colours are perceptible.*

When the incident rays are partially intercepted by an opake body, the image is formed by eccentrical pencils, and the colours become very conspicuous.

Fraunhofer admitted the different prismatic rays successively into a telescope, and found it necessary, in passing from the red to the violet ray, to adjust both the eye-glass to the object-glass, and the eye-glass to the micrometer-wire, in order to see the wire distinctly in the different sorts of light.

The whole displacement must be the sum of the chromatic aberrations of the object-glass, the eye-glass, and the eye.

Fraunhofer does not notice the first; says that the second is allowed for; and takes the residuum as the dispersion of the eye.

He elsewhere states that the telescope was not perfectly achromatic; and as the data are not stated, we cannot regard the inference as conclusive, and Fraunhofer admits that it is not precise.

The author of this paper has tried similar experiments, but found the displacement so small that he is quite in doubt whether any was requisite: and considering that the aberrations of the lenses may be uncertain to a larger amount than the quantity sought, it cannot be satisfactorily deduced by this method.

The theories which have been proposed are by no means satisfactory.

D'Alembert conceives that the agitation (*ébranlement*) occasioned on any one point of the retina, extends itself to the neighbouring points, and thus each point is influenced by the sum of the effects due to all the coloured rays at once, and simple vision results.

Euler's explanation is grounded on hypothetical assumptions as to the dispersive powers.

This is remarked by Dr. Maskelyne, who by calculation finds the aberration so small as to be insensible. He, however, assumes the dispersions only by analogy from the proportions assigned by Newton.

Dr. Wells leaves it as a point yet to be explained.

Mr. Coddington, admitting a considerable difficulty in the subject, suggests that a compensation may take place between the refractions at the cornea and the crystalline.

Several writers observe, that the sensible achromatism is difficult to understand, *because* the refractions are all performed *one way*.

With a view to the theory, it is to be observed that although the *refractive* powers of the several media of the eye have been accurately determined, and the *dispersive indices* of the aqueous and vitreous humours, yet no observations of the separation of the different prismatic rays in these media have been given, and none for the dispersion of the crystalline lens.

The author has accordingly tried such experiments by forming portions of these media (from the eye of an ox) into prisms between glass plates, and measuring the proportions, both of the whole extent, and of the several parts, of the spectra which they produced.

In the vitreous humour this was easily done, as the whole spectrum was very distinct, and the principal lines well defined. With the crystalline it was extremely difficult to make the observations satisfactorily, and the results are less certain.

The measurement was made micrometrically by the rotation in its own plane of a plate-glass prism of very small angle, so as to give an image of the spectrum sensibly unaltered, but deviated in position, and any definite part of this image could be brought by rotation into coincidence with any part of the fixed image: the angular separation of the different parts or lines of the spectrum could be thus readily deduced from the arc of rotation observed.

For comparison of the prismatic dispersion of the vitreous and crystalline, the mean results of several sets of observations were as follows:

Angle of the prism, 75°.

	Angular separation of the extreme red form.		
	The mean yellow.	Boundary of green and blue.	Extreme violet.
Crystalline lens:			
Central portion	0° 21′	0° 42′	1° 22′
Outer portion	0 21	0 38	1 25
Vitreous humour	0 15	0 31	1 9

As far as such determinations can be relied on, it hence appears:

1st, That the dispersion of the several portions of the crystalline are nearly equal.

2ndly, That the separation of the different rays in the crystalline is greater than in the vitreous humour; but

3rdly, That they are as nearly as possible in the *same ratio*.

In theory, the principles on which achromatism might be produced in a combination similar to the eye are easily understood by reference to the common formula for the dispersion of a single lens. Let m, m_r, m_v, &c., be the absolute indices, relative to vacuum, of the medium of the lens, for the mean, red, violet, &c., rays.

The common formula applies to the focus for parallel rays formed *in vacuo*. Let us now suppose it formed in some other medium, whose absolute indices (as before) are $m_{\prime}$, $m_{r\prime}$, $m_{v\prime}$, &c., and f the principal focal length of the lens for mean rays in this medium. Then, instead of the common formula for the chromatic aberration (a), we shall have the analogous one

$$a = \left\{ \frac{\frac{m_v}{m_{v\prime}} - \frac{m_r}{m_{r\prime}}}{\frac{m}{m_{\prime}} - 1} \right\} f.$$

Here the numerator may be $= 0$, or the *aberration be destroyed*, whatever be the absolute values of the indices, provided only the *indices of corresponding rays are in the same ratio* in the two media.

By a construction of the course of the rays it will be easily seen, that with certain dispersive powers and curvature the rays will reunite at given distances.

This appears to be the same principle as that on which Sir David Brewster proposed an achromatic microscope. (*Phil. Instruments*, p. 408.)

On the Power of Glass of Antimony to reflect Light. By R. Potter, Jun.

In an abstract of a paper which is published in the preceding volume of these Reports, Mr. Potter gave the results he had obtained of the reflective power of diamond for light, by the method of photometry by comparison. Since then he had experimented with a large larke diamond belonging to Sir David Brewster, and obtained results nearly identical. He found this latter diamond to reflect at 2° to 3° incidence equally with crown-glass at 64° incidence, from which the various trials differed very little.

The ordinary surfaces on diamond being too small and the planes not sufficiently true to determine the reflective power at high incidences, he had recourse to glass of antimony, a substance of very high refractive power, to which he succeeded in giving a plane surface and high polish. Comparing such a surface with crown-glass, he obtained measures which enabled him to calculate the light reflected for all requisite incidences, from the known reflective power of crown-glass. These show that glass of antimony follows the same law in the reflection of light as low refracting substances, and that the number of rays reflected of every 100 incident may be calculated from the same formula, namely, $y = a \times \frac{c^2}{r + b - x}$, where y is the number of rays reflected of every 100, x the sine of incidence to radius (r), as 100; a, b, and c being constants, and having the following values for this substance, $a = 7{\cdot}4$, $b = 1{\cdot}25$, and $c = 9$. The maximum polarizing angle was found to be about 65°, so that the refractive index of the specimen would be about 2·1 to 2·2. The undulatory formula $\left(\frac{\mu' - \mu}{\mu' + \mu}\right)$, by using this value of μ, shows that according to the received principles of that theory, the reflection of glass of antimony at a perpendicular incidence should have been about 13·33 rays of every 100 incident, whilst experiment shows that only 8·20 such rays are really reflected.

On a Phænomenon in the Interference of Light hitherto undescribed. By R. Potter, Jun.

When the image of a luminous point is viewed in a Newtonian microscope, of which the mirror is of a spherical figure, and the aperture large in proportion to the radius of the sphere, there is a large circle of aberration visible. This appearance is best studied when the point, where the rays reflected near the edge of the mirror cross the axis, is in the focus of the eye-glass. The circle of aberration then appears to be composed of dark and bright rings, which are broad and distinct near the edge, but from the mixture of colours become soon lost in white light.

To produce the luminous point, Mr. Potter used a small mercurial globule, attached by some glutinous matter on a slender wire. When the image of the sun given by this globule is viewed in the microscope, the appearance of the rings formed by interference is one of the most beautiful of this class of phænomena. It may be seen by candlelight, but to much less advantage. Mr. Potter states, that on studying the theory of this case of interference he found that, according to the undulatory theory, the outer ring should be brightest on its outside edge, whilst, according to the theory of interference which he has proposed, it should shade away gradually into darkness on this outside edge; and he adds that the phænomenon corresponds exactly to this latter deduction.

Explanation of the Principle and Construction of the Actinometer. By Sir JOHN F. W. HERSCHEL, F.R.S.

This is an instrument, invented by Sir John F. W. Herschel, for measuring at any instant the direct heating power of the solar rays.

In the ordinary methods of estimating the sun's calorific effect, the heating power of the sun is put in equilibrium with the cooling influence of external causes, and the elevation of temperature in the body (usually a thermometer,) thus heated, so as to maintain the equilibrium, is noted and set down as a measure of the sun's calorific effect. But the principle of such a proceeding is obviously faulty: the temperature maintained is just as properly a measure of the cooling as of the heating influences, and, in fact, is a measure neither of one nor of the other. This objection applies to all *statical* measurements of *dynamical* effects, taking the word generally to mean cases where *power* is transformed into its immediate effect, and exhausted by such transformation. The actinometer is intended to afford a dynamical measure of the solar radiation, *i. e.* one in which the actual quantity of heat received per second or in a given infinitely small time is the object of measurement, upon a given surface directly exposed to the sun. The instrument itself is nothing more than a very large cylindrical thermometer-bulb with a scale greatly enlarged, so as to render the smallest possible increase of temperature distinctly measurable. In consequence, the scale is not divided into degrees, but into arbitrary equal parts; and as a very slight elevation of temperature suffices to carry the liquid up the whole column, the bulb itself is furnished with a screw, by withdrawing which its capacity may be enlarged, and the power of reading off restored. The bulb is of colourless glass, and is filled with an intensely blue liquid. Into the interior of this the calorific rays penetrate, and are absorbed at some sensible depth within the bulb, so that the liquid is heated *from within*, and the *whole* of the calorific rays (not stopped by the glass) go to dilate the contents, and are extinguished in producing that effect. To make an observation with the actinometer, the instrument must be freely exposed in the shade for one minute, and the variation of the reading noted; then freely exposed to sunshine for the same time, and the variation again noted; and lastly, once more in the shade. The mean of the two variations in the shade being subducted from the variation in the sun, the excess gives the dilatation *per minute* due to the sun's rays, the quantity subducted being the effect of the other causes in action at the time.

Many years' experience and repeated trial of the instrument under a great variety of circumstances have demonstrated the complete dependence which may be placed on its indications, as well as its extreme sensibility. It has been used by its inventor, Sir John Herschel, on the Puy de Dome; in an imperfect state also on Etna by him, and since by Mr. Lunn, in its improved form, on the same volcano; and by M. Nicollet and Professor Forbes on the Alps in Switzerland. In a series of experiments on the absorptive powers of glasses on heat, and on the reflective powers of metallic and other polished surfaces, as well as for the purpose of ascertaining the rate of increase and diminution of solar radiation throughout the day, from sunrise to sunset, it has been found of great utility. In the latter inquiry, the increase or diminution of altitude of the sun near the horizon is marked by the actinometer nearly as well as by actual measurement with a sextant*.

Sir John F. W. Herschel also communicated some results obtained by Professor Forbes on the diminution of the intensity of the solar rays in traversing the atmosphere by means of the actinometer. He pointed out by direct comparison with two instruments the degree of dependence to be placed on these results, which, from experiments made by Professor Forbes at the observatory of Paris, seem to indicate not more than $\frac{1}{100}$ of the whole effect of radiation as the probable error under favourable circumstances. But the point particularly deserving of attention, and upon which observations strictly comparative have now been made for the first time, is the numerical estimate of the loss of intensity suffered by the radiant heat of the sun in passing through a definite thickness of the atmosphere. Out of twenty series, each consisting of several (often *many*) individual comparisons made by Professor Forbes, with the assistance of M. Kämtz, of Halle, at the top and bottom of the Faulhorn, an elevated mountain of the Bernese Oberland, and under favourable circumstances, *not one* failed to indicate the diminution of heat alluded to. The difference of height at the different stations at which simultaneous observations were made, varied from 5000 to near 7000 English feet, and these obser-

* The actinometer, as used by Sir John Herschel, is made and sold by Mr. Robinson, optician, Devonshire-street, Portland-place, London.

vations were made on different days and at different hours of the same day. Professor Forbes has not yet finally reduced his observations; but from the approximate numbers communicated by Sir John Herschel to the Meeting, it appears that even in the clearest weather (in which alone these experiments were made,) the force of solar radiation is diminished by about *one fifth* in passing through a column of 6000 feet of the purest air.

Account of some recent Experiments on Radiant Heat. By M. MELLONI. *Communicated by Professor* FORBES.

The following truths at which M. Melloni has arrived are of capital importance. *The quantity of calorific rays which traverse a screen, derived from sources of high or low temperature, is proportional to that temperature; but the difference constantly* diminishes as the thickness of the screen *becomes less,* till at last, with excessively thin *laminæ of mica, it is insensible.* This remarkable result proves that the resistance to passage which affects the transmission of heat depending on the temperature of its source, is not exerted at the *surface,* but in the interior of the mass.

The extension of this observation to the solar rays gives the following result. M. Melloni operated with various thicknesses of sulphate of lime, water, and acids. He found that *the quantity of heat intercepted by increasing the thickness of the transparent medium is greater for the less refrangible than for the more refrangible rays;* that is, that while for example an evanescent pellicle of mica would permit as much per cent. of the heat accompanying the red ray to pass through it as of the violet ray, if the thickness be increased, a much larger percentage of the former will be stopped than of the latter; whence the author concluded that *the refrangibility of a heating ray is coordinate with, and a measure of, its intensity.* Thus, a plate of glass 2^{mm} in thickness stops, out of 100 incident rays of heat, 45 when they proceed from a lighted candle, 70 from a mass of copper at 950° Cent., 92 from a vessel of boiling mercury, and 100 from a vessel of boiling water.

Hence the impossibility, as he observes, by the use of lenses of glass and suchlike materials of concentrating heat of low temperature. This admits, however, of one most important exception, the discovery of which forms an epoch in the history of the science of heat. In a previous memoir, M. Melloni had compared the transmissive powers of a great number of substances. For uncrystallized compounds and fluids, he found the ratio of transmission for the heat of a lamp to be proportional to their refractive powers; but in crystallized solids the strangest anomalies appeared. The most remarkable of these was, that the transmissive power of rock-salt for heat was six or eight times greater than that of an equal thickness of alum, which had almost the same transparence and refractive powers. But the extraordinary property discovered by M. Melloni in rock-salt is this, that it is equally transparent for heat of all temperatures or of all degrees of refrangibility; that the heat of the hand loses as little in passing through a given thickness of it, as the same quantity of solar or any other kind of heat. This singular fact places rock-salt in a still more important position in relation to heat than the discovery of Sir David Brewster has already placed it in regard to light.

A plate of rock-salt 7^{mm} (·28 in.) in thickness transmits equal proportions of heat, radiating from red-hot iron, from the flame of oil or alcohol, boiling water, and water even below 120° Fahr.; and in all these cases the transmission was 92 per 100 of the incident rays. A piece about an inch in thickness was equally constant in the ratio of transmission.

To all who have been in the slightest degree occupied with the study of radiant heat, the value of this property will at once be apparent. It gives the means of making experiments hitherto physically impossible. The more delicate of M. Melloni's experiments are made with the thermo-multiplier; but many are capable of repetition by common instruments. The different transmissibility of alum and salt, for example, may be shown with the rudest apparatus. By means of a common three-sided prism, formed of rock-salt, M. Melloni can refract a pencil of heat derived from boiling water.

The following is an abstract of the later discoveries of M. Melloni, communicated by him to Professor Forbes, in order to be laid before the Association.

The colouring matter of glass has been proved to diminish its power of transmitting heat. The question is, Does this absorptive force stop all the radiant heat, except that having a definite degree of refrangibility, in the same way as it acts on the rays of light?

The character of colour, which always accompanies and indicates the refrangibility of light, is wanting in regard to heat; and the operation of measuring the intensity of an emergent pencil of the latter would be, it is easy to see, extremely difficult. It has formerly been shown that the power of heat to traverse diaphanous bodies diminishes with the refrangibility of the heat. From this we derive almost as easy a mean for estimating the refrangibility of a degree of heat as in the corresponding case of light. M. Melloni took a clear glass, and coloured glasses of various tints of the spectrum, and adapted each glass to the opening in a screen, through which rays fell from a lamp upon the sensible part of the thermo-multiplier. The distance of the lamp was varied till, with each glass successively, a deviation of 40° was produced in the index-needle of the galvanometer. In each case the rays transmitted by the glass were then caused to traverse a plate of sulphate of lime. The needle always indicated a smaller thermal effect than before, and its new position was the same for the white, violet, indigo, blue, yellow, orange, and red glasses; only in the *green*, instead of the angular deviation being reduced from 40° to 18°, as in the former cases, the needle stood at from 7° to 10°. When alum was substituted for sulphate of lime, the deviation for green glass was from 1° to 1°·6, whilst for all the others it was 8°. Hence we conclude that red, orange, yellow, blue, indigo, and violet, as well as clear glasses, produced no *elective action* on heat, since the rays issuing from all these traverse an interposed plate with equal facility, which would not be the case were they differently refrangible. Green glass, on the other hand, transmits rays easier stopped than any of the others; and therefore, from what has been already said, admits the passage of the least refrangible rays, whilst it stops the more refrangible, which we know to traverse the sulphate of lime or alum most easily. To confirm this deduction was the object of a second series of experiments, since the transmission of the first and the stoppage of the second ought to be made manifest.

The author took an Argand lamp, and a spiral of platinum wire placed over a lamp of alcohol, which, it is easy to show, had a much lower temperature, and therefore its heat had a lower degree of refrangibility than that of the Argand burner. The differences of the quantity of heat from these two sources, transmitted by all the kinds of glass except the green, showed that the transmitted rays, in the case of incandescent platinum, were almost exactly *half* those transmitted when the source was an Argand burner. In the case of green glasses, the difference was little or nothing. Hence the conclusion that, as was anticipated, the green glass was more transparent for the rays of small refrangibility than for those of greater refrangibility (or temperature).

Another experiment was performed with the same object, and with similar results.

Since the rays which have been transmitted by citric acid and some other substances are those only of greatest refrangibility, these have, relatively to most substances, a great penetrating power. In the remarkable case of green glass, where rays of a low degree of refrangibility are most easily transmitted, the reverse should be the case. Accordingly, out of 100 parts of heat transmitted by citric acid, white-, violet-, red-, orange-, yellow-, blue-, and indigo-coloured glass transmitted various quantities of heat, varying from 89 to 28 parts; whilst two specimens of green glass only transmitted 6 and 2 parts. What is most remarkable is, that while for all the glasses but green the transmissive power is three times greater than when the heat of incandescent platinum was directly incident upon them, the effect with that particular glass descended from 23 or 24 per cent. nearly to 0. The reason is obvious; for the whole of the rays of low refrangibility emitted by the platinum, and for which alone the green glass was transparent, had been stopped by the interposition of the plate of citric acid, which had, as it were, sifted it free from these rays.

Hence M. Melloni concludes that green glass is the only kind which has coloration for heat, if we may use the expression; the others acting upon it only as a more or less transparent glass of uniform tint does upon light.

On Thermo-Electricity. By JOHN PRIDEAUX.

In examining the theory of Thermo-electricity, Mr. Prideaux satisfied himself—

1. That thermo-electricity is conveyed through long wires with the same promptitude as common or voltaic electricity; and that it does not contain caloric as an element, differently from those principles:—

2. That bringing two heated wires into contact in the bulb of a large air thermometer, so as to generate a thermo-electric current there, does not accelerate the cooling of the wires, and thus that caloric is not consumed in its production; nor does a thermo-electric current become developed when caloric is rendered latent, as in the fusion of a metal:—

3. That making the surfaces of a thermo-electric pair identical, by tinning them all over before contact, does not impair their powers (but rather the reverse); and that large plates of copper and tin, or tin plate, heated together, separated from metallic contact only by fine gauze, or a few threads spread over the surface, will not produce the slightest deviation of the needle, although metallic communication being made between

them by means only of a slip of tinfoil, immediately throws off the needle 25°; which experiments, he conceives, can hardly be reconciled with M. Becquerel's views respecting radiation:—(*Ann. de Phys. et de Chim.*, Août 1829.)

4. That copper is, under *all* circumstances, a better conductor both of heat and electricity than iron; wherefore the electro-positive quality of iron to copper cannot be due to superiority of conduction; and, by analogy, conduction should not generally be the positive principle in this particular development of electricity:—

5. That iron, antimony, and zinc become positive by heating; that is, a bar of either of these metals hot is positive to a bar of the same metal cold; a quality which he denominates thermo-positive.

That platinum, silver, copper, lead, tin, bismuth, become negative by heating; that is, a hot bar is negative to a bar of the same metal cold, which he calls thermo-negative; and thus the metals are resolved into two thermo-electric classes.

That the thermo-positive quality is possessed by the above-named three metals in the following order:

Antimony, } nearly equal,
Iron, }
Zinc.

That the thermo-negative order is as follows:

Bismuth,
Silver,
Platinum,
Copper,
Tin,
Lead.

And that the order of thermo-electric reaction seems due to the idio-positive or negative quality, compounded with that of conduction, taking conduction directly in the thermo-negative class, inversely in the thermo-positive. That is to say, antimony and iron are not very different in thermo-positive power; but antimony is the worse conductor, and takes the lead.

Iron and zinc differ little in conducting power; but iron is the stronger thermo-electric, and takes the lead.

In the thermo-negative class bismuth is the worst conductor, and strongest thermo-negative, and therefore takes the lowest place.

Silver is a better conductor, but stronger thermo-negative, than copper. The one quality should raise it, the other lower it, in the table, and accordingly the difference between those two metals is small; and Mr. Prideaux frequently finds copper wires + to silver, though the latter generally stands highest in the tables. Platinum is a worse conductor, and a stronger thermo-negative, than copper: both properties cooperate to lower it in the table, and it falls considerably.

Platinum conducts a little better than tin, but is much more thermo-negative. The latter quality being predominant, tin stands above platinum in the thermo-electric table.

Lead conducts worse than either of those metals, but has much less thermo-negative quality; hence they both range below it in thermo-electric reaction.

Zinc has a peculiarity deserving the notice of experimenters on this subject:

Above 250° it is thermo-positive.
Below 200° it is thermo-negative.

And two bars—one heated to about 230°, the other at the atmospheric temperature (say 60°)—have no reaction on the needle; that is, there is between 200° and 250° a neutral point. The investigation of this thermo-electric change, at such manageable temperatures, seems to offer an opening to new and advanced ground. Zinc becomes ductile at about 250°.

6. That the currents in masses of bismuth cannot be easily drawn out by better conductors, and do not seem analogous to currents in the same or other metals acting in pairs:—

7. That foreign metals brought into contact with a homogeneous circuit, near a heated point, enter into the thermo-electric communication, and determine a current conformably to their position in the table.

On some new Phænomena of Electrical Attraction. By W. Snow Harris, F.R.S. *&c.*

The author exhibited and explained to the Section the construction and use of some new electrical instruments, by which he has been enabled to investigate more rigorously than has been hitherto done the phænomena of electrical attraction. These instruments consist of an *electroscope*, an *electrometer*, a *unit* of *measure*, and an adaptation of the common balance to the measurement of electrical force.

The electroscope acts on the principle of divergence, and consists of a long index of reed, easily moveable on a delicate axis set on points in an elliptical ring of brass. This index diverges over a graduated circle from two fixed arms of brass when a slight degree of electricity is communicated to the ring.

The electrometer measures the force of electrical attraction, under given conditions, by means of two opposed conductors, one of which is fixed, the other moveable, the latter being suspended from the periphery of a wheel. The distance between the conductors is either constant or variable, according to the conditions of the experiment, and the force is estimated on a graduated arc in terms of a known standard of weight or otherwise in degrees.

The unit of measure consists of a small electrical jar, having a discharging electrometer, and inverted on the prime conductor of the machine. The quantity accumulated is measured by its repeated explosions, in consequence of electricity passing from the outer coating.

When it is required to communicate comparative quantities of electricity to simple insulated conductors, sparks are drawn by means of an insulated transfer plate from an insulated jar charged by the preceding method with a known quantity.

By the aid of these instruments, the author has arrived at some new results in electricity, which appear to be not unworthy of notice.

In disposing different quantities of electricity on insulated conductors of similar form and dimensions, he finds that the attractive force is directly as the square of the quantity, the surface being constant; and inversely as the square of the surface, the quantity being constant; being similar to the results which he had already arrived at in examining the disposition of electricity on coated jars*.

On comparing those results with the striking distance of a given accumulation, he finds that the quantity of electricity requisite to produce a discharge is invariably as the square of the distance directly; that is to say, when the distance is twice as great, there is four times the quantity of electricity accumulated at the instant of the discharge, all other things remaining the same; and this is true, whether the accumulation take place on simple insulated conductors or on coated jars.

The above laws are general when the conductors or jars are in every respect similar, but they do not obtain under every possible kind of extension of the *same* surface. The author finds, with Professor Volta, that extension in length increases the capacity of a given conductor. In examining the capacities of plane surfaces for electricity, he finds the capacity to be as the linear dimensions of the plate inversely, the area being constant; and if the linear dimensions be the same, it varies in an inverse ratio of the area. The extent of edge or boundary of the plate, however, has not in itself any influence on the capa-

* *Memoirs of the Plymouth Institution.*

city of a conductor: the extent of edge is merely a function of the peculiar kind of extension to which the given area has been subjected, and by which the electrical particles become placed in respect of each other, in such way as to diminish their operation on external bodies, that is to say, their intensity.

This view seems confirmed by numerous experiments made by the author. The same plates turned into cylinders, both in the direction of their lengths and breadths, were found to have still the same capacities, that is to say, whether the conductor was simply a plate or an open cylinder. Neither did any differences arise in turning a given plate into other figures approaching cylinders, such as triangular and hexagonal prisms. The capacity of a sphere, also, is the same as a plane circular area of equal superficies. Hence the capacity of a cylinder or sphere for free electricity, may be conceived to be the same as that of the plane area into which it may be supposed to be rectified.

These results seem of some consequence to the theory of electrical action. It has been found, for example, that the interior surface of a sphere charged with free electricity does not electrify a conducting substance when placed completely within it; it has been hence inferred, that the electrical accumulation is disposed altogether upon its exterior surface. Now the intensity of a sphere being the same as that of a plane area of equal superficial extent, it should follow that the electrical distribution is in each case the same, since it is difficult from any known fact to suppose a given quantity of electricity expanded over twice the surface, as in the plane area, and yet maintain the same intensity. The redundant electricity, therefore, if the above deduction be true, should only be disposed on one side of the plate.

Since the capacity of plane conductors varies in an inverse ratio of their area and linear dimensions, it is requisite, if we desire to obtain plane surfaces whose capacities for electricity shall be double, treble, &c., of each other, to construct the plates so that their areas and linear dimensions shall be likewise double, treble, &c., of each other respectively.

The law according to which the force of electrical attraction varies at different distances is an important object of physical research. It may be easily and satisfactorily arrived at by the methods of experiment above mentioned. The author exhibited to the Section, by the means of his balance, the resulting law of electrical force when exerted between two circular plane areas directly opposed to each other, at different distances, and which was found to be in an inverse ratio of the squares of the respective distances exactly.

This law is immediately apparent when the opposed surfaces are parallel planes or rings; but in the case of spheres, or bodies of other forms, the experiment is of a somewhat more complicated character.

With a view of reducing the experiments to a more simple form, the author has been led to some further inquiries into the peculiar mode of action of the force under examination, which merit an attentive consideration. He finds,

1. The force exerted between an electrified and insulated neutral conductor is not at all influenced by the form and disposition of the *unopposed* portions. Thus the force is precisely the same, whether the opposed bodies are merely circular plane areas, or are otherwise backed by hemispheres or cones.

2. The force is as the attracting surface directly, and as the squares of the distances inversely. Hence the attractive force between parallel plane circles being found, the force between any other two similar planes will be given.

3. The attractive force between two unequal circular areas is no greater than that exerted between two similar areas each equal to the lesser.

4. The attractive force between a mere ring on a circular area is no greater than that between two similar rings.

5. The force between a sphere and an opposed spherical segment of the same curvature is no greater than that of two similar segments each equal to the given segment.

These results have been arrived at by the instruments already mentioned, the electrical intensity in each series of experiments being supposed the same.

A careful induction from the above facts has led the author to consider the attractive force between two equal spheres as made up of a system of parallel forces operating between the homologous points of the opposed hemispheres, the total force being as the number of attracting points directly, and as the squares of the respective distances inversely.

These simple elements enable us to determine a point q q' within each hemisphere, in which the whole force may be supposed to be concentrated, and to be the same as if emanating from every point of the hemisphere. The locus of this point within the surface will depend on the distance between the nearest points of the spheres, and may be readily found by the expression $z = \frac{(a^2 + 2\,a\,r^{\frac{1}{2}}) - a}{2}$, a being the distance between the nearest points of attraction, and r the radius of the sphere, and z the distance of the point within the hemisphere.

The points q q' being thus found, the whole force is observed to vary between them according to the general law already arrived at. It varies also as $\frac{s}{a \cdot (a + 2r)}$, that is, inversely as the distance between the nearest points multiplied into the distance between the centres.

The author found these deductions accord very completely with subsequent experiments, so that the weight requisite to counterpoise the force between two equal spheres at given distances may be invariably predicted with great precision. Some of these results were exhibited to the Section by means of the balance.

When two plane circular areas, each equal to the areas of the hemispheres rectified into plane surfaces, are opposed to each other, and placed so as to pass through the points $q\ q'$ above mentioned, the attractive force is extremely near that obtained by means of the opposed spheres, the distances being estimated from the same points.

The author believes that many of the results thus arrived at have not been hitherto included in the received theories of electricity, and is disposed to think that we may yet arrive at easier views of electrical action, and hence bring the phænomena more completely under the dominion of analysis.

On Electricity. By the Rev. JOHN G. MACVICAR, *of St. Andrew's.* (*Extract of a Letter from the Author to the Rev.* W. WHEWELL, F.R.S., &c.)

As a basis for his theory of electrical phænomena, Mr. MacVicar assumes that those parts of bodies which are capable of displaying electric phænomena possess a filamentary or acicular structure, an hypothesis which he is not disposed to rest upon such a consideration as M. Ampère makes it rest, when advancing his hypothesis of magnetic action, (namely, that by making such a supposition one may explain many phænomena,) but upon these two principles : *First,* The action of the first law of motion, which must ever tend to distribute the matter in which impulses are embodied in a filamentary or acicular arrangement, or at all events in an arrangement in which a linear continuity of matter is maintained, which (since every material medium is known to be very porous,) can only give rise to such a structure. *Secondly,* Upon the known intimate structure of bodies, so far as that can be observed by the microscope, during solidification, by cleavage or otherwise.

This granted, he inquires what are the phænomena of transverse vibration that must be displayed by such filamentary tissue, the length of each filament being regarded as indefinite in comparison with the smallness of the elementary impulses to which it is supposed to be subjected during the excitement of electric phænomena. And from the well known phænomena of extended chords, he infers that each must undulate in a serpentine manner, the point where the exciting impulse is applied being the centre of a vibrating portion, which may be called the primary; and this accompanied on both hands by ulterior vibrating portions, which may be called reciprocal or secondary, and whose phases of motion must ever tend to be exactly opposite to that of the primary or central part, and would of course succeed in being so could the filament consisting of the primary and two secondary vibrating portions be regarded as an elastic chord stretched between two fixed nuts at its extremities. But since the filament, as has been stated, is in the present case to be viewed as of indefinite length, and instead of being stopped by two nuts at two definite points, is to be viewed as equally prepared for being stopped at every point, and so is analogous to an extended chord possessing many moveable nuts which spontaneously shift their position so as to be always adapted to the impulse, he concludes that the central vibratory portion, and the two reciprocal or secondary vibratory portions, will not be in unison, but the latter shorter and consequently more rapid in their movements, and taken together equivalent to the central or primary one in the quantity of motion they embody. Such is the theory from which he thinks all the phænomena of electric action may be deduced, without calling in the aid of electric fluids.

Mr. MacVicar then proceeds to apply his theory to the explanation of electric polarity and induction, and points out the analogy between the production of light by undulations in the universal æther, and the excitement of electrical phænomena by undulations in denser media.

Inquiry into the Cause of Endosmose and Exosmose. By the Rev. J. Power.

The author investigates these phænomena upon La Place's principles of capillary attraction.

The two fluids are supposed to communicate by means of a capillary tube, which has a stronger attraction for one than the other. The attraction between the particles of the opposite fluids is supposed to be greater than the attraction of the particles of either fluid for particles of their own kind. Also the attraction between the particles of the tube and of the more strongly attracted fluid in contact with it, is supposed to be greater than the attraction of the latter particles for themselves. By reason of this last supposition the fluid will, according to La Place, form an internal coating of the capillary tube, which internal coating will itself act as a capillary tube upon the fluid next in contact. This new capillary tube, attracting the particles of the opposite fluid more strongly than its own, will in turn be coated by a thin stratum of that fluid, which may again be regarded as a fresh capillary tube; and this alternation may extend itself to the very axis of the original tube, the opposite fluids thus forming a series of interlacing cylinders one within another.

That cylinder of fluid which lies in immediate contact with the tube, must be supposed to become mixed with the opposite fluid, either before it issues from the tube, or at a distance beyond it less than the sphere of sensible attraction. Were this not the case, the tube acting equally at its two extremities would produce no effect; and the action of endosmose (so far at least as it depends on the tube,) would cease, allowing the ordinary mixing process time to extend itself within the tube; after which the motion would recommence.

The maximum force is obtained by supposing this mixture to be completed *within* the tube, at a distance from the issuing extremity not less than the sphere of sensible attraction; and this is the force which it is our object to compute, for it is plain that at the moment when the endosmose ceases from the increase of pressure within the vessel where the fluid accumulates, the ordinary mixing process will be at liberty to extend itself within the tube; and, in experiment, it is by means of these ultimate pressures that the forces are compared.

The principle of the computation is this:—the process being supposed to have gone on for some time, the effective attractions are found by taking the sums of the attractions due to the different substances regarded as coexisting (each with a diminished density,) within the same identical space. Expressions for the forces are thus obtained in terms of these partial densities. The fluids being supposed to suffer no penetration of dimensions in consequence of mixture, the partial densities, which are four in number, are readily expressed in terms of the two actual or total densities, and the constants depending on the initial state of the fluids; and thus we are able to express the forces in terms of the actual densities and the constants above mentioned.

The expression for the maximum force of endosmose is thus found to be composed of two terms; the first being proportional to the difference of densities, and the latter to the square of this difference.

The last term is the force of exosmose. It is proportional to the effort with which the fluid cylinders tend to penetrate each other, independently of the action of the tube, and seems to be a proper measure for the force with which the opposite fluids tend to unite in the ordinary case of mixture. It is positive when the attraction between the particles of the opposite fluids exceeds the arithmetic mean between the attractions of the particles of the two fluids for particles of their own kind; is negative when less than this mean, and of course vanishes in the case of equality.

When the force expressed by this last term is negative, the fluid cylinders, instead of penetrating further into the opposite fluid, will retreat into their respective fluids, and no mixture will take place. This is probably the reason why some fluids, such as oil and water, refuse to mix.

The substances with which Dutrochet made his interesting experiments were saccharine or gummy solutions on the one hand, and water on the other. These fluids have a sufficient tendency to mix for the purpose of carrying on the process; but that tendency being a very slow one (as any one may convince himself by dropping a little treacle or a solution of gum arabic into a glass of water), the last term is probably very small; in which case the force ought to be very nearly proportional to the difference of densities, which is the law observed by Dutrochet.

In support of that part of this theory which supposes the interlacing of the internal cylinders, a passage is quoted from the *Mécanique Céleste*, in which La Place contemplates such an arrangement*. But in whatever way the fluids be supposed to mix in the interior of the tube, the same force of endosmose will result, provided that the extremities of the tube are totally immersed, one in one fluid and the other in the other, to a distance within the tube not less than the sphere of sensible attraction.

On Electro-chemical Decomposition. By MICHAEL FARADAY, D.C.L. F.R.S. *Professor of Chemistry in the Royal Institution.*

In explanation of the present report of the subject brought forward by Mr. Faraday at this time, it may be needful to say that it forms part of his fifth series of Experimental Researches

* Supplem. au X^e Livre, pp. 28, 29.

in Electricity. In the first and second were developed the theory and facts of magneto-electricity; in the third, the identity of voltaic, common, and other electricities, was proved, so as to allow of the use of one for the other; in the fourth, a new law connected with the conduction and decomposition of bodies was developed; and in the fifth, by the application of the third and fourth, an attempt is made to ascertain the true nature of electro-chemical decomposition.

This important part of chemical or electrical science has been expressly treated of by different philosophers, and viewed in various lights. The opinions formed of it differ very much from each other, but agree generally in referring it to the attraction of the poles or metallic terminations of the pole, for the principles either proximate or ultimate present in the decomposing body.

Having proved to his own satisfaction that common electricity had the same power of effecting chemical decomposition as that of the voltaic battery, and even shown that when measured out, a quantity of it does the same chemical work as an equal quantity of voltaic electricity, Mr. Faraday resorted to it for the purpose of taking advantage of its high intensity in effecting decomposition under circumstances in which the voltaic battery would be quite inefficient, and so to accumulate facts and gain knowledge which might bear upon and explain the usual mode of action of the latter. He found that decomposition could be effected without the use of any poles, or at least without the use of those metallic terminations usually called poles; for when salts and iodides were submitted to the action of electricity, passing to and from them only through the air, still decomposition took place. For instance, a piece of paper being cut into a lozenge-shape, moistened with solution of sulphate of soda, and placed between two points, one connected with the electrical machine, and the other with a discharging train, formed of a very extensive metallic system, communicating with the ground, soon showed evidence of the evolution of free acid and alkali, as the electricity of the machine passed through it, though it was two inches distant from either of the points. The acid was evolved at that point where the positive electricity entered the moistened paper, and the alkali at that point where the electricity left the paper; *i. e.* considering the paper as a little conductor surrounded by air, the *acid* was evolved at the *negative* extremity, and the *alkali* at the *positive* extremity. Many other facts were referred to, in which, as in this, air poles were successfully used; but the word pole must in these researches be understood as having no other meaning than the substance or surface which limits the decomposing body in the direction of the electric current.

It then became an object to produce a water pole, and by much care this was effected. A strong solution of sulphate of magnesia was so adjusted as to have pure water floating upon it over one half of its surface; a platina plate was dipped horizontally into this water, and another placed vertically in the half of the solution of magnesia uncovered by water; so that the order of things were, platina, sulphate of magnesia solution, pure water, platina: the first platina was rendered positive, and the latter negative, by contact with a voltaic battery. Immediately there was decomposition both of the water and of the salt present; the water yielded its elements against the platina pole, but the salt could not do so. It is true the acid appeared against the positive pole, but the earth magnesia appeared in the plane where the solution and water were in contact; in fact, upon the surface of water next the decomposing solution, and not upon the negative platina pole. Here, therefore, a true water pole was obtained, *i. e.* a surface of water upon or against which the earth magnesia evolved in a case of electro-chemical decomposition was deposited.

These were considered as very important facts in determining the true character of the action taking place in cases of electro-chemical decomposition generally.

Another correction of commonly received opinions was considered as essential in assisting to determine the nature of electro-chemical action. It has been generally stated by Sir Humphry Davy and others, that water is essential to decomposition by the voltaic battery; but Mr. Faraday has shown that such is not the case, and that an immense number of other bodies have, not merely equal powers, but powers far surpassing those of water in that respect. The cause of this mistake has been our ignorance of a law developed in the *fourth* series of the *Experimental Researches,* under which an extensive class of compound bodies, such as oxides, chlorides, iodides, salts, &c., which are either very bad or nearly non-conductors when solid, become most excellent conductors when melted, and then are decomposed with extreme facility. Water happens to be the only one of this class of bodies existing naturally in the fluid and therefore decomposable state: it loses this power when it becomes a solid, and other bodies gain it when they become fluid. Electro-chemical decomposition does not therefore, of necessity, involve the presence of water.

For the purpose of making his own view of matters clear and distinct, Mr. Faraday gave a brief view of the theories of electro-chemical decomposition advanced by others, as far as they could be gathered from their published papers.

Grotthuss published on the subject in the year 1805. He considers the pole as an *attractive* agent; the poles have *attractive* and *repelling* powers: the negative pole attracts hydrogen and repels oxygen, whilst the positive pole attracts oxygen and repels hydrogen. The powers of each pole upon any one particle are inversely as the squares of the distances; hence giving, as he says, (though erroneously,) for any one particle, a *constant* force. He first put forth the view of successive decompositions and recompositions, and so explains the appearance of the elements at a distance from each other. He thinks the elements about to separate at the poles, unite to the two electricities there, and so become gases*.

Sir Humphry Davy wrote on the subject in 1806. His expressions are very general, so as to accord with many specific views of decomposition; but when more particular, he refers the decomposing effects to the attraction of the poles†. He agrees with Grotthuss in supposing successive compositions and decompositions throughout the fluid; supposes that the attractive and repellent agencies may be communicated from the metallic surfaces throughout the whole of the menstruum, being communicated from one particle to another particle of the same kind, and diminishing in strength from the place of the poles to the middle part, which is necessarily neutral. In 1826, Sir Humphry Davy said he had nothing to alter in the fundamental theory just referred to, and uses the terms attraction and repulsion in the same sense as before.

MM. Riffault and Chompré in 1807 came to the conclusion that not merely decompositions, but actual separations of the elements took place throughout the whole of the humid conductors. They consider the negative current as collecting and carrying the acids, &c., to the positive pole, and the positive current as doing the same thing with bases, leaving them at the negative pole.

M. Biot speaks very philosophically and cautiously, but refers the effects to the attraction and repulsion exerted by the poles and the parts in their vicinity. He does not appear to admit the successive recompositions and decompositions of Grotthuss and Davy, but seems to consider the particles under transfer as attached for the time to the electricity, and carried on by it.

* *Annales de Chimie*, 1806, tom. lviii. p. 64.

† *Philosophical Transactions* 1807, pp. 28, 29, 30; *Elements of Chemical Philosophy*, p. 160.

Decomposition is supposed to take place at both poles, and not at all in the intermediate parts, which merely act as imperfect conductors*.

M. A. De la Rive experimented and wrote on this subject in 1825. He says, those who refer the effect to the attraction of the poles only express the fact, and do not explain it. He thinks the results are due to a combination of the elements with the electricities at the poles by virtue of a species of play of affinities between the substances and the electricities. The current at the positive pole unites with the hydrogen, combustibles, metals and bases it may find there, leaving the oxygen and the acids at liberty to escape: it passes across to the negative pole, on the surface of which it leaves the ponderable bodies, entering itself into the metal, and passing away†. The electricity at the negative pole does the same thing with oxygen, chlorine, acids, &c., and carrying them across to the positive pole, there leaves them. Thus half the elements on each side are ejected by a species of superior affinity, and the other half are brought from the opposite pole in combination with the electricity of that pole. Successive decompositions and recompositions are not admitted by M. De la Rive. Decomposition takes place at both poles, and only at the poles, and the intervening parts of the humid conductor are themselves unaltered.

Mr. Faraday contended that the experiments he had described opposed generally all those views which referred electrochemical decomposition to the attractions and repulsions of the poles. Thus, when the elements were evolved against air, the poles as attractive agents were absent; or if it be theoretically said that the air then acted as poles, and attracted the element, which, however, could not pass across it to the superior metallic poles on the outside, then he quoted the water pole, where the element might have gone to the metal, according to the theory, but was found not to do so; and as the evolved elements can be made to appear against air, which is not a conductor, nor is decomposed; or against water, which is a conductor, and can be decomposed; as well as against metals, which are excellent conductors, but undecomposable, there appears but little reason to consider the phænomena generally as due to the attraction or attractive powers of the latter, when used in the ordinary way, since similar attractions can hardly be imagined in the former instances.

Mr. Faraday's view is, that the effect is produced by an *internal corpuscular action*, exerted according to the direction of

* *Précis Elémentaire de Physique*, 1824, tom. i. pp. 637, 641, 642, &c.

† *Annales de Chimie*, tom. xxviii. pp. 190, 200, 202.

the electrical current, and that it is due to a force either *superadded to*, or giving direction to, the ordinary chemical affinity of the bodies present. The decomposing body may be considered as a mass of acting particles, all those which are included in the course of the electrical current contributing to the final effect: and it is because the ordinary chemical affinity is relieved, weakened, or partly neutralized by the influence of the electric current in one direction parallel to the course of the latter, and strengthened or added to in the opposite direction, that the combining particles have a tendency to pass in opposite courses.

In this view the effect is considered as *essentially dependent* upon the mutual chemical affinity of the particles of opposite kinds. A particle could not travel from the negative towards the positive pole unless it found other particles in its course inclined to travel from the positive to the negative pole. As far as regards two compound particles, the case may be considered as analogous to one of ordinary chemical decomposition; but as all the compound particles in the course of the electric current, except those actually in contact with the poles, act conjointly, the case becomes more complicated, though not more difficult of comprehension.

Many facts were then quoted in illustration of the manner in which particles acting under the force of their ordinary chemical affinities, governed in a greater or smaller degree for the time by the passing current, according to its force or quantity, would render themselves, or be excluded at the opposite extremities of the decomposable mass in the direction of the currents. Dilute sulphuric acid of a certain strength was found, when submitted to the action of the pile, to yield after a time a stronger acid at the positive than at the negative side; *i. e.* a certain quantity of acid had been transferred from the negative towards the positive pole: but when the acid was previously neutralized by an alkali, then a far greater quantity of acid was transferred by the *same current of electricity*. These and many other experimental results will appear in the fifth series of Experimental Researches, to be published in the *Philosophical Transactions*.

All the phænomena of transfer of acids through alkalies, &c. &c., which appeared so extraordinary to Davy and others when they were discovered, are the necessary consequence of the theory of Mr. Faraday; and, in fact, such transferences are by him considered as essential to electro-chemical decomposition.

The manner in which the evolved elements are set free at the poles, inexplicable upon the old theories, are necessary consequences of his theory; or if they combine, still the effects are natural results due to the influence of the chemical affinity, modified by the current. The refusal of elements or substances present to collect at the poles unless they are in relation by solution or chemical affinity with the substances present, also finds its natural reason in the theory.

The fact that an element will sometimes go to one pole and sometimes to the other, according to the substances with which it is in association at the time, is an immediate result of the theory. Nitrogen is said to do this: it will go freely to the positive pole, and doubtfully to the negative pole. Water will go either to the positive or negative pole, and sulphur will also do so: from oxygen it will go to the negative pole, from silver to the positive pole.

Want of time and apparatus prevented a further development of this view and its consequences, but it will appear in detail in the next Part of the *Philosophical Transactions.*

Experiments on Atomic Weights. By Dr. Turner.

Dr. Turner reported to the Meeting that he had continued his researches into atomic weights, and had to his own conviction determined the points which had induced him to undertake the inquiry. These were, first, to form an opinion of the relative accuracy of the tables of equivalents employed in this country and on the continent; and, secondly, to ascertain whether there existed any trustworthy evidence in proof of the hypothesis that the equivalents of bodies are multiples by whole numbers of the equivalent of hydrogen. To examine these questions he endeavoured to ascertain by careful and often-repeated experiments the equivalents of silver, chlorine, lead, barium, mercury, and nitrogen, in relation to oxygen. These were selected in consequence of their frequent use in analysis. An error in these could not exist without affecting the equivalents of nearly all the other elementary substances. The researches on this subject had been lately read before the Royal Society, and would probably, ere long, be published in some form or other. The general result is, that the atomic weights current in this country are much less exact than those given by Berzelius; that though they had been recommended to British chemists as rigidly correct, they were often very inexact, and had been determined by methods which in some important cases were defective. Further, he finds that as far as experimental evidence at present goes, the hypothesis above alluded to is unsupported. In some instances the equivalents are so nearly simple multiples of that of hydrogen that they may be taken as such without appreciable error; but in many other cases the numbers given by experiment cannot be reconciled with the hypothesis. The following are the numbers which he is disposed to believe very nearly correct:—lead, 103·6; silver, 108; chlorine, 35·42; barium, 68·7; mercury, 202, perhaps slightly higher, but not higher than 202·3; nitrogen, 14·2. Dr. Turner states that his methods for ascertaining nitrogen were not so advisable as that in which Dr. Prout is occupied by weighing the gases. This weight should be kept in abeyance for the present. He conceives that it does not fall below 14, nor exceed 14·2. During these researches he incidentally obtained some facts for inferring the equivalent of silver; and from these it appears that the equivalent of sulphur is nearer 16·1 than 16. He would not venture, however, to make a positive statement without further inquiry.

He then mentioned that Dr. Prout had kindly informed him of a fact which he conceived analytical chemists in general to be ignorant of, and which he thought might have had an influence on these researches. The fact is, that chloride of silver, however white and well washed, gives out a little muriatic acid at the moment of fusing. This fact Dr. Turner has examined, and can confirm. It especially ensues when fusion takes place before the chloride has been well dried; but in the event of the chloride of silver being first well dried at 300° (when no acid is given out), and then, without exposure to the atmosphere while cold, fused, the loss of acid is not appreciable in weight, though it is still sufficient to redden delicate litmus paper. In two experiments about fifty grains of chloride of silver were fused, (previously dried at 300°, introduced while hot into a dry bottle furnished with a tight cork, and weighed in that state,) and the loss was inappreciable. From this circumstance, taken in conjunction with the mode in which he habitually weighs the chloride of silver, he is satisfied that the fact observed by Dr. Prout does not necessarily produce any error in the determination of chlorine by means of silver.

Notice of a Method of analysing Carbonaceous Iron. By Professor JOHNSTON.

Professor Johnston gave an account of a new mode of determining the amount of charcoal in the carbonaceous irons, by which he hopes to obtain results more precise and trustworthy than those arrived at by any former mode. This method consists essentially in reducing the iron to fine powder in a steel mortar, and burning it with oxide of copper. Mr. Johnston expects to be able to lay a series of results before the next Meeting of the Association.

Communication respecting an Arch of the Aurora Borealis. By R. POTTER, Jun.*

A very luminous arch of an aurora borealis was observed at Edinburgh by Professor Forbes on the evening of the 21st of March 1833. It was observed at Athboy in Ireland by the Earl of Darnley, by Dr. Robinson at Armagh, and also by a correspondent of one of the Carlisle newspapers.

The observations demonstrate the view of the symmetrical arches being similar to parallels of latitude round the magnetic axis, the arch being seen in those positions at Edinburgh, Armagh, and Athboy which such a direction requires.

Report of Experiments on the Quantities of Rain falling at different Elevations above the Surface of the Ground at York, undertaken at the request of the Association, by WILLIAM GRAY, Jun., *and* JOHN PHILLIPS, F.R.S. G.S., *Secretaries of the Yorkshire Philosophical Society; with Remarks on the Results of these Experiments, by* JOHN PHILLIPS, F.R.S. G.S.

I. *Report of the Experiments.*—York, the site of these experiments, stands in the centre of perhaps the most uniform and extensive vale in England, reaching from the mouth of the Tees to the æstuary of the Humber, a length of 70 miles, with a breadth of from 15 to 25 miles. In this vast space no ground rises more than 100 or 150 feet above the level of York; and the Minster, elevated 200 feet from the ground, looks down upon an area of above 1000 square miles, in which hardly any object, whether of nature or art, rises to within 100 feet of its summit.

On the east the vale is bordered by the range of the Wolds, whose extreme height is 805 feet, and the escarpments of the oolitic system, which swell to 1485 feet. On the west, the distant hilly regions of the coal and limestone tract appear above the low plateau of magnesian limestone.

These circumstances of situation give an importance to the moderate height of York Minster which is denied to many loftier buildings in England. From its summit the course of a passing storm may be well traced from even the distant hills of Richmond; and the deflections occasioned by the attraction of the sides of the vale, the rushing of the air, the sudden fall of temperature, and many other curious phænomena accompanying the precipitation of rain, may be well observed.

It is, probably, to the peculiarity of its geographical situation that we are to attribute the remarkable general regularity of the curves of mean temperature at York; for the deviation of the

* See *Lond. and Edinb. Philosophical Magazine,* Third Series, vol. iii. p. 422.

daily mean temperature at this place from the annual mean is pretty exactly proportional to the sines of the sun's declination 25 *days before* the day of observation. The mean temperature of the year is 48°·2; of July 62°, of January 34°·5. Average quantity of rain 24 inches. Prevalent winds W. and S.W.: about the vernal equinox N.E. winds are frequent.

The Yorkshire Museum lies nearly west of the Minster, entirely beyond the city, which here encircles the Minster by a narrow belt of houses. Its roof is the highest in the immediate vicinity: it stands free from other buildings, and is surrounded on every side by the grounds of the institution.

In these grounds, south-west of the Museum, the third station is taken, in the midst of a large grass-plat. The second and third stations are nearly equidistant from the Minster: the intervening distances are,

	Feet.
Between the Minster and the Museum gauges .	1100
the Museum and garden gauges . . .	136

Elevation of the gauges above the river, which is nearly level with high water in the Humber:

	Feet.	In.
Minster top gauge, raised on 9 ft. pole . . .	241	$10\frac{1}{2}$
Museum top gauge	72	8
Gauge in grounds	29	0

From these data it will appear that it would be difficult to select three points more remarkably embracing the desired conditions of gradation of altitude, openness of sky, and contiguity of position.

The gauges employed are of the simplest construction. A cubical box of strong tin, exactly 10 inches by the side, open above, receives, at an inch below its edge, a funnel sloping to a small hole in the centre. On one of the lateral edges of the box, close to the top of the cavity, is soldered a short pipe, in which a cork is fitted. The whole is well painted. This is the gauge. The water which enters it is poured through the short tube into a cylindrical glass vessel graduated to cubic inches and fifths of cubic inches. Hence one inch depth of rain in the gauge will be measured by 100 inches of the graduated vessel, and $\frac{1}{1000}$th of an inch of rain may be very easily read off. All the gauges were made on the same mould, and the same glass jar has been used in every observation.

The gauge in the ground has its edge nearly level with the grass; that on the Museum projects 11 inches above the stonework; and the Minster gauge is supported on a pole nine feet above the level of the battlements of the great tower, whose top is 70 feet square.

Table of Results for Twelve Months.

1832, 1833.	Minster.	Museum.	Ground.	Remarks.
	Inches.	Inches.	Inches.	
Feb. 4 to Feb. 13.	·060	·119	·147	
20.	·010	·010	·008	
27.	·017	·020	·018	Abundance of Aphodii in the Minster gauge.
March . . 5.	·174	·251	·366	
12.	·198	·273	·386	
19.	·052	·062	·093	
26.	·041	·116	·238	Violent gales.
April . . . 2.	·005	·006	·004	
9.	·701	·756	·855	Perpendicular rain, without a trace of wind, in large drops.
16.	·013	·015	·017	
23.	·249	·353	·444	
30.	1·113	1·574	1·887	
May . . . 7.	·375	·442	·530	
14.	·133	·203	·257	
21.	·088	·141	·167	
28.	·002	·010	·012	
June . . . 5.	·557	·719	·792	
12.	·953	1·138	1·161	Thunder storm.
July . . . 2.	·908	1·166	1·291	
9.	·351	·397	·438	
16.	·999	1·115	1·230	
23.	·113	·133	·150	
August . . 6.	·711	·785	·825	
13.	·033	·050	·062	Abundance of small Hymenoptera in the Minster gauge, but not in the others.
27.	1·639	1·911	2·172	Ditto - ditto.
September . 3.	1·388	1·747	2·036	(This occurrence has
17.	·376	·500	·583	been frequently noticed
October . . 8.	·439	·605	·753	in the summer and au-
November . 12.	1·459	2·080	2·280	tumn.)
30.	1·019	1·308	1·459	
December . 17.	·703	1·012	1·399	
January . . 14.	·836	1·165	1·725	
February . . 1.	Snow ·195	Snow ·279	Snow ·616 drift-ed.	Drifted snow on the top of the ground gauge.
Total of 12 months	15·910	20·461	24·401	
Ditto, exclusive of snow in Feb. 1833	15·715	20·182	23·785	

II. *Remarks on the Results of the Experiments.*—The preceding table of results appears sufficient, when combined with some other data which I have obtained, to authorize some interesting inductions concerning the law and the cause of the remarkable inequality of the quantities of rain at different elevations above the ground.

1. The notion which is most generally entertained of the cause of this inequality is, that wind, blowing horizontally, causes fewer drops of rain to fall upon the more elevated gauges. That this notion is a mere fallacy, the least acquaintance with mechanics is sufficient to prove; for certainly the number of drops of rain which fall, under the joint influence of gravitation and ordinary wind, upon horizontal surfaces, will be, *cæteris paribus,* exactly the same at all elevations below the point from which the rain descends.

2. It is supposed by some that eddy winds, produced by the sides of buildings and rising upwards, may deflect the rain so as to prevent much of it from falling on those buildings. It is certainly conceivable that this irregular action *against gravity* may, when very violent, under particular circumstances, produce a sensible effect, and such appears to be recognised by our experiments, in one instance, during the equinoctial period of March 1832.

But it is evident that in the majority of cases the effect of the eddying wind is quite unimportant. I have noticed in several instances the fact, that the wind which accompanies the fall of rain takes the line of the rain-drops themselves; and on the Minster, in particular, this was very strikingly illustrated, when, with my friends Mr. Jonathan Gray and Mr. William Gray, junior, I watched the progress of a storm for 30 miles down the vale of York. The wind was insensible except during the fall of rain, and then it came downward *with* the drops. There is no need of further remarks on this subject, because the results recorded are too regular, considerable, and consistent with known properties of the atmosphere, to be explained by such fluctuating and inadequate agency.

3. With respect to the observations on the ground, I have procured several registers of the rain which fell in and about York, for comparison with the observations in the Museum garden. By this comparison it is abundantly evident that the situation of the gauge, its exposure to eddy winds, and other irregularities, have very little influence upon the mean results. While the gauge in the Museum garden is remarkably open on all sides, and set *level* with the ground, Mr. J. Gray's is raised three feet above, and placed in a small garden, surrounded by

high walls and buildings. Yet in the Museum garden we have for 12 months 23·785 inches of rain, and Mr. J. Gray's results for the same period are 23·020. (The snow which fell in this period is excluded from both these numerical statements.) The depth of rain appears pretty nearly uniform over the broad vale of York, and even beyond it. Thus at Ackworth, 25 miles S.W., the quantity collected in 1832 . . . . . =24·94
At Brandsby, 12 miles N. (station nearly level with the top of York Minster) . . . . 25·65
At York . . . . 23·78*

4. I shall now proceed to arrange the numerical results of the experiments, in relation to mean temperature and the season of the year, and thence to *infer* the *ratios* of quantity at the several stations. The quantity of snow which fell is always deducted, because it was found to drift into the lower gauge. This quantity was, however, very small and only sensible in February 1833.

Periods.	Mean Temp.	On Minster. In. of Rain.	On Museum. In. of Rain.	On Ground. In. of Rain.	Ratios.		
Whole year	48·20	15·715	20·182	23·785	66·1	85·3	100
7 coldest months, Oct., Nov., Dec., Jan., Feb., Mar., April	40·8	7·089	9·725	12·079	58·6	80·5	100
7 warmest months, April, May, June, July, Aug., Sept., October	55·5	11·146	13·669	15·666	71·2	87·1	100
5 coldest months, Nov., Dec., Jan., Feb., March . .	39·3	4·569	6·414	8·119	56·2	79	100
5 warmest months, May, June, July, Aug., September	58·5	8·626	10·457	11·706	73·7	89·2	100
Winter quarter, Dec., Jan., Feb.	36·3	1·626	2·326	3·297	49·3	70·5	100
Spring quarter, Mar., April, May	47·6	3·144	4·202	5·256	59·8	80	100
Summer quarter, June, July, Aug.	60·8	6·264	7·414	8·121	77·1	92·5	100
Autumn quarter, Sept., Oct., Nov.	48·3	4·681	6·240	7·111	65·8	87·7	100

* In 1833, The Museum gauge gave . . . . 22·959 inches.
Mr. J. Gray's . . . . 23·060
Dr. Wasse, at Moat Hall, (10 miles from York) . . . . 23·895

5. The first remark which I shall make on these results is, that the *diminution of the quantity of* rain received at different heights above the ground, as compared with that received on the ground, is very accurately represented by a simple formula involving one constant, viz. the square root of the height of the station above the ground, and one variable coefficient.

Thus, $m \sqrt{h}$ = the diminution of rain at the given height. In these experiments

$$\sqrt{h} \text{ for the Minster gauge } = \sqrt{212{\cdot}833} = 14{\cdot}5885$$
$$\text{for the Museum gauge } = \sqrt{43{\cdot}666} = 6{\cdot}6080$$

Taking $m = 2{\cdot}29$, we have for the whole year,

Ratios	By calculation . . .	66·5	84·9
	By observation . .	66·1	85·3
For the 7 coldest months ($m = 2{\cdot}88$,)			
	By calculation . . .	58	81
	By observation . .	58·6	80·5
For the 7 warmest months ($m = 1{\cdot}97$,)			
	By calculation . . .	71·3	87·0
	By observation . .	71·2	87·1
For the 5 coldest months ($m = 3{\cdot}06$,)			
	By calculation . . .	55·4	79·8
	By observation . .	56·2	79
For the 5 warmest months ($m = 1{\cdot}75$,)			
	By calculation . . .	74·5	88·4
	By observation . .	73·7	89·2

to 100.

In these, which are the longest averages attainable from the experiments, there is an almost exact agreement between the calculated and the observed results, the greatest error being $\frac{1}{70}$th.

In shorter averages of three months, and, indeed, though less exactly, in every single month when much rain fell, we may recognise the same constant relation. Thus we have

For the summer quarter ($m = 1{\cdot}43$,)			
	By calculation . . .	79·0	90·5
	By observation . .	77·1	92·5
For the winter quarter ($m = 3{\cdot}79$,)			
	By calculation . . .	44·6	74·7
	By observation . .	49·3	70·5
For the spring quarter ($m = 2{\cdot}84$,)			
	By calculation . . .	58·6	81·1
	By observation . .	59·8	80·0
For the autumn quarter ($m = 2{\cdot}19$,)			
	By calculation . . .	68·1	85·4
	By observation . .	65·8	87·7

to 100.

The three most rainy months of the year 1832 were June, August, and November.

For June we have,		
By calculation . . .	77·6	90·1
By observation . .	74·5	93·2
For August,		
By calculation . . .	77·6	90·1
By observation . .	77·9	89·8
For November,		
By calculation . . .	70·2	86·8
By observation . .	66·3	90·7

6. From these comparisons it appears to follow, that though the exact relation between the diminution of rain and the height of the station can hardly be considered as satisfactorily determined by the experiments of twelve months, the nature of this relation is so far ascertained that we may conclude it to be constant for all periods of the year, and that the form $\sqrt{h}$ is a good first approximation.

7. The account of Dr. Heberden's experiments on Westminster Abbey does not state the elevation of the stations; yet if we take the height of the square part of the roof at about 120 feet, and from this infer, according to the formula, the height of the house-top which was the middle station above the point below the house-top which was the lowest station, we shall still be able to use those experiments as a check upon the law of the ratio now given. In this case, $\sqrt{h} = 11{\cdot}0$ and 4·6, and we have

For the whole year,			
By calculation ($m = 4{\cdot}23$) . .	53·5	80·5	
By observation	53·5	80·5	
For the 7 coldest months,			
By calculation ($m = 4{\cdot}26$) .	53·1	80·4	
By observation	53	80·5	
For the 7 warmest months,			
By calculation ($m = 3{\cdot}90$) .	57·1	81·9	to 100.
By observation	57	82	
For the 5 coldest months,			
By calculation ($m = 4{\cdot}70$) .	48·3	78·3	
By observation	48·6	78·0	
For the 5 warmest months,			
By calculation ($m = 4{\cdot}19$)	53·9	80·6	
By observation	54·5	80	

For the winter quarter,			
By calculation ($m = 4\cdot42$) .	51·4	79·6	to 100.
By observation	51·5	79·5	
For the summer quarter,			
By calculation ($m = 4\cdot14$) .	55·5	80·8	
By observation	52·7	82·6	
For the spring quarter,			
By calculation ($m = 4\cdot92$) .	45·9	77·2	
*By observation	48	75·1	
For the autumn quarter,			
By calculation ($m = 3\cdot71$) .	59·2	82·9	
By observation	57·8	84·3	

It is therefore probable that the Westminster results obey the same *constant relation to height* as those of York.

8. But it is evident that the values of the *variable* coefficient are very different; that its maxima and minima are perhaps not quite in the same periods of the year as at York; and that the range of variation in its value is very much less. From M. Arago's determination of the relative quantities of rain falling on the Observatory at Paris and in the court 28 metres below, as given by Professor Forbes, in his Report on Meteorology, 50·47 : 56·37, the relative mean value of m at Paris = 1·24, while at Westminster it was 4·23, at York 2·29. It must be owned that these discrepancies with other observations as respects the *quantity* of the diminution of rain upwards are somewhat discouraging, and probably will, for a considerable time, deprive the most exact local determinations of this quantity of a general application. This, indeed, could hardly be expected, since the *whole quantity* of rain is so variously dependent on circumstances of physical geography, that centuries have been found insufficient to determine the general law and ascertain the numerical constants of local climate. Yet, on account of the remarkable regularity of the progress of monthly temperature at York, and some very obvious relations between the quantities of rain collected and the mean temperature of the period, I will venture to state what seem unavoidably to suggest themselves as probable inferences.

9. First, it is obvious that the diminution at the upper stations is greatest in the cold season, and least in the warm season, and therefore the coefficient is in some way *inversely* dependent on the temperature, or on some effect of this tem-

* March very anomalous.

perature. If we consider it in relation to the mean temperature, we shall find a near coincidence between the following formula, and observation: $m = \dfrac{a\,\dfrac{t}{t'} + a\,\dfrac{t^2}{t'^2}}{2}$, where a = the ascertained value of m, for the whole year, t the mean temperature of the year, and t' that of the particular period under consideration.

	t'.	Value of m by calc.	Value of m by obs.	Difference.
For the 7 colder months ..	40·8	= 2·98	= 2·88	0·10
7 warmer months. .	55·5	1·87	1·97	0·10
5 colder months ..	39·3	3·16	3·06	0·10
5 warmer months. .	58·5	1·74	1·74	0·00
Winter quarter. . . .	36·3	3·57	3·79	0·22
Spring quarter. . . .	47·6	2·35	2·84	0·49
Summer quarter ..	60·8	1·64	1·43	0·21
Autumn quarter ..	48·3	2·30	2·19	0·11

10. Secondly, it is obvious that the relation between the values of m and the *dryness* of the air is inverse. This dryness is usually expressed by the difference between the mean temperature and the mean dew point, and where the latter is perfectly determined, no better plan perhaps can be suggested. But this is the case for very few places in Great Britain. There is, however, another mode of expressing the dryness of the air, which is fortunately applicable to the present purpose; the *mean range* of daily temperature, or mean difference of maxima and minima, is a good approximation to an accurate expression of the relative dryness of the air. The following table of the mean ranges of temperature near York has been determined with the greatest nicety, by long averages, from the careful and continued observations of Francis Cholmeley, Esq., of Brandsby.

January mean range. . . .	8·0	July mean range	19·6
February	10·1	August	17·7
March	13·1	September	16·0
April.	16·2	October	11·8
May	19·7	November.	9·0
June.	20·1	December.	7·7

General mean range . . . . 14·08

On comparing these numbers with Mr. Daniell's estimates of the dryness of the air in London, they will be found analogous in general proportions. They may also be compared with an excellent series of dew point observations in the *Manchester Memoirs* by my friend Mr. John Blackwall, whose results in other respects I have found remarkably in accordance with my own inferences concerning the climate of York.

Now, let m be taken inversely as the mean range of temperature, (r) or $m = a \dfrac{14{\cdot}08,}{r}$ we shall have the following comparison between the calculated and observed values. ($a = 2{\cdot}29$.)

	Value of m calc.	Value of m obs.	Difference.
For the 7 coldest months......	= 2·98	= 2·88	0·10
7 warmest months	1·86	1·97	0·11
5 coldest months......	3·36	3·06	0·30
5 warmest months	1·73	1·73	0·00
Winter quarter	3·74	3·79	0·05
Spring quarter........	2·48	2·84	0·36
Summer quarter	1·68	1·43	0·25
Autumn quarter	2·63	2·19	0·44

So remarkable and continued an accordance between the coefficients fixed by observation and those derived by two methods from a very simple view of the condition of the air as to heat and moisture, appears to me decisive of the question as to the general cause of the *variation of the quantity of diminution of rain at any one height* above the ground. It has already been shown how strictly the observations warrant the conclusion that the *ratio of diminution at different heights is constant* through the whole year. It is therefore rather as a matter of very probable inference than a plausible speculation that I offer the hypothesis, that the whole difference in the quantity of rain, at different heights above the surface of the neighbouring ground, is caused by the continual augmentation of each drop of rain from the commencement to the end of its descent, as it traverses successively the humid strata of air at a temperature so much lower than that of the surrounding medium as to cause the deposition of moisture upon its surface. This hypothesis takes account of the length of descent, because in passing through more air more moisture would be gathered; it agrees with the fact that the augmentation for given lengths of descent is greatest in the most humid seasons of the year; it accounts to us for the greater absolute size of rain-drops in the hottest months and near the ground, as compared with those in the winter and on mountains; finally, it is almost an inevitable consequence from what is known of the gradation of temperature in the atmosphere, that some effect of this kind must necessarily take place. The very common observation of the cooling of the air at the instant of the fall of rain, the fact of small hail or snow whitening the mountains, while the very same precipitations fall as cold rain in the valleys where the dew point may be many degrees above freezing, is enough to prove this. A converse proof of the dependence of the quantity of rain at different heights on the state of the air at those heights, is found in the rarer occurrence of a shower falling from a cloud, but dissolving into the air without reaching the ground. Lastly, I cannot forbear remarking, that this hypothesis of augmentation of size of the elementary drops agrees with the result that the increase of quantity of rain for equal lengths of descent is greatest near the ground; for whether the augmentation of each drop be in proportion to its surface or its bulk, the consequence must be an *increasing rate* of augmentation of its quantity as it approaches the ground.

The direct mathematical solution of this problem, now that the laws of cooling and of the distribution of temperature have undergone such repeated scrutiny, may perhaps be attempted with success; but for the purpose of eliminating the effects of periodical or local modifying causes, it is desirable that observations on the same plan should be instituted at many and distant places,—both along the coasts and in the interior,—in the humid atmosphere of Cornwall and in the drier air of the midland counties. Always, at least three stations should be chosen, as open as possible, one of them very near to the ground: their relative heights, the mean temperatures, the mean ranges of temperature, and the mean dew point for each month should be ascertained. It would be useful to measure the size of the rain-drops, and, if possible, their own temperature. The height of clouds, according to the plan of Mr. Dalton, in his *Meteorological Essays*, and the direction and force of wind should be noted, and distinctions made between snow, hail, and rain. Some of these data I have not yet found the means of procuring, partly in consequence of the great labour and time required, and partly from the difficulty of well arranging the experiments themselves. But since it is now ascertained that the general results follow some settled laws, and that the effects may be very well appreciated at moderate heights, I hope not only to procure these, but also several other data towards the completion of the theory of this curious subject, the patient investigation of which cannot fail to give us new and penetrating views into the constitution of the atmosphere.

II. PHILOSOPHICAL INSTRUMENTS AND MECHANICAL ARTS.

On a peculiar Source of Error in Experiments with the Dipping Needle. By the Rev. WILLIAM SCORESBY, F.R.S.

Certain discrepancies, at the time apparently unaccountable, in observations made with a beautiful dipping-needle, by Dollond, entrusted to Mr. Scoresby's care by the Board of Longitude, in an arctic voyage, led him, after a considerable interval of time, to reflect upon the cause. Whatever might be the apparent consistency of any particular series of observations in the ordinary use of the instrument, the differences perceived when the poles of the needle were changed indicated that the preceding results were *not accurate.* But as the different results thus obtained were capable of being combined for obtaining the true position of the needle, the formula of Professor J. Tobias Meyer, given in his treatise *De Usu accuratiori Acûs Inclinatoriæ Magneticæ,* was adopted for this purpose. To verify the position thus obtained, another series of observations on the same spot was then made, with one of the arms of the needle *weighted,* so as to render its position more decisive from being the resultant of two forces, gravity and magnetic attraction. The results, however, of the different series were again anomalous; but the cause of the discrepancies thus observed not being fully apprehended at the time, the consequence was that the observations were set aside for want of consistency.

Subsequently, however, it occurred to the author that the cause of the discrepancies was to be found *in the alteration of the magnetic intensity of the needle* when the polarity was changed; a circumstance furnishing a new element in these calculations, not hitherto, he believes, taken into account. For when the poles of the dipping needle are changed, (unless magnetized with extraordinary care and some perseverance in repeating the process,) the magnetic intensity of both positions will not be the same. It is indeed a fact which the author frequently observed, that the capacity of a properly tempered steel bar for the magnetic power (provided it have been kept a considerable time in the same magnetic condition,) is the greatest when the polarity is preserved in the usual way, and of consequence loses in intensity of directive force whenever the polarity is changed. This effect, then, if the needle be not *perfectly* balanced and suspended,—which, strictly speaking, it scarcely can be expected to be,—must necessarily follow, that the calculated position, by the usual formulæ, derived from observations made with the needle in opposite conditions of polarity, will not give the true direction of the magnetic force. For wherever there is any defect in accuracy of suspension, balancing, or adjustment, then the needle being acted upon by two forces,—that of magnetic direction, depending on its magnetic intensity, and gravitation, depending on the horizontal leverage of the centre of gravity of the needle in respect to the axis,—its position will, of course, be the resultant of the two operating influences. But if with one direction of the poles the magnetic intensity be *less* than in the other, then the resultant of the combined forces must change its direction from the preponderating of *gravitation there*; and, consequently, the effect of gravitation will not be neutralized by the ordinary mode of mutual correction, because of the relations of that force to the directive being changed. Of this, an example taken from arithmetical means will be sufficient for illustration. Suppose the dip, as determined by the mean of various observations in the usual position of the needle, to be, say 70°, and the result on inversion of poles be 72°, then the actual dip will evidently not be the arithmetical mean of 71°, neither the mean of the tangents of 70° and 72°, that is, 71° 3′, *unless* the magnetic intensity be in both cases precisely the same. But as the intensity in the original direction of the polarity would, probably, be very considerably greater than the other*, the real dip might be 70° 50′, or even 70° 40′, rather than 71°! Hence, for accuracy of result in such cases, a new element seems to be requisite—that of the relative magnetic intensities or powers of the poles of the needle under each condition of polarity—and by observing the number of vibrations of the needle in a given time, in each state of polarity, these reduced to actual intensities would afford an element as a corrective for the source of error herein under consideration.

* The difference of intensity on changing the poles will be the most considerable in the hardest-tempered needles, or in cases where the fixedness of the axis renders the best modes of magnetizing impracticable. In soft bars, or where very powerful magnets are used, the differences from this cause become comparatively trifling, and sometimes altogether disappear; but still there is no security without verification, that in any case the intensities of the changed poles will be the same.

On the Construction of a New Barometer. By the Rev. W. H. Miller, F.G.S., *Professor of Mineralogy, Cambridge.*

This barometer consists of two tubes, of equal diameter, a little longer than the greatest height and greatest range of the barometric column respectively, terminating in a small cistern, the bottom of which can be elevated or depressed by a screw. The long tube is bent so that the upper part of it, which is closed at the end and has a fine point of glass or steel fixed in its axis, may coincide with the prolongation of the short tube, which is open at the end. A graduated scale slides along a vernier attached to the frame of the instrument, in such a manner that a steel point fixed to the lower end of the scale may move in the axis of the short tube.

In making an observation with this instrument, the bottom of the cistern must be elevated or depressed till the surface of the mercury in the long tube touches the fixed point therein: the moveable point on the scale being then brought down till it touches the surface of the mercury in the short tube, the height of the barometric column is indicated by the division of the scale opposite to zero on the vernier. The barometer may be rendered portable by placing a stopcock between the short tube and the cistern.

On a Barometer with an enlarged Scale. By William L. Wharton.

In this barometer a light fluid is introduced upon the top of the mercurial column of the common barometer, the tube of the instrument being enlarged at the point of junction of the two fluids, by which device an instrument of equal or superior extent of scale may be obtained, without the expense and difficulty attendant upon the construction and erection of a barometer of which the whole tube is occupied by the light fluid. Mr. Wharton has employed an instrument of this construction for twelve years without perceiving that its sensibility is at all impaired.

On the Construction of a new Wheel Barometer. By William Snow Harris, F.R.S., *&c.*

The tube of this instrument is 0·5 of an inch in diameter within, bent in a siphon form at one end, and at the other expanded into a flattened spheroidal bulb, whose diameter is four inches, and axis, in the direction of the tube, two inches.

The straight part of the tube, exclusive of the bulb, is 32 inches; inclusive of the bulb, 34; the recurved end is bent twice at right angles, so as to project from the tube 3 inches, and rise parallel thereto 7·5 inches. The tube is attached to a mahogany support, the spheroidal bulb being upwards; and the quantity of mercury is so adjusted in the tube that at mean pressures the upper level is nearly coincident with the greatest diameter of the spheroid, and the lower is near the middle of the shorter leg.

There is a circle of brass, divided into 1000 parts, fixed to the front of a light copper drum or case, having a glass front and back, the centre of which circle is placed just over the orifice of the glass tube: a small frame of brass is fixed to the circle behind, so as to carry a light horizontal axis bearing two small pulleys. The extremities of this axis are turned to extremely fine pivots, and are set in small jewels: the front one projects forward so as to carry a light index of straw, which is sustained on a small brass ring, placed by means of a socket on the extremity of the axis, in the manner of the hand of a watch. The two small pulleys above mentioned carry, by means of fine untwisted silk threads, two small cones of glass or wood, one of which rests on the surface of the mercury in the recurved tube, the other hangs freely on the outside of the tube. These cones are nearly equal in weight, that resting on the mercury being rather the heavier of the two. This slight difference of weight, setting aside the inertia and friction of the axis, is the amount of the resistance which the rising or falling of the mercury in the tube has to contend with; and this is all extremely little, so little that the index moves by the unequal action of the wind during a light gale, and is put into a state of oscillation of some considerable duration by the mere opening of the door of a room. These pulleys measure very nearly one inch in circumference, so that if the mercury moves an inch the index is carried once round the brass circle, and hence one division thereon corresponds to $\frac{1}{1000}$th of an inch, a correction being made on the pulley according to the relative capacity of the tube and the bulb.

The index is made in three parts, of light straws, a centre piece and two extreme pieces inserted into it: one extremity is cut after the manner of a pen to a somewhat short and very fine point, which is turned edgeways. The whole is carefully equipoised by a short piece of straw sliding on one of the extreme pieces, so that when attached to the axis it takes indifferently any position in the circle, and, consequently, follows exactly the movement of the mercury.

A varnished paper is pasted on the front of the tube, marked 27, 28, 29, 30, &c., to denote the height of the mercurial column in inches; these measures being taken with care from the surface of mercury in the bulb to that in the tube. The index is set as nearly as possible when the altitude corresponds to fixed divisions of the scale or measure.

There is a thermometer close to the mercurial column, the bulb of which is placed in a small cistern of mercury, to indicate the temperature, and a hygrometer to measure the change which may be supposed to happen in the silk line, to which the cone resting on the mercury is attached; but the author has found that by employing fine unspun silk the changes are quite unimportant.

The quantity of mercury in the instrument is about 15 lbs. In order to fill the tube clear of air, the following process was adopted as a substitute for boiling. A small iron cap, polished, was first cemented air-tight upon the end of the tube, and into this was screwed an iron stopcock: a long glass tube was then cemented to the stopcock, furnished with iron caps, &c., so that by reverting the instrument and steadying the tubes by cross-bars of wood, tied with silk-ribbon band, the whole may be screwed into the plate of a good air-pump.

The air being withdrawn as completely as a good air-pump will effect, the cock is closed, the whole is detached from the pump, the long tube removed, and the barometer tube transferred to a cistern of mercury, under the surface of which the stopcock is fairly immersed, whilst the tube is inclined as much as possible. The operator, being placed in a convenient position, supports the ball of the tube in his hand, and turns the cock gradually, so as to allow the mercury to be pressed up in an extremely small stream into the tube, and to flow down without violence into the ball. During this process the ball is gently moved about with an easy circular motion, which allows of the more speedy union of the mercury and displacement of the air. An assistant should be ready to close the cock occasionally, for the purpose of examining the state of the mercurial mass within the tube.

In this way the barometer tube may be filled with great nicety, so as to show a most resplendent surface, equal in appearance to that produced by boiling even under a powerful magnifying glass.

When the tube is complete to the point required, the stopcock is again closed, the whole is reverted, and the tube is placed in its intended place; the cock is then opened by degrees, and the mercury will gradually descend to the level of the atmospheric pressure. The iron stopcock and cap may now be removed, by cautious application of first a warm and then a hot iron rod to the cement.

The mercury intended for the purpose of the barometer should be first distilled, and then well agitated, about an ounce or less at a time, in phials capable of holding one ounce and a half. Previously to introducing the mercury into the tube it should be well boiled in a crucible, of porcelain or Wedgwood ware, and should be used just before getting cold, at a temperature of 90° or 100°, the tube of the barometer being also made a little warm by careful exposure to a charcoal fire.

The process now described is believed by the author to be, when carefully performed, in every respect equal to that of boiling. The wheel-barometer made in this way has been compared with other instruments with boiled tubes and of undoubted excellence, amongst others with a fine mountain barometer on Gay-Lussac and Renard's principle, which had been compared with the standard of the Royal Society in Somerset House, and with that in the observatory at Paris. The differences from this instrument, when both were placed in the same room, were very minute. It is found to be more sensible than a very finely boiled tube, carefully prepared by that eminent maker Mr. Cox, of Plymouth, with a scale and vernier divided to $\frac{1}{500}$th of an inch, set up in an adjoining room.

On a new Method of Constructing a Portable Barometer. By JOHN NEWMAN, *Mathematical Instrument Maker.*

The object of this construction is to make barometers portable without the use of a leather bag, which has always been a defective part of the instrument.

The method adopted is to have a cylindrical cistern of iron in two parts, rather longer than usual, the upper part, or chamber, or that to which the cap is fastened, which connects to the tube, being about three times the length of the lower part, of the same diameter, moving round upon a pin, and secured by a screw and collar. The two chambers thus formed communicate internally in one situation by means of holes in the divisions, through which the mercury flows upon inverting the instrument. The vacant space, or that intended to receive the mercury from the tube when the barometer falls, is, when the instrument is in use, in the upper part of the upper cistern, the lower one being full. Upon inverting the instrument, the mercury flows from the latter into the former, which becoming filled, is by a quarter turn of the one now uppermost cut off from communication with it, and the instrument is rendered portable with the end of the tube dipping into a cistern of mercury, which is perfectly secure.

By this method Mr. Newman is enabled to make portable mountain barometers with very large tubes, for sufficient room can be left in the cistern to receive the mercury which flows from the tube into the cistern in high situations, notwithstanding the increased diameter of the tube. Barometers, therefore, can be made and transported, which when put up may be depended upon as standard instruments with perfect security.

On an Instrument for measuring the total heating Effect of the Sun's Rays for a given time. By the Rev. James Cumming, V.P.R.S., F.G.S., *Professor of Chemistry, Cambridge.*

It has appeared to Professor Cumming that the information conveyed to us by the ordinary instruments for measuring the heating power of the sun's rays is, in one respect, imperfect, in as much as these instruments indicate only the momentary energy of the rays: he was therefore led to devise a process which should measure the total result of their action in a given time. The process employed is to expose to the sun a retort with a blackened bulb containing æther, and to note the quantities of this liquid distilled over in different days. In some cases, a second bulb of plain glass has been used to increase the condensing surface, and the apparatus has been otherwise modified. With instruments on this plan Professor Cumming has registered the daily effects of the sun's radiation for more than two years, and he hopes soon to publish his results in a connected form.

On some Electro-magnetic Instruments. By the Rev. James Cumming, V.P.R.S., *Professor of Chemistry, Cambridge.*

The instruments exhibited and explained by Professor Cumming consisted of:

1. A galvanometer of four spirals, similar to that described in his translation of Demonferrand, (pl. v. fig. 86,) but formed of *flattened* copper wire with silk ribbon interposed, each spiral being fixed upon a graduated slide.

2. A Breguet's thermometer, with a conducting wire passed through its axis, for the purpose of measuring either the heat evolved by different galvanic arrangements in passing through a given wire, or that evolved in different wires by the same battery.

On the Thermostat, or Heat-governor, a self-acting physical Apparatus for regulating Temperature. By ANDREW URE, M.D., F.R.S., &c.

This instrument acts by the unequal expansion of different metals in combination: it admits of many modifications of external form, but, in all, the metallic bars must possess such force of flexure in heating or cooling as to enable their working rods or levers to open or shut valves, stopcocks, and ventilating orifices.

Steel and zinc are the two metals employed: they possess a great difference of expansiveness, nearly as two to five, and are sufficiently cheap to enter into the composition of thermostatic apparatus; but zinc has in reference to the present object one property which should be corrected. After being many times heated and cooled, a rod of that metal remains permanently elongated. This property may, however, be in a great measure destroyed, and considerable rigidity acquired by alloying it with four or five per cent. of copper and one of tin. Such an alloy is hard, close-grained, elastic, and very expansible, and therefore suits pretty well for making the more expansible bar of a thermostat.

Let a bar of zinc or of this alloy be cast, about an inch in breadth, one quarter of an inch thick, and two feet long, and let it be firmly and closely riveted along its face to the face of a similar bar of steel of about one third the thickness. The product of the rigidity and strength of each bar should be nearly the same, so that the texture of each may pretty equally resist the strains of flexure. Having provided a dozen such compound bars, let them be united in pairs by a hinge-joint at each of their ends, having the steel bars inwards. At ordinary temperatures the steel plates of such a pair of compound bars will be parallel and nearly in contact, but when heated they will bend outwards, receding from each other at their middle parts, like two bows tied together at their ends. Supposing this recession to be one inch for 180° Fahrenheit, then six such pairs of bows, connected together in an open frame with rabbeted end plates, and with a guide rod playing through a hole in the centre of each, will produce an effective aggregate motion of six inches, being half an inch for every 15° Fahr., or $8\frac{1}{3}$° C. Instead of limiting ourselves to half a dozen such pairs of compound bars, we may readily lodge in a slender iron frame a score or two of them, so as to furnish as great a range of motion as can be desired for most purposes of heat regulation; and the power of pressure or impulsion may be increased, if necessary, by increasing somewhat the thickness of each component lamina. One extremity of the series must obviously be firmly abutted against a solid fulcrum or bearing, while the opposite extremity gives motion to a working-rod of a suitable kind.

The author of the communication then describes in detail the various mechanical adjustments by which the apparatus may be applied to maintain any determined rate of ventilation through the casements of church windows; to give alarm and open valves in water-cisterns in case of fire; to preserve a certain rate of combustion in furnaces, a uniform temperature in baths and stills, and to act as a safety-valve for a steam-engine.

On a Reflecting Telescope. By THOMAS DAVISON, *of Low Brunton, near Alnwick.*

The author of the invention described in this communication is a weaver of linen, who has devoted himself with great perseverance and ingenuity to the construction of telescopes and other instruments. The modification of the ordinary construction of a reflecting telescope which Thomas Davison has executed is intended to improve the performance of the instrument by diminishing the *false or aberrant* light which interferes with the distinctness of the image.

From the nature of its construction, the Gregorian telescope is most exposed to this defect, and it is, therefore, to that form of the instrument that the invention more particularly applies. To each reflector tubes are adapted, having their axes coincident with that of the mirrors, and their free ends directed towards each other. The tube connected to the great speculum enters the hole of that speculum, and is of a slightly conical form, diminishing outwards, and prolonged to such a degree as, without stopping many of the rays which should meet in the image, to prevent nearly all the false light from entering the eye-tube. The tube connected with the small mirror is prolonged so as to meet the extreme rays which converge from the great speculum towards the smaller. Constructed in this manner, Thomas Davison's telescope was found more effective than one upon the ordinary plan. By simple contrivances the instrument can be converted to the Cassegrainian or Newtonian form.

On a Steam-engine for pumping Water. By W. L. Wharton.

In this engine the steam is admitted from the boiler upon a deep float, occupying the top of a column of water contained in a metallic cylinder, placed in the flue of the boiler fire. The lower part of the column of water is connected by pipes to the under side of a piston, moving water-tight in a much smaller cylinder, fixed immediately above the pumps of any mine, to the rods of which is affixed the piston rod. By this arrangement the steam always acts upon a heated surface, and its power is applied to the pump rods without the intervention of a main beam, parallel motion, &c., and, consequently, without any expense for frame-work and buildings requisite for their support in other engines. The friction of this engine, moreover, is very trifling, a stratum of oil being introduced both above and below the piston. A rod or wire is attached to the float, and, passing through a stuffing box in the top of the large cylinder, works the hand gear at the proper periods after the admission and escape of the steam, and consequent depression and elevation of the water and float, within that cylinder. A condensing apparatus may be added, by which the atmosphere may be rendered available, in addition to the weight of the pump rods, to force down the piston in the small cylinder, and, consequently, the water and float to the top of the large cylinder, after each stroke of the engine.

On the Application of a glass Balance-spring to Chronometers. By Edward John Dent.

Mr. Dent described the various difficulties in the construction of chronometers dependent on the imperfection of metallic balance-springs, whether made of gold, or of soft or hardened steel; and explained the advantages which may be expected to arise from the substitution of some substance possessed of greater and more regular elasticity. Glass appeared a substance likely to answer this condition, and when formed into a cylindrical spring, it promised, from the trials that had been made, to be both accurate and durable. An instrument was exhibited with the glass spring in movement.

On the Effect of Impact on Beams. By Eaton Hodgkinson.

The author gave the results of some inquiries into the power of beams to resist impulsive forces. The experiments were made by means of a cast-iron ball, 44 lbs. weight, suspended by a cord from the top of a room with a radius of 16 feet. The ball, when hanging freely, just touched laterally an uniform bar of cast-iron, sustained at its ends in a horizontal position by supports under it and behind it, four feet asunder. The intention was to strike the bar, sometimes in the middle and sometimes half-way between the middle and one end, with impacts obtained by drawing the ball and letting it fall through given arcs, shifting the bar when the place of impact was to be changed, and obtaining the deflections of the bar at that place by measuring the depth which a long peg, touching the back of the bar, had been driven by the blow into a mass of clay placed there. The results were:

1. The deflections were nearly as the chords of the arcs through which the weight was drawn, that is, as the velocities of impact.

2. The same impact was required to break the beam, whether it was struck in the middle, or half-way between the middle and one end.

3. When the impacts in the middle and half-way between that and the end were the same, the deflection at the latter place was to that at the former nearly as three to four, which would be the case if the locus of ultimate curvature, from successive impacts in every part, was a parabola.

The preceding deductions the author had found to agree with theoretical conclusions, depending on the suppositions, (1.) that the form of a beam bent by small impacts was the same as if it had been bent by pressure through equal spaces; and (2.) that the ball and beam, where struck, proceeded together after impact as one mass. These suppositions likewise gave as below:

4. The power of a heavy beam to resist impact is to the power of a very light one, as the sum of the inertias of the striking body and of the beam is to the inertia of the striking body.

5. The time required to produce a deflection, and consequently the time of an impact, between the same bodies, is always the same, whether the impact be great or small. The time, moreover, is inversely as the square root of the stiffness of the beam.

6. The results of calculations, comparing pressure with impact, gave deflections agreeing with the observed ones, within an error of about one eighth or one ninth of the results.

On the direct tensile Strength of Cast Iron. By E. Hodgkinson.

The absolute strength of this metal, notwithstanding the extensive use made of it in the arts, is still a matter of doubt. If we turn for information to authors, we find Mr. Tredgold and Dr. Robison making it nearly three times as great as Mr. Rennie or Captain Brown, and the advocate of the greater strength (Tredgold) attributing the less strength, as found by the others, to the straining force not having been kept in the centre of the prism. For supposing the extensions and compressions to continue always equal from equal forces (which they are under slight strains), a small deviation from a central strain would make a great reduction in the strength; and if the force were applied along the side of a square piece, the strength would be reduced to one fourth. (*Tredgold*, Art. 61, 62. 234.)

The above contrariety of opinion was the cause of the following experiments, in which the utmost care was taken to keep the straining force along the centre of the castings, which had their transverse sections of the form +, except in the last two experiments, in which the section of the castings was rectangular, and the force applied exactly *along the side*. The iron was of a strong kind, the same as in the author's experiments on beams (*Manchester Memoirs*, vol. v.), and was broken by a machine on Captain Brown's principle for testing iron cables.

Force up the middle.

Experiments.	Area of section in parts of an inch.	Breaking weight in tons.	Strength per square inch in tons.
1	3·012	22·5	7·47 } Tons.
2	2·97	21·0	7·07 } mean 7·65.
3	3·031	25·5	8·41 }
4	2·95	19·5	6·59 Different quality of iron.

Force along the side.

Experiments.	Area of section.	Breaking weight.	Strength per inch.
5	4·83	11·5	2·38 } Tons. mean 2·62.
6	4·815	13·75	2·855 }

Whence the strength of a rectangular piece of cast iron drawn along the side is rather more than one third of $7\frac{2}{3}$ tons, its strength, as above found, to bear a central strain $\left(\text{for } \frac{2 \cdot 62}{7 \cdot 65} > \frac{1}{3}\right)$; but from the preceding remarks it ought only to be one fourth; and, therefore, it would appear that a shifting of the neutral line had made the pieces capable of bearing a greater force along the side than in their natural state.

An Investigation of the Principle of Mr. Saxton's locomotive differential Pulley, and a Description of a Mode of producing rapid and uninterrupted Travelling, by means of a Succession of such Pulleys, set in Motion by Horses or by stationary Steam-engines. By JOHN ISAAC HAWKINS.

In order to a clear understanding of the operation of this differential pulley, in the propelling of carriages or vessels, it will be convenient to view the principle under three cases.

Case 1st. Let the bottom spoke or radius of a wheel, rolling on a horizontal plane, be considered as a lever.

Let the point of contact of the wheel with the plane be the fulcrum of the lever.

If a cord be fastened at one fourth of the length of the lever above the fulcrum, and it be pulled a given distance, (say one inch,) then the top of the lever or axis of the wheel will be moved in the same direction four times the distance, or four inches, agreeably with the common doctrine of the lever.

If now a pulley be concentrically affixed to the wheel, and the circumference made to meet the point in the lever where the cord is fastened; in other words, if the pulley be three fourths of the diameter of the wheel, and the cord be wound around the pulley, and be drawn horizontally in the vertical plane of the pulley, then the wheel will run along the horizontal plane in the direction of the pull with a velocity equal to four times the speed of the cord, because every point of the circumference of the wheel as it comes in contact with the plane becomes a new fulcrum, and the perpendicular line from that point to the axis becomes a new lever, upon which the cord acts at one fourth of the length of the lever above the fulcrum, and thus a repetition of such leverage is continually brought into action as the cord is drawn along.

Case 2nd. Let the point where the periphery of the pulley meets the spoke or lever, and where the cord of Case 1. was attached, be considered the fulcrum; and let another cord be applied to the bottom end of the lever. If this lower cord be drawn horizontally in the same vertical plane, but in the opposite direction to that in which the former one was pulled, then the top of the lever or axis of the wheel will be moved in the same direction as before, three times the distance that the cord passes through: thus, if the cord be pulled one inch, the axis will be moved three inches, because the leverage is in this case as three to one. Let the pulley be made to roll along a horizontal plane, and the cord be passed around a wheel concentrically attached to the pulley by the side of the plane, the radius of which wheel is equal to the whole lever, as was the wheel of Case 1.; then the cord being passed around that wheel, and pulled, the pulley will run along the plane with three times the velocity of the cord that draws the wheel, but the motion will be in a direction opposite to the pull.

Case 3rd. Let both the cords of Case 1. and Case 2. be pulled at the same time, (say each one inch,) then the fulcrum will necessarily be removed to a point exactly half-way between the two cords, which fulcrum will be at one eighth of the length of the lever from the bottom end; and the top of the lever or common axis of the wheel and pulley will, in this case, be moved seven inches, being seven times the distance through which the cords pass. The ratio of the velocity of the axis to the cord is as the sum of the two radii of the pulley and wheel divided by their difference.

Now fix a spindle in the axis, and support it on a four-wheeled travelling carriage, or on a vessel afloat upon water, and make a groove in the wheel to constitute it a pulley, and pass a cord around each pulley in opposite directions, and pull both cords with equal speed, then the carriage or floating vessel will be propelled with seven times the velocity of the cords, in the direction in which the cord of the smaller pulley is drawn, because the axis of the pulleys or top of the lever is seven eighths of the whole length of the lever above the fulcrum, and the two cords act at one eighth of the length of the lever above and below the fulcrum, which, in every part of the revolution of the pulleys, remains perpendicularly under the axis, at a height half-way between the bottom ends of the radii of the two pulleys. But if instead of the two cords being attached to the pulleys, an endless cord be stretched around two riggers, placed at some considerable distance from each other, and one side of the cord be made to take one turn around one pulley, and the other side of the cord one turn also around the other pulley, then the cord being drawn at either side or either end will cause the pulleys to revolve, and the carriage or vessel in which they are hung to be propelled the whole length of the space between the riggers, with a speed seven times greater than the motion of the cords.

In applying this admirable invention of Mr. Saxton to the propelling of carriages to great distances, Mr. Hawkins proposes to place a number of endless ropes in a line, each rope stretched between two riggers, from a quarter of a mile to four miles apart, the rope lying upon several rollers to keep it off the ground, and passing around a pair of differential pulleys, supported on a light four-wheeled truck, running upon a pair of slight rails about 30 inches apart; the diameter of one pulley to be about 22 inches, and of the other about 26 inches, giving a velocity of 12 to 1: the diameter of the wheels on which the truck runs to be about 30 inches. Each rope to be set in motion from one of the two riggers being placed on a shaft passing under the rails and extending a few feet outside the railway, where the shaft may be turned either by a horse or horses, by an ox or oxen, or by a stationary steam-engine, according to the quantity of travelling or traffic on the road, or to other circumstances. The coach for passengers, or wagon for goods, to be placed upon four wheels, of about four feet diameter, running upon a pair of rails, placed five feet apart, parallel with and lying on each side the pair of truck rails, and also a little above their level; so that the axletrees of the coach and wagon wheels shall pass over the rims of the truck wheels; or the same effect may be produced by placing the four rails on a level, and cranking the axletrees to raise them over the truck wheels. A pawl in the frame of the carriage or wagon being let fall upon a post arising from the frame of the truck, will enable the truck to draw or drive the carriage the length of its rope; but on the truck being stopped near the end of its rope, the momentum of the carriage will continue its motion until it pass over and beyond the truck of the next rope, which truck being set in motion, its post catches against the pawl of the carriage, and drives or draws it on until it reach the third truck, which again operates in the same manner.

In this way 388 horses, each acting, at their most effective or walking pace of two miles and a half per hour, on a mile of rope, might easily drive a coach containing eight persons from London to Edinburgh in 13 hours, at the rate of 30 miles an hour, the coach passing from truck to truck without stopping, and the truck returning to take another coach every five minutes: 500 passengers a day for the whole distance would be very moderate labour for that number of horses.

Account of the Depths of Mines. By JOHN TAYLOR, F.R.S., *&c.*

Mr. Taylor exhibited a section, showing the depths of shafts of the deepest mines in the world, and their position in relation to the level of the sea.

The absolute depths of the principal ones were:

	Feet.
1. The shaft called Roehrobichel, at the Kitspühl mine in the Tyrol	2764
2. At the Sampson mine, at Andreasberg in the Harz	2230
3. At the Valenciana mine, at Guanaxuato, Mexico	1770
4. Pearce's shaft, at the Consolidated mines, Cornwall	1464
5. At Wheal Abraham mine, Cornwall	1452
6. At Dolcoath mine, Cornwall	1410
7. At Ecton mine, Staffordshire	1380
8. Woolf's shaft, at the Consolidated mines	1350

These mines are, however, very differently situated with regard to their distance from the centre of the earth, as the last on the list, Woolf's shaft, at the Consolidated mines, has 1230 feet of its depth below the surface of the sea, while the bottom of the shaft of Valenciana in Mexico is near 6000 feet in absolute height above the tops of the shafts in Cornwall. The bottom of the shaft at the Sampson mine in the Harz is but a few fathoms under the level of the ocean; and this and the deep mine of Kitspühl form, therefore, intermediate links between those of Mexico and Cornwall.

Mr. Taylor stated, that taking the diameter of the earth at 8000 miles, and the greatest depth under the surface of the sea being 1230 feet, or about $\frac{1}{4}$th of a mile, it follows that we have only penetrated to the extent of $\frac{1}{32000}$ part of the earth's diameter.

Some account was then given of the mines to which the shafts referred to belong.

Of the deepest, at Kitspühl, as it has long ceased to work, we do not know much. Villefosse, in his great work on the *Richesse Minérale de l'Europe,* states that this was a copper mine, which passed for being the deepest in Europe; and that in 1759, it was reported on, amongst other mines, by MM. Jars and Duhamel, and it was then proposed to abandon the working, the water having been already suffered to rise near 200 fathoms.

The Sampson mine in the Harz is one of the most celebrated in that district: it has been working since the middle of the sixteenth century, and produces silver ores of superior quality. The principal shaft is sunk about 6 feet deeper every year, by which ground enough is drained for a regular extraction of the ores. The mine is one of the oldest in Germany, and has always been profitable: it employs from 400 to 500 men. It is the property of shareholders, who are very numerous, the interest having been much subdivided in the course of time.

The mine of Valenciana at Guanaxuato was one of the most renowned in Mexico. It produced annually, about the end of the last century, 360,000 ounces of silver, worth about £600,000 sterling, and then employed 3100 persons. The shaft referred to in the section was commenced in 1791, the mine having been long previously worked by other shafts: it had attained its present depth in 1809, when the mine was stopped by the Revolution. It is octagonal, and more than 30 feet in diameter, a great part of its depth being walled with beautiful masonry, and is probably the most magnificent work of the kind. The expense of forming this shaft is estimated by Humboldt at the enormous sum of £220,000. The mine was so little troubled with water that it was considered almost a dry one: during the suspension of the works it, however, gradually filled. In 1825 one of the English companies undertook to drain it, which was, after great labour and expense, accomplished; but the mine has not been sufficiently productive since to make it worth while to continue the working.

The Consolidated mines form the most extensive concern in Cornwall, embracing what were formerly several distinct mines, which, as the name indicates, were connected in one undertaking.

This was arranged in 1818, and the mines which had remained unwrought for many years were drained by very powerful steam-engines, and were put into a state of active working. The management was confided to Mr. Taylor and the late Captain William Davey: an outlay of £73,000 was incurred, which has since been repaid with ample profit. The present produce is 20,000 tons of ore a-year, yielding about 1920 tons of fine copper, being one seventh of the whole quantity raised in Great Britain. The mines employ about 2400 persons, of whom about 1400 are miners working underground. The water raised to the adit level is about 2000 gallons per minute: the height to which this is lifted is more than 220 fathoms, or 1320 feet; the aggregate weight of the columns of water in the pumps being 512,000 pounds, or about 230 tons, and the whole is put in motion by eight immense steam-engines, four of which are the largest ever made.

The depth of the mines has been increased 100 fathoms since the period of the drainage being completed, being at the rate of about 8 fathoms a year.

There are, in the whole concern, 95 shafts, besides other perpendicular communications from level to level underground called winzes. The depths of the whole added together make up about 22,000 fathoms, or 25 miles; and the levels, or galleries, will make up, in horizontal distance, a length of 38,000 fathoms, or about 43 miles.

Wheal Abraham is an old copper mine, the working of which was abandoned a few years since, the vein having ceased to be productive in depth. It was, until very lately, the deepest mine from the surface in Cornwall, but is now surpassed by the Consolidated mines.

Dolcoath mine was formerly called Bullen Garden, and a section of it as it was at that time will be found in Dr. Pryce's work, *Mineralogia Cornubiensis*, published in 1778. It wasthen rather more than 90 fathoms deep, and probably one of the deepest mines at that time. It has, therefore, been sunk 140 fathoms since; but, like all the great mines, it has not been in constant work. It has now been actively prosecuted for many years, and at present stands third in the list of copper mines in Cornwall, arranging them according to the value of their produce. That of Dolcoath, however, does not amount to one half of that of the Consolidated mines.

Ecton mine is celebrated in most books on mineralogy as one of the principal copper mines in England; and it was so at one period, though the produce is now inconsiderable. It is situated in Staffordshire, on the borders of Derbyshire, and is very curious, from being in limestone and having no regular vein. The ore has been found in large masses, irregularly deposited, and is generally taken to be an example of contemporaneous formation. The mine has been regularly worked for a long series of years, and is now nearly exhausted. It is the property of the Duke of Devonshire, and very large profits were given by it in the latter part of the last century, some of which, it is said, were applied by the late Duke to the erection of the beautiful Crescent at Buxton. The mine is not far distant from this place, and is in a very picturesque situation on the banks of the river Manifold.

Mr. Taylor gave some account of the extent to which steam power is at present employed in Cornwall in draining the mines which penetrate so far beneath the level of the sea, showing the influence that the great improvements, which have from time to time been made, and many of them even recently, must have upon the production of some of the most useful metals.

The number of steam-engines used in pumping water from the mines in Cornwall in December 1832 was altogether 64.

Some of these are of immense size and power: there are five in the county, of which the diameter of the cylinder is 90 inches, the pistons making a stroke of 10 feet. Four of these are at the Consolidated mines, and the first constructed of this size was planned and erected there by Mr. Woolf. The beam of such an engine weighs 27 tons; the pump rods are of mast timber, 16 inches square, connected by iron strapping plates of enormous weight. The column of water lifted, the rods and beam, make up a weight of more than 100 tons, and this is kept in motion at the rate of from 5 to 10 strokes per minute.

The quantity of coal consumed in drawing water in the same month in all the mines of Cornwall was 84,034 bushels, and the quantity of water delivered, about 19,279 gallons per minute. The weight of water actually poised by all these engines to produce this effect amounts to about 1137 tons.

From calculations carefully made in Mexico as to horse power employed in draining mines, and deduced from a large scale of operations, it is found that the performance is equal to 19,000 lbs. raised one foot high per minute for each horse.

According to this rate, the coal consumed in Cornwall in a month being 84,000 bushels, or 2800 per day, and taking the duty of the engines at 55,000,000 pounds lifted 1 foot by each bushel, which is very nearly the fact, it will be found that the sixteenth part of a bushel does as much in raising water in Cornwall as a horse does in Mexico, (working 3 hours out of 24,) and that thus the number of horses required to drain the mines of Cornwall would be 44,800.

On Naval Architecture. By JEREMIAH OWEN.

A great deal has been done by mathematicians towards attempting to establish a general theory of resistances, and considerable expense has been incurred in conducting experiments, some of which have been made on the Continent under the superintendence of eminently scientific men. D'Alembert, Bossut, Romme, and several others were employed at different times in experiments of this nature. Don Juan in Spain, and Chapman, the great Swedish naval architect, also made several experiments on the same subject; as did also the Society for the improvement of Naval Architecture, which was established in England some years ago, but which has now ceased to exist.

These experiments have always been made upon models, the largest of which, it is believed, have never exceeded 14 feet in length. They were generally much smaller. The results which have been thus obtained on small bodies have not been found to agree with results similarly obtained on larger bodies; and not only has this been the case, but experiments conducted apparently with equal care by different individuals have even led to different results.

Naval architecture has, consequently, gained but little from the labour that has been bestowed upon these experiments, and the forms which have been given by different individuals to ships have depended rather upon the fancy and general experience of those individuals than upon any facts which this branch of experimental science has furnished.

In order to discover that form of the body of a ship which shall oppose the least resistance to its passage through the water, the author recommends that experiments be made on ships themselves, under all the ordinary circumstances of sailing.

These experiments must be conducted, not in the mode in which experimental squadrons have hitherto been, viz. by comparing together the sailing qualities of ships that have varied in every particular. We are no more justified in saying that results obtained in this way have proved which form of body is best calculated for velocity, than we are in saying which ship has been the best managed.

If it be desirable to discover by experiment which of two or more forms is best adapted for velocity, it is, of course, necessary that the form shall be the only variable element; the ships in *every other* respect must be exactly similar.

By the aid which the mathematics afford, we shall be able most completely to accomplish this. Let a ship be given which sails well, and which is in all other respects an efficient man-of-war as regards capacity, stability, &c. &c. Let this ship be docked, and let the most complete drawings of her form be made, from which we shall be able to calculate exactly every necessary element, such as displacement, area of midship section, of load water section, stability, &c.; and let the surface of sail, the position and rake of masts be also acertained.

Then let one or more ships be constructed, having exactly the same principal elements as the given one, with whatever difference of form it may be thought proper to select, and let the same surface of sail be given to them all. We shall thus have the same weights to be moved, and the same propelling force to move them; the result will, of course, show which form is best calculated for velocity.

These ships may be made to sail against each other under ever possible circumstance of sailing, taking care always to apply the propelling force in the same way, that is, by bracing the corresponding yards of all the ships to the same angle with a fore and aft line, during every comparison, and by raking the masts to the same degree.

It is of importance that during the experiments the surface of sail in each ship should be presented as nearly as possible at the same angle to the action of the wind; and this is perfectly practicable, for it is easy to measure the angles of the yards by an instrument for that purpose; and the officer who commands the squadron can take care, by means of frequent signals, to have the yards of all the ships braced to the same angle at the same time.

These experiments would not be limited in their result to the discovery merely of that form of a ship which is best calculated for velocity, although this of itself is so important as to justify almost any expense, but we might also be able to discover how far the angle of leeway is affected by the form, which is also a very important question connected with the sailing of ships; and after having, by repeated and careful trials, discovered the order of superiority of ships in respect of velocity, we might then, by varying the angle of bracing the yards, discover also the trim of the sails and the course of the ship by which to gain most on a wind, a question which is not by any means satisfactorily settled, in as much as it involves all the uncertainty of our present knowledge of the resistance of fluids.

Experiments to determine this latter question might, however, be made immediately on sister ships, of which there are several at present in His Majesty's service.

Let two ships be selected of the same form, and let the utmost pains be taken to make the position and rake of the masts, the seat in the water, the stowage of the ballast, and of all the heavy weights, exactly the same in both ships, and let them be compared together in sailing both on the wind and at various points off the wind; the angles of bracing the yards being constantly varied, we should doubtless, from a series of experiments of this nature, succeed in discovering the best trim of the sails for every direction of the wind on every course.

It may, perhaps, be urged against a series of experiments like those which have been here recommended, that the expense of building a ship is so great as to render it advisable not to run the risk of building a bad one for the sake of experiment merely. But the author suggests that our knowledge of naval architecture is such as to enable us to construct ships which we can certainly predict will be good, notwithstanding they may not be the best that might be produced; and the ships which would be included in the experiments proposed would have all the essential qualities of a man-of-war, except, that by differing in form, some would be superior to others in respect of velocity.

Naval architecture is a branch of science which depends so essentially upon experiment for its advancement, and the experiments are necessarily upon so large and expensive a scale, as to place it out of the power of individuals, or even of societies of individuals, to conduct them. It is, therefore, one of those enterprises in science which none but a nation can undertake; and it is worthy of so great a maritime nation as England to endeavour to advance, at whatever reasonable cost, a subject so important to its defence.

III. NATURAL HISTORY—ANATOMY—PHYSIOLOGY.

On the originary Structure of the Flower, and the mutual Dependency of its Parts. By Professor Agardh.

The observations in this paper were generally directed against the commonly adopted view, that the flower is formed of several verticils independent of each other. The author remarked the difference between the *appearance* of the verticillated parts and their *real* and *originary* situations. Adopting the view, that the flower is nothing more than a branch, which has been reduced to a mere point whilst its subordinate parts have been transformed, he concluded that the different parts of each verticillus formed an originary spiral, and are really of unequal order, age, and situation, which, in many cases, is still evident during the inflorescence; and imagining the branch shortened to a point, it will be found that the upper, later, and weaker parts must be the inner ones of the apparent verticillus.

The second point of Professor Agardh's view is, that the stamens are not transformations of petals but of buds. This view is consistent with the whole theory of the development of plants, as laid down in a separate work (*Organography of Plants*), and founded on the principle, that the several appendicular parts of the plant are not all transformed leaves, but only one part of them are transformed leaves, and others are transformed buds, so that to every part which is a transformed leaf belongs another part, which is a transformed bud. The stamens are now, according to Professor Agardh, the buds of the leaves of the flower, or of the petals and sepals. They are therefore situated in their axillæ, and each stamen belongs to a certain petal or sepal, and both organs together form a little flosculus as part of the whole flower, in the same way as the carpella are parts of the fruit. Thus a Decandrous plant, for example a *Cerastium,* consists of 10 floscules, each consisting of a leaflet and of a stamen. A Pentandrous plant, for example a *Borago,* consists also of 10 floscules; but 5 of them, those of the interior verticillus, are incomplete, bearing no stamens in their axillæ.

The observations made on the situation of the parts of the flower were collected by Professor Agardh into the following general laws or views.

1. The number of stamens, in all cases where this number is determinate, depends upon the number of sepals and petals; and when there seems to be a different normal number for the leaves of the flower and for the stamens, it is an aberration arising from abortion.

2. The difference between the flowers which have the same number of stamens as of leaflets, and those where their number is only the half of the leaflets, is caused by the abortion of the stamens of a whole verticillus of floscules; and generally of the corolline floscules. The reason is, that the corolline verticillus is constituted of later parts, which do not arrive at complete development.

3. The same reason is to be assigned to the general fact that the petaline stamens are generally the weaker, smaller, and later.

4. The determinate stamens are either 1, 3, or 5 in number, belonging to each leaflet, because the buds, according to Professor Agardh's theory laid down in his above-mentioned work, originate properly in the axillæ of two deviating fasciculi of spiral vessels, which fasciculi in the leaf (being no other than the nerve) are always 1, 3, or 5, &c., and the buds must therefore have the same symmetry and number. This is the reason of the determinate number of stamens in several Polyandrous families, as, for example, in *Rosaceæ,* in which the sepals have each 3 stamens, and the petals each 1; and in *Philadelphiæ,* in which the sepals have 5 stamens, and the petals 1: whence the former family must have 20 stamens, and the latter 24.

5. Some Polyandrous plants have not a determined number of stamens. In these Professor Agardh distinguished two cases.

In some, as in the *Ranunculaceæ*, the buds of the flower are in a vascillating state between the form of stamens and flower-buds, and even the sepals are in a vascillating state between bracteæ and sepals. In *Helleborus, Nigella*, &c., the inferior buds are nearly like flowers, and also in Ranunculus the buds are only to be regarded in the same state of transition to petals, as the sterile flowers of *Synanthereæ* approach the form of petals. By this is explained, not only why the nectaria of *Helleborus* are axillary to the sepals, which they could not be if they were originary petals or leaves, but also why the nectaria of *Berberideæ*, which are so nearly allied to *Ranunculaceæ*, are axillary to the sepals; and finally, why there is an evident transition between stamens and petals in *Nymphæaceæ*, and in all the families allied to *Ranunculaceæ*, the buds in the flower having an equal tendency to form *flowers, petals*, and *stamens*.

The other case of Polyandrous plants is where no relation at all is observable between the flowers, leaves, and the number of stamens. This is to be explained by the analogous case, where the flowers in capitula, as, for example, of some *Synanthereæ*, are without distinct bracteæ to each single flower; and it is not more singular, that the stamens should be in some cases without their respective flower-leaves, than that the flowers in some cases should be without determinate bracteæ.

6. When some floscules in the same verticillus are sterile or without stamens, they are frequently those which are younger or later than the others. The same reason is to be assigned for the inequality of the stamens in the same verticillus. Ex. *Personatæ, Labiatæ*, in which the two younger stamens are smaller, and the youngest stamen fails.

7. The ternary number in Monocotyledonous plants is derived from a leaf-bud, in which two outer leaves, or squamæ, turn their back to the stem, and form two sepals; and the third sepal is the leaf, in the axilla of which the bud is situated. This is evident in *Carex* (in which the two leaves coalesce into the utriculus,) and in the *Gramineæ*.

8. Dicotyledonous plants have their flowers formed on two different plans.

9. One group of them has originally opposite leaves; on these the floscules are naturally in pairs, and when a fifth floscule exists, it is to be regarded as the last, and the only one of the third pair which has found sufficient room to develop. (See *Calyx* of *Dianthus*.)

10. The other portion of Dicotyledonous plants has alternate leaves, and, in consequence, impair and unequal floscules; but

the floscules have a tendency to take a symmetrical form on both sides, so that in this case a floscule exists which is especially to be regarded as impair, and which is the first or the last in the spiral. This impair floscule is commonly placed either nearest to the axis of the racemus (*axilis*), or outermost in the periphery (*periphericus*) of the racemus.

11. The petaline floscules have a contrary progression to the sepaline, so that if the odd or impair *sepal* is placed nearest to the axis of the racemus, the odd or impair *petal* is placed outermost in its periphery.

12. The situation of the impair floscule is different in different families, for example, the odd sepal, (the first or last sepal,) is *axilis* in *Labiatæ, Personatæ, Umbelliferæ*, and *periphericus* in *Leguminosæ, Rutaceæ*, &c.

13. By the situation of the carpella two cases are to be distinguished.

14. In some cases the carpella are commensurable with the number of floscules. They are then placed either parallel to the sepals, as in the *Liliaceæ, Primulaceæ, Geraniaceæ, Cruciferæ*, or parallel to the petals, as in the *Rutaceæ, Philadelphus, Onagrariæ*.

15. In other families, and by far the greatest part, the carpella are two, (complete and incomplete,) and thence not commensurable with the five divided flowers. In this case one carpellum is parallel with the impair sepal, and the other with the impair petal. The fruit of the *Boragineæ* and *Labiatæ* is to be referred to this case, two carpella taken together being placed parallel to the impair sepal, and the two others parallel to the impair petal, the fifth carpellum having vanished.

Notice of Researches on the Action of Light upon Plants. By Professor DAUBENY.

The author communicated a notice of certain researches which he is at present pursuing concerning the action of light upon plants, and that of plants upon the atmosphere.

He considers that he has established, by experiments on plants immersed sometimes in water impregnated with carbonic acid gas, and at others in atmospheric air, containing a notable proportion of the same, that the action of light in promoting the discharge of certain of their functions, and especially that of the decomposition of carbonic acid, is dependent neither upon the heating nor yet upon the chemical energy of the several rays, but upon their illuminating power.

He regards light as operating upon the green parts of plants in the character of a specific stimulus, calling into action and keeping alive those functions from which the assimilation of carbon and the evolution of oxygen result, and that the description of rays which are proportionally more abundant in solar than in artificial light are those most instrumental to the above purposes.

With regard to the second branch of the inquiry, Professor Daubeny has only proceeded in it so far as to have satisfied himself, that in fine weather a plant consisting chiefly of leaves and stem will, if confined in the same portion of air night and day, and duly supplied with carbonic acid during the sunshine, go on adding to the proportion of oxygen present, so long as it continues healthy, at least up to a certain point, the slight diminution of oxygen and increase of carbonic acid which takes place during the night bearing no considerable proportion to the degree in which the contrary effect is observable by day.

He accounts for the discrepancy between his own results and those reported by Mr. Ellis in his work on Respiration, by his having taken care to remove the plant from the jar immediately upon its beginning to suffer from the heat or confinement, and from his having carried on the experiments upon a larger and more suitable scale.

Considering the quantity of oxygen generated by a very small portion of a tree or shrub so introduced, he sees no reason to doubt that the influence of the vegetable may serve as a complete compensation for that of the animal kingdom, especially since this same function appears to belong to every plant which has come under his review, whatever may be its structure or organization.

On some symmetrical Relations of the Bones of the Megatherium. By Walter Adam, M.D.

The author, having examined the bones of the megatherium which are preserved in the Museum of the College of Surgeons, was led to observe their forms according to their symmetrical relations. For this purpose, the coronal breadth of the cranium is taken as a common term of reference. It measures 8·75 inches, but in the following scale of proportions its breadth is denoted by 10, and all the other measures are altered in the same ratio, and expressed by the nearest integers. Dr. Barclay's nomenclature is employed.

HEAD	Coronal breadth of cranium	10
	Mesial thickness of the bony plate forming the palate and the basilar surface of the nasal passage	2
	Mesial height from the surface of the palate to the concavity of the coronal surface, about the rostral margin of the rostral molar	8
	Greatest height of the fragment of the head from the palatal surface in the direction of the socket of the third molar	12
VERTEBRÆ	Breadth of the transverse process of the atlas	14
	of the fifth cervical	9
	of one of the largest dorsal	12
	of a caudal vertebra, supposed to be that next the sacrum	25
	of the seventh of the 12 caudal which remain	10
	of the smallest caudal	4
	Length of the body of the atlas	3—
	of the fifth cervical	3—
	of one of the largest of the dorsal	4+
	of one of the lumbar	5
	of six caudal	4+
	of the smallest caudal	3

In the first three vertebræ of the tail in which the length is diminished, that dimension is greatest on the upper or dorsal aspect, indicating that about the middle the tail had a tendency to curvature downwards and forwards.

Longest spinal process of a dorsal vertebra		16
SPINAL CANAL	Width in atlas	3
	in fifth cervical	3+
	at the pelvis	4

The ribs of the megatherium were connected to the sternum by osseous attachments.

Length of the longest rib which has been found (without its sternal attachment)		36
	of the shortest rib found (probably the first)	15+
Greatest breadth of the shortest rib		5
	of the longer ribs	3·5
Thickness of the longer ribs		1·8
STERNUM	Breadth of the rostral portion	8
	Mesial length of the same	10

Pelvis	Greatest transverse extent of the iliac bones	75
	Width of pelvic aperture	16
	Depth of ditto	28
	Breadth at the mesial margins of the acetabula	18
	between the acetabula and the ischial tuberosities	28
	at the ischial tuberosities	24
	Thickness of bone at the ischial tuberosities	3
	Symphysis pubis, its rostro-caudal extent	11
	From the sternal surface of the symphysis pubis to the sternal surface of the spinal canal, the mesial distance is	39
	Length of the thyroid foramina	12
	Breadth of the same	6
	From the lateral extremity of the left iliac bone to the right ischial tuberosity	58
Scapula	Breadth from the acromico-glenoid sinuosity	17
	from the caudal margin of the glenoid cavity	20
	Length of the glenoid cavity	7
	Extent of dorsal margin of scapula	31
	from the dorso-caudal angle to the extremity of the acromion	33
	From the acromion to the glenoid cavity, (greatest breadth)	14
	Thickness of scapula at the dorso-caudal angle	3
Clavicle	This strikingly resembles that of man.	
	Its length	17
	Its girth	7
Radius	Its length	28
	Its greatest breadth near the os humeri	8
	Its smallest girth	10
Astragalus	Breadth	11
	Length	10
Calcaneum	Digital breadth	8
	Greatest length	20
	Proximal breadth	12
	Smallest girth	20
Crus	Digital breadth	13
	Proximal breadth	14
	Fibular length	24
	Tibial length	25
	Tibia, smallest girth	16
Femur	Breadth	18
	Smallest girth	30
	Greatest length	32

Dr. Adam observes, that by the completion of Mr. Clift's labours in adapting and mounting the remains of this animal, some peculiarities now visible in the internal structure of the bones will be concealed, and on this account he directs the attention of zoologists to the following observations:

"In the thicker parts of the ribs as well as of the bones of the limbs where broken, there are dispersed and conglomerated in the reticulated texture, like the spherules in some cryptogamous plants, numerous round bodies from one tenth to two tenths of an inch in diameter. These bodies are hard, but of a steatomatous appearance: they seem to have resulted from the same exuberance of ossification so conspicuous in the external surface. The external surface of the thicker parts of the bones looks as if formed by a conflict of the osseous spiculæ, which are of the size of coarse needles."

Dr. Adam is of opinion, that probably the nails of the megatherium might have been doubled under the foot in the same manner as those of a living cognate species, the short-tailed manis, the feet of which living species had hitherto been incorrectly figured in zoological works.

On some new Species of Fossil Saurians found in America. By R. Harlan, M.D., *of Philadelphia.*

The species of saurians mentioned in this communication had been all examined by Dr. Harlan, and a full account of them is preparing for publication. The following extracts will make known the names and localities of these fossils.

1. *Ichthyosaurus Missouriensis.*—A fragment of the head has been found in a hard bluish grey limestone, near the junction of the Yellow-stone and Missouri rivers.

2. A dorsal vertebra, analogous to those of plesiosaurus, except that its length is remarkably greater in proportion to its breadth. Found in marl on the banks of the Arkansaw river: supposed to belong to a very large individual. The marl contains many bivalve shells.

Remains of crocodiles, geosauri, &c., were also mentioned by Dr. Harlan as occurring in West New Jersey in marls.

Remarks on Genera and Subgenera, and on the Principles on which they should be established. By the Rev. Leonard Jenyns, *A.M., F.L.S.*

The object of this paper was to make some remarks on the great multiplication of genera at the present day, and to show that in constructing them sufficient attention had not always been paid to the true principles of classification. It was particularly stated that in this country zoologists had very much overlooked the principle which determines that all groups bearing the same title should be groups of the same *value*; and that in raising to the rank of *genera* the *subgenera* of the French, they put these last on the same footing with groups of a higher denomination, to which in strict reality they were subordinate. Instances were brought forward from amongst the genera of British birds, in which this disregard to a due subordination of groups was particularly manifest. It was mentioned that in this way *Plectrophanes* was made a group of equal value with *Emberiza, Lagopus* with *Tetrao, Coturnix* with *Perdix,* and *Botaurus* with *Ardea,* although it might be clearly seen, that in each of these instances the first group rested on characters far less important and less numerous than those which were common to the two considered as one genus.

Some remarks were then made on the method of ascertaining the value of any new group that presents itself. It was observed, that to fix this with certainty required a previous acquaintance with all the other existing groups belonging to the same family, and that therefore it can only be determined so far as the present state of our knowledge of that family will allow. If it be found on comparison that its characters are of equal value with those of other *acknowledged* genera in that family, the group in question may be considered as a genus also; but if of less, it is clear that the group itself is one of less importance, and must occupy a subordinate station.

The author concluded with pointing out the impropriety of splitting up natural genera, as had been done in some cases, merely because they contained a large number of species. He stated that the value of a group was not affected by such a circumstance; furthermore, that no groups should exist in our systems but such as exist in nature; and that for the mere purpose of abridging labour in the search after particular species, it was quite sufficient in the case of extensive genera to institute *sectional* divisions, indicating such sections by signs.

On some parts of the Natural History of the Common Toad. By James Macartney, *M.D., F.R.S.*

After commenting upon the unfounded prejudices against the whole class of reptiles, and the toad in particular, the author corrects an error concerning the mode of feeding of the toad—into which even Linnæus had fallen—that the flies are attracted into its mouth by a power of fascination. "The toad takes its prey in the same manner as the chamæleon and many other lizards, by projecting its tongue, striking the insect, and drawing it back into the mouth, and this it does so rapidly that the action cannot be seen; but if a fly alights on the outside of a glass vessel in which a toad is inclosed, the creature, thinking its prey is within its reach, performs the usual act, and the stroke of the tongue is very distinctly heard against the inside of the glass opposite to the fly."

Stories are very frequently published of living toads being found encased in solid rocks and in the trunks of trees, and these accounts receive very general credit. To ascertain how far this is probable, Dr. Macartney made the following experiments.

He placed a toad in a glass vessel covered loosely with a piece of slate, and buried the vessel containing the toad about a foot deep in a garden; on digging it up a fortnight afterwards, the animal was in perfect health, and had recovered from a wound it had previously received in the thigh. He then took the same toad, and having secured the top of the vessel in such a way that no air nor moisture could be admitted, he buried it in the same place, and on raising it a week afterwards found the animal dead and putrid: from hence he concluded that the toad cannot live if moisture and atmospheric air be perfectly excluded. It is very probable that toads have been often found alive in chasms of rocks, or in hollow trees having a small aperture through which the air and also insects might enter; but that any animal possessing lungs should live for an indefinite time without some communication with the atmosphere appears quite incredible.

Cuvier has stated that the toad, although not venomous, yet when provoked ejects a liquor from two glands placed on its head, which is capable of irritating the skin. Dr. Macartney has often had toads in his possession, but never observed anything of the kind; nor does he believe that the animal has any disposition to injure others: on the contrary, the toad is very gentle, capable of being domesticated, and of becoming attached to those who treat it well.

It is a popular notion that toads cannot live in Ireland, which opinion is in some degree countenanced by the fact of there being no reptiles in that country except the water-newt and the frog, and the latter was introduced within the last century. It is also understood that there are no reptiles in the Isle of Man. The climate of both these islands being more moist than that of England would be particularly suitable to frogs and toads, although it would probably be unfavourable to serpents and many kinds of lizards.

Some years ago the author brought eleven toads from this country to Ireland, and as he did not wish them to be propagated, on account of the alarm and disgust which many weak people feel towards them, he buried them in a flower-pot covered loosely with a slate, to prevent the earth falling in upon them. In this situation he kept them for two years, occasionally digging them up, for the purpose of exhibiting them and making them the subject of experiments. They at length all died during a very hot summer, the ground in which they were buried having become so dry that the animals could no longer receive any moisture; for although the toad eats many insects when it is at liberty, it will live and increase in size by imbibing moisture alone, for which purpose its skin is provided with numerous pores.

The toad possesses greater powers of repairing the effects of injuries than most other animals. One of the toads which has been mentioned as living beneath the surface of the earth for two years had been subjected to the experiment of having the upper part of the skull removed, and a portion of the brain scooped out. The wound rapidly healed, leaving a depression corresponding to the quantity of bone and brain taken away. The only effect which remained from this injury was that the animal afterwards did not walk in a direct line, but in curves to the one side, a fact which has been observed in other instances consequent to injuries of the brain. Dr. Macartney has seen one instance of the same kind in the human subject, the person being incapable of locomotion, except in circles, as if he were waltzing.

There is one fact in the natural history of the toad which the author believes to be quite unknown,—the utterance by the animal of a musical sound, consisting of one note, so clear and pure that it perfectly resembles that which is produced by striking a piece of glass or some sonorous metal. The season of the year in which this was heard was the latter part of autumn. Dr. Macartney concludes by observing that "One object in studying zoology, and that not an unimportant one, is, by closely investigating the habits of animals, to remove the prejudices and apprehensions which are traditionally handed down to us from those ages in which fable took the place of knowledge. Many of these errors and prejudices with respect to animals exist in the present day, even amongst well informed persons, to an extent that would scarcely be believed unless our attention had been directed to the subject. In selecting the history of the toad, I have merely employed a remarkable example of the fact."

Observations relative to the Structure and Functions of Spiders. By JOHN BLACKWALL, *F.L.S.*

During the last three years the author has been engaged occasionally in conducting experiments having for their object the determination of a highly interesting question in physiology, namely, what are the true nature and functions of the remarkable organs connected with the fifth or terminal joint of the palpi of male spiders? The opinion advanced by M. Treviranus, and adopted by M. Savigny, that those parts are instruments employed for the purpose of excitation merely, preparatory to the actual union of the sexes by means of appropriate organs situated near the anterior extremity of the inferior region of the abdomen, is in direct opposition to the views of Dr. Lister and the earlier systematic writers on arachnology, who regarded them as strictly sexual; and the results of the author's investigations clearly demonstrate the accuracy of the conclusion arrived at by our celebrated countryman.

In the spring of 1831 Mr. Blackwall procured young female spiders of the following species, *Epeira diadema, Epeira apoclisa, Epeira calophylla, Epeira cucurbitina, Theridion nervosum, Theridion denticulatum, Agelena labyrinthica,* &c., and having placed them in glass jars, fed them with insects till they had completed their moulting and attained maturity, which is easily ascertained in most instances by the perfect development of the sexual organs. He then introduced to them adult males, taking care to remove the latter as soon as a connexion had been consummated in the usual manner, by the application of the palpal apparatus to the orifice situated between the plates of the spiracles in the females. He never in any instance suffered the sexes to remain together any longer than he found it convenient to continue his observations, and remarks that their union, however prolonged and undisturbed, was invariably accomplished in the manner stated above. After a lapse of several weeks the females thus impregnated respectively fabricated their cocoons, and deposited their eggs in them, all of which proved to be prolific; affording a complete refutation of the opinion promulgated by M. Treviranus.

That there might not remain the slightest doubt, however, on the mind of the most fastidious inquirer, in the summer of 1832 the author brought up from the egg young females of the species *Epeira calophylla* and *Epeira cucurbitina*, which, when they had arrived at maturity, he treated in the manner described in the preceding cases. In the autumn of the same year these spiders deposited their eggs in cocoons spun for their reception, out of which the young issued in the ensuing spring, having undergone their final metamorphosis in the cocoons.

These experiments, besides effecting the purpose for which they were instituted, served also to supply collateral evidence of the correctness of M. Audebert's observations relative to the capability of the House-spider, *Aranea domestica*, to produce several sets of prolific eggs in succession, without renewing its intercourse with the male; for three females of the species *Agelena labyrinthica* deposited each a second set of eggs, and a female, *Epeira cucurbitina*, laid four consecutive sets, intervals of fifteen or sixteen days intervening, all of which produced young, though these females had not associated with males of their species for a considerable period antecedent to the deposition of the first set of eggs.

MM. Lyonnet and Treviranus, with other skilful zootomists, have fallen into the error of mistaking the superior spinning mammulæ of spiders, when triarticulate and considerably elongated, for anal palpi (*palpes de l'anus*), denying that they perform the office of spinners, in consequence of their having failed to detect the papillæ from which the silk proceeds; and in this opinion they are followed by the most distinguished arachnologists of the present day. The author is inclined to attribute this singular oversight to the peculiar disposition and structure which the papillæ or spinning tubes connected with the superior mammulæ, when greatly elongated, frequently exhibit. Arranged along the under side of the terminal joint, they present the appearance of fine hairs projecting from it at right angles; but if the spinners when they are in operation be carefully examined with a powerful magnifier, the function of the hair-like tubes may be ascertained without difficulty, as the fine lines of silk proceeding from them will be distinctly perceived.

In conducting this observation Mr. Blackwall usually employs the *Agelena labyrinthica* of M. Walckenaër: its size, the length of its superior mammulæ, and its habits of industry, afford a combination of advantages comprised by no other British spider.

The purpose subserved by the superior mammulæ, when very prominent and composed of several joints, is the binding down with transverse lines, distributed by means of an extensive lateral motion, the threads emitted from the inferior mammulæ; by which process a compact tissue is speedily fabricated.

The foregoing facts supply a striking exemplification of the importance of connecting physiological researches with anatomical details.

In attempting to drown a small spider, new to naturalists, (which the author has named *Erigone atra*,) for the purpose of taking its dimensions accurately by measurement, he was astonished to find that at the expiration of two days, though it had remained under water the whole of the time, it was as lively and vigorous as ever. This extraordinary circumstance induced him to submerge numerous specimens of both sexes in cold water contained in a glass vessel with perpendicular sides, on the 21st of October 1832, in which situation they continued till the 22nd of November, an interval of 768 hours, without having their vital energies suspended.

He has tried the same experiment with individuals of other species, and some of them have preserved an active state of existence for six, fourteen, or twenty-eight days, spinning their lines and exercising their functions as if in air, while others have not survived for a single hour. It is evident, therefore, from these curious facts, that some spiders possess the power of abstracting respirable air from water; for though in the act of submersion the spiracles are generally enveloped in a bubble of air, yet so small a supply is speedily exhausted, and, indeed, soon disappears.

The external and internal organization of such species of *Araneidæ* as can exist for a long period of time under water deserves to be attentively examined; but those species which the author has observed hitherto are minute, and it would require the hand and eye of an accomplished anatomist, assisted by the most delicate instruments and powerful magnifiers, to effect this desirable object satisfactorily.

On the Reproduction of the Eel. By WILLIAM YARRELL, *F.L.S.*

Sir Humphry Davy, in his "*Salmonia*," considered the mode in which eels produced their young as a problem in natural history not then solved, the more general opinion being that they were viviparous.

The paper commences with a recital of the opinions of various writers on this subject, from Aristotle and Pliny to the time of Bloch and Lacépède, and the author states his belief that the viviparous nature of eels had been inferred from the circumstance of their being subject to numerous intestinal worms, three species of which are named and described as of frequent occurrence.

The sexes are distinct; the females oviparous. The situation, structure, and peculiarities of the sexual organs are described, and the author gives a statement, from his own examinations, of the dates at which eels in various ponds and rivers in different southern counties deposited their ova and milt, all of which occurred between the 15th of April and the 7th of May.

The migration of adult eels in autumn, in tide rivers, is considered as extending to the brackish water only, and believed to be induced by the higher degree of temperature there existing. The mixed water is shown by experiment to maintain a temperature two degrees higher than the pure sea or fresh water, from the combination of two fluids of different densities.

Eels pass the winter imbedded in mud.

The return of adult eels is shown by the habits and success of the basket fishermen in rivers within the tide-way, who place the mouths of their eel-pots up stream in autumn, and down stream in the spring.

The ascent of the fry is described as it occurs in the Thames, the Dee, the Severn, and the Parret.

Sea water contains a much larger proportion of earthy matter, and in consequence less air, than the water of rivers, and fresh water also yields its oxygen much more rapidly than that of the sea; the author states his belief that no instance of a freshwater fish going to the sea to deposit its spawn will be found, while more than twenty species of truly marine fishes ascend rivers to deposit their spawn, obtaining thereby, for the vivification of the ova, the assistance afforded by a larger quantity of oxygen.

The restlessness of eels during thunder-storms, when enormous quantities are taken, is referred by the author to the high degree of muscular irritability known to exist in all animals possessing a low degree of respiration, with which coexist the power of sustaining privation of air and food, a low animal temperature, and great tenacity of life, all of which eels are well known to possess.

Fishes that swim and take their food near the surface die soon when taken from the water, having a higher degree of respiration and less muscular irritability, compared with those that swim near the bottom; and *vice versâ*.

The paper concludes with descriptions of the characters which distinguish three different species of British freshwater eels.

On the Naturalization in England of the Mytilus crenatus, *a native of India, and the* Acematichærus Heros, *a native of Africa. By* Charles Willcox, *Curator of the Museum of the Portsmouth and Portsea Literary and Philosophical Society.*

Mr. Willcox states that when His Majesty's ship Wellesley was docked at Portsmouth in July 1824, he discovered on the lead of the cutwater and under the keel a great number of Mytili, which, on examination, proved to belong to the species named *M. crenatus.* The Wellesley was launched at Bombay about February 1815, and came into this harbour in May 1816, where she remained for upwards of eight years previously to her being taken into dock.

The same species of Mytilus has, however, within the last twelve months, been found by Mr. Willcox among groups of *Mytilus edulis,* on the fore part of the keel of several ships on being taken into dock, which proves their naturalization in a climate apparently uncongenial to their nature.

The *Acematichærus Heros* has been found in many parts of His Majesty's dockyard at Portsmouth for some years past. Several specimens are in the possession of Mr. Willcox, and although it was generally supposed that they were bred in African timber, imported for the building and repairs of the navy, yet it was not until the following circumstance occurred that this fact was proved. With the intention of determining this question, Mr. Willcox had for a considerable time been in the habit of examining every piece of African timber which came under his inspection: at length, whilst a piece of this timber was being cut, several small holes, the size of a pea, were discovered running in a direction more or less oblique to the fibres of the wood, and generally increasing to six or seven times their diameter at the orifice, the inside surface being perfectly smooth. Shortly after, a larva was found surrounded by the dust of the wood, and it was carefully extracted from one of these holes. This circumstance encouraged Mr. Willcox to make further search, and he at length succeeded in finding in another hole the pupa, which was taken out alive, and he found it to be that of the insect before mentioned.

The perfect insect was kept alive by Mr. Willcox nine weeks, by feeding it on sopped bread sweetened with sugar. Several of these insects have of late been found in an apparent healthy state, at different parts of the island of Portsea; two of them (male and female) Mr. Willcox has now alive. It may be, perhaps, concluded from this circumstance that this species of insect will ultimately become naturalized in this climate.

Abstract of Observations on the Structure and Functions of the Nervous System. By James Macartney, *M.D., Professor of Anatomy and Surgery in the University of Dublin.*

The author begins by stating the received opinion respecting the structure of the brain, as consisting of two substances; the one an opake white pulp, which is considered to be the nervous matter; the other a coloured substance, in some places inclosing the white, and at other places being imbedded in it.

It has been long known, he adds, that the white substance in many parts assumes the shape of bands or bundles of fibres. Dr. Spurzheim did not hesitate to call these fibres nerves, and was more successful in tracing their course in some parts of the brain than his predecessors had been.

But the author has employed a method of dissecting the brain, which has enabled him to discover that all our former ideas with respect to the structure of the cerebral organ fall far short of the intricacy with which its several parts are combined.

In order to perceive the real structure of the brain, recent specimens are necessary. The sight should be aided by spectacles of a very high magnifying power; and as the different parts are exposed in the dissection, they should be wetted with a solution of alum in water, or some other coagulating fluid. By these means it will be observed that all the white substance, whether appearing in the form of bands, cords, or filaments, or simply pulp, are composed of still finer fibres, which have a plexiform arrangement, and that all those fibres, to the finest that can be seen, are sustained and clothed by a most delicate membrane. By the same mode of dissection, also, it is possible to make apparent the existence of still finer interwoven white fibres in all the coloured substances of the brain, in many of which the nervous filaments are so delicate and transparent that they are not visible until in some degree coagulated by the solution of alum or by spirits.

Dr. Macartney has thus been enabled to see twenty-six plexuses not hitherto described in the brain, the fibres composing which assume two arrangements, the one reticular, the other arborescent.

The membrane mentioned as pervading the entire substance of the brain, and supporting its delicate organization in every part, has heretofore escaped the observation of anatomists, and yet when the fact is declared, we at once perceive that such a membrane must exist. It cannot be supposed that a mass of the magnitude of the brain, and possessing so definite an organization, should form an exception to the fabric of all the other parts of the body, and be left unprovided with a membranous support. This membrane is analogous to the cellular membrane; and if we admit that the filaments of the brain are similar to the fibres in other parts of the nervous system, we may consider the membrane which sustains and connects the cerebral plexuses as their proper musclema.

The pia mater, or vascular integument of the brain, is composed of two layers; the external of which passes over the convolutions of the cerebrum and the gyri of the cerebellum, and the internal is reflected between these forms, and gives all their exterior surface an intimate covering. The blood-vessels seen on the brain are inclosed between these layers, and are conducted on the inner layer to the substance of the organ. The inner portion of the pia mater is continuous with the membrane of the substance of the brain, but becomes so delicate on entering the structure of the organ that it is readily detached from the brain without apparently injuring the integrity of its surfaces. When the inner layer of the pia mater is obtained in connexion with a portion of the vessels and membrane which penetrate the brain, it has the appearance of tufts or shreds, and as such has been described by Ruisch and Alkenus under the name of tomentum cerebri.

The musclema of the brain appears in the adult to be only furnished with colourless vessels, except in those places where red vessels are seen to pass into the substance of the organ; but in the fœtus, the coloured substance of the convolutions may be injected so as to appear quite red. This fact is consistent with the structure of many other organs during fœtal life, which in that period of existence receives red injection, yet only admits afterwards colourless fluids. The great degree of vascularity in the fœtus is particularly remarkable in the eye, the lining of the labyrinth of the ear, the periosteum, &c.

The author has ascertained that the actual quantity of the sentient substance existing in the brain and other parts of the nervous system is extremely small. The bulk of these parts is not materially diminished by removing their nervous matter, provided their membranous structure be left behind; and whenever we meet with the sentient substance in connexion with a highly attenuated membrane, as in the retina and in several of the cerebral plexuses contained in the coloured matter of the brain, it is absolutely invisible until it has undergone some degree of coagulation. It is, perhaps, not assuming too much from these facts, to suppose that the whole nervous system, if sufficiently expanded, and divested of all coverings, would be found too tender to give any resistance to the touch, too transparent to be seen, and probably would entirely escape the cognizance of all our senses. Consistently with this view of the matter, the author thinks that we can hardly take upon us to say that the simplest animals, and even plants, may not have some modification of sentient substances incorporated in their structure, instead of being collected, as in the higher classes of animals, into palpable membranous cords and filaments.

The term plexus has been generally employed to signify an interweaving or crossing of filaments; but Dr. Macartney is satisfied that there is an actual union or intermixture of substance in both the plexuses of the brain and of the other parts of the nervous system. He has discovered that the roots of the spinal nerves, instead of being connected with the medulla by mere contact or insertion, as hitherto supposed, actually enter into the composition of the filaments of the spinal marrow, and that these roots of nerves (as they are called) form communications with each other within the substance of the medulla. With regard to the cerebral nerves also, it can be shown that they are continuous with the cerebral plexuses in their immediate neighbourhood.

Many of the communications formed between the right and left sides of the nervous system are well known, such as the commissures of the brain, the crossing white filaments of the spinal marrow, the decussation of the pyramids, and the interchange of the two optic nerves in fishes. The author has found so many communications to exist between the origins of the nerves on the right and left sides of the body, that he is disposed to believe it to be a general fact. The optic nerves in the human subject do not decussate, as some have supposed, but form a very intricate plexus where they come into contact. This mode of conjunction accounts for atrophy of the tractus opticus being in some instances found on the same side, and at others on the opposite side to that of the eye affected with blindness.

The facts already observed would justify the opinion that the sentient substance is in no place distant or isolated; that it is essentially one and indivisible; and consequently the nervous system differs from all the other systematic arrangements in nature.

It appears to the author that this view of the sentient system will alone serve to explain the numerous sympathies which exist in animal bodies, the occurrence of disease in the higher orders of animals from indirect or remote impression, and the operation of all remedies which act through the medium of the sensibility.

The mode in which the sentient substance is arranged, its more or less minute subdivision, and the degree of arterial vascularity, determine the phænomena of sensibility as they come under our observation. Hence, we find that the brain, even different parts of it, the spinal marrow, the trunks of the nerves, and their sentient extremities, are so differently endowed, that we might be almost led into the error of supposing them all composed of different materials.

It is well known to surgeons and to experimental physiologists, that the brain is not endowed with any *feeling*, in the common meaning of the word. It may be wounded without any sense of pain to the individual.

The trunks of the nerves not possessing the arrangement of the sentient substance suitable to common sensation can only transmit the feeling of pain. Thus, patients after amputation often complain of pain in the part that has been removed; but the author believes that in no instance have they experienced natural or agreeable sensations, or have expressed a consciousness of the presence of the removed limb unattended with pain.

The sentient extremities of nerves are alone capable of being affected by narcotic poisons. Half a tea-spoonful of the essential oil of almonds introduced into the substance of the brain of a rabbit did not produce the least effect on the animal, nor was any effect produced by placing the end of the sciatic nerve in a spoonful of the essential oil of almonds during half an hour, although the animal was afterwards killed in the usual manner by a few drops of this liquid on the tongue.

Impressions on the extremities of nerves sent to the organs of sense and to the external surfaces of the body are attended with consciousness in the individual, whilst those naturally made on the interior surfaces cause no perception. These surfaces, however, are amply supplied with nerves, and possess a high degree of local sensibility, by which they not only discern mechanic forms, but qualities in food and medicines that the perceptive powers of the individual cannot distinguish. These internal and unperceived sensations are continually though secretly influencing the condition of the whole nervous system, and are often the cause of remote morbid actions. Under some circumstances movement follows impression made on the *external* parts of the body after consciousness has become extinct. It is known that the ordinary actions of the iris correspond with the impressions of light on the retina; and the author has observed that the iris continues to move under the same law after the animal's head has been cut off, or the eye taken out, as long as the retina retains its local sensibility: similar effects take place in other parts of the body.

The mutual influence of the nerves and spinal marrow seems to be all that is necessary during fœtal life, as the absence of the brain in the acephalous fœtus does not interfere with any of the functions of the creature until the moment of birth.

The offices which the coloured substance performs in the nervous system have been matter of speculation with anatomists. One obvious purpose of its existence is to give support and security to the finest subdivisions of the sentient substance; we therefore find that it affords such protection in proportion to the necessity: hence, in the brain, the coloured substance is soft and tender, while in the ganglia of the nerves it is generally dense and firm. Besides, however, forming a nidus for the ultimate plexuses of the sentient matter, the coloured substance would seem to fulfill some other use not yet ascertained, as wherever it exists it exhibits the same character with respect to colour, varying from yellow to green or brown. Dr. Macartney considers the yellow spot in the retina of the human eye, and in that of the monkey and lemur, as a ganglion, having discovered that it contains a more intricate reticulation of the nervous filaments than exists in the other parts of the retina.

The coloured substances of the nervous system in no degree derive their peculiar tints from the blood that circulates in them, since the colours are palest in the fœtus, and grow darker as the nervous system approaches its perfect organization.

It is a generally received opinion that the ventricles of the brain are cavities or hollow spaces containing some liquid. This error has arisen from the common modes of dissecting the brain, which necessarily separate the surfaces of the ventricles from each other. If, however, the dissection be performed without disturbing the natural position of the parts, not the slightest appearance of cavity or interspace presents itself. The sole use of the ventricles, therefore, seems to be, merely to gain an extent of surface necessary to the development of the peculiar organization of the brain. Apparently there is less superficies in proportion to the magnitude of the mass of the brain in man than in that of animals; but if we calculate the depth of the surfaces between the convolutions of the cerebrum and on the branches of the arbor vitæ in the cerebellum, together with the internal surfaces, we shall find that the superficies of the human brain is greater in relation to its bulk than that of any other animal. In addition to the surfaces already known, Dr. Macartney has ascertained the existence of ventricles (so called) in the bulb of the olfactory nerves, and in the optic thalami of the human adult brain. In the thalami the distinction of surface is obscure, but in the olfactory tubercles it is sufficiently plain.

The author concludes with stating his belief that every assemblage of the nervous filaments in the form of plexus is destined to fulfill an especial purpose, and with the anticipation that at no distant period we shall be able to understand many of the phænomena of sensation which have been hitherto veiled in the utmost obscurity.

Abstract of Observations on the Motions and Sounds of the Heart. By Hugh Carlile, *A.B., Demonstrator in the School of Anatomy in the University of Dublin.*

The circumstances in the history of the heart's action which have been most the subject of controversy within late years may be enumerated as follow:—1st, the expansion and contraction of the auricles and ventricles, commonly called their 'systole' and 'diastole'; 2nd, the beat of the heart against the side of the chest; 3rd, the arterial pulse; and 4th, the sounds perceptible during the heart's motion. With a view to the explanation of these phænomena the author has made some experiments on living animals, the results of which he was desirous of communicating to the Association.

In experiments of this kind it is desirable, as well for ensuring the means of accurate observation as for the sake of humanity, to diminish as much as possible the suffering of the animal. This can be accomplished by the use of the artificial respiratory apparatus, the animal having been suddenly deprived of sensation without shedding its blood. But the author has found that the application of this apparatus causes the heart to continue and terminate its motions in an unusual manner, and is therefore liable on some points to mislead the observer. In those cases in which the employment of artificial respiration is not expedient, there is much advantage in using very young animals for experiment. In this stage of life, as well as in animals of the inferior classes, the different organs appear to have a comparatively independent existence; and as their functions are in many instances performed with little disturbance under serious injury to the individual, they also retain their vitality long after their separation from the rest of the system. From the same causes very young animals appear to suffer less pain during experiment than those of mature age.

After discussing the methods of experimenting, the author proceeds to describe the opinions which have been held by other persons on the subjects in question, and to compare them with the conclusions to which his experiments have led.

1st. It has been asserted by Bichat, and his celebrity has induced many to adopt the opinion, that the ventricles possess a power of active dilatation, by means of which, when their systole has terminated, they are enabled to invite into their cavities the blood from the neighbouring auricles. The author, however, has ascertained by experiment that there is no such dilating power in the ventricles, but that these muscles, when their state of contraction has ceased, become perfectly soft and flaccid, like all other muscles in their state of repose, and thus readily admit the blood from their respective auricles, which had become distended during the systole of the ventricles. The feeling of resistance which was mistaken by Bichat for a dilating power, and was supposed by him to accompany the diastole of the ventricles, the author has ascertained to be caused by the swelling of their muscular fibres during their systole.

The auricles contract but little upon their contents in man and in the higher classes of animals, the small quantity of blood which the ventricles discharge at each contraction being compensated by the frequency of their movement; while in the cold-blooded animals, in which the heart acts with less frequency, the degree of expansion and contraction of both auricle and ventricle is much greater than in the former classes, and the quantity of blood sent through the heart at each movement is much larger.

2nd. The impulse of the heart against the side of the chest, commonly called its beat, has been explained by different writers in various ways. Mr. Hunter supposed it to have been caused by the straightening of the curve of the aorta during the systole of the ventricles, whereby the apex of the heart was thrown forwards. Meckel refers it, in part, to the elongation of the arterial tubes during the ventricular contraction, and partly to the swollen state of the auricles at that time, by which the ventricles are pushed forward against the side of the chest. Harvey mentions an opinion held by some in his time, and which has been lately revived, namely, that the beat is caused, not by any active power in the ventricles, but by the muscular contraction of the auricles during their systole, by which the blood being sent with force into the ventricles, distends their cavities, and causes them to strike against the chest. This opinion, therefore, supposes the beat of the heart to coincide with the ventricular diastole. Various other suppositions have been put forward upon this subject by different authors.

The author's experiments show that the beat of the heart is coincident with the systole of the ventricles, and is caused by the peculiar shape which these parts acquire in their contracted and hardened state, their middle part becoming globular and prominent, and their apex being, as Hunter expressed it, 'tilted' forward. During their systole the ventricles, like other muscles in a state of contraction, become swollen and hard to the touch, as was observed long since by Harvey. The greatest quantity of muscular fibre being situated about their middle part, where the 'musculi papillares' are placed, this part during the systole assumes a globular and prominent form, projecting in front, and by its protuberance behind pushing forward the body of the ventricles. The apex is '*tilted*' forward for the following reason. The author has ascertained, by unravelling the structure of hearts prepared by boiling, that the fibres which pass from the base to the apex, on the front of the ventricles, are considerably longer than those similarly placed behind. In some human hearts he has found them in the ratio of five to three; the shape of the ventricles being nearly that of an oblique cone, whose base is applied to the auricles, and whose longest side is in front. Now it is a law of muscular action that fibres are shortened during their contraction in proportion to their length when relaxed. For instance, if a fibre one inch long lose by contraction one fourth of its length, or one quarter of an inch, a fibre two inches in length will lose *one* inch by a contraction of equal intensity. We have seen that the fibres which by their contraction cause the apex to approach the base of the ventricles, are much longer on the front than on the back part, and, consequently, the former are more shortened during their contraction than the latter. The apex, then, does not approach the base in the line of the axis of the ventricles, but is drawn more to the side of the longer fibres, that is, towards the front, thus producing the 'tilting' forward.

This conclusion is strengthened by the fact that the forward motion of the apex of the ventricles is always proportioned to the obliquity of the form of these cavities in different classes of animals. In the heart of some reptiles, the frog for example, in which the lengths of the fibres of the ventricle before and behind are nearly equal, the tilting of the apex is scarcely discernible. The obliquity is greater, as far as the author has been able to observe, in the human heart than in that of any other animal.

Mr. Carlile has ascertained, also, that the ventricles assume this form during their contraction, after they have been separated from the auricles by a ligature, and even after they have been removed from the body, and placed in a vessel of tepid water, or held upon the hand, the auricles having been previously cut off; in all which cases the peculiar motions which accompany their contraction and relaxation were observed to recur as long as their power of moving remained; proving that the beat of the heart is produced altogether by the action of the ventricles during their systole, and that in these, as in all other muscles, the peculiar forms assumed during their contraction depend upon the relation, as to length and position, of the fibres of which they are composed.

3rd. The arterial pulse, which is produced by the jet of blood sent from the left ventricle into the aorta during its systole, has been stated by Bichat and many other writers to be synchronous throughout the whole arterial system. But the experimenter can ascertain in his own person that the pulse is successive at different distances from the heart. If the hand be placed over the region of the heart, and the radial artery be felt at the same time, an interval will be distinctly perceptible between the beat and the pulse; and if the anterior tibial artery be substituted for the radial, the interval will be found still greater. Repeated observations of this kind show that the intervals of time between each beat of the heart and the corresponding pulse in different parts of the body are proportioned to the distances measured along the arteries, from the heart to the respective parts; and a knowledge of this fact leads, without further anatomical inquiry, to the conclusion that the beat of the heart is coincident with the ventricular systole. For, as the intervals of time between the beat and pulse are proportioned to the distances from the heart to those parts where the pulses are felt, it follows that when the distances become evanescent the intervals of time will also vanish. Consequently, at the origin of the aorta the pulse will coincide as to time with the beat of the heart; but the pulse at the origin of the aorta is necessarily synchronous with the ventricular systole, by which the blood is driven into that artery; and therefore the beat of the heart will coincide with the ventricular systole, a conclusion which agrees with that drawn from positive experiment.

The proportion which exists in the pulse between the intervals and distances is dependent upon the elasticity of the arteries.

4th. An explanation of the *sounds* of the heart has become necessary since the employment of the stethoscope in ascertaining the state of internal parts. Laennec has well described these sounds, and properly refers the first to the rush of blood from the ventricles during their systole. But, in supposing that the second sound is produced by the auricular systole, he has fallen into an extraordinary error, as the second sound follows immediately after the first one, whereas the auricular systole precedes the ventricular. This mistake has been noticed by different writers since Laennec's time, who have rejected his explanation, and substituted others in its place.

From the observations which the author has made, he has no doubt that the second sound is caused by the obstacle which the semilunar valves present to the passage of the blood from the arteries back into the heart, at the termination of the ventricular systole.

At each contraction of the ventricles a quantity of blood is driven by them into the trunks of the arteries, which, being already full, accommodate the addition to their contents by a lateral expansion of their parts nearest to the heart. When the systole of the ventricles is at an end, the elastic force of the arteries, acting upon their contained blood, drives it towards the heart, its entrance into which is prevented by the sudden closing of the semilunar valves: and thus a shock is communicated to the front and upper part of the ventricles, and to the adjacent trunks of the arteries, which may be heard by the ear placed over the region of the heart. The relation, as to time, which the second sound has to the first, its abrupt character, and its coincidence with the end of the ventricular systole, have led the author to adopt the foregoing opinion.

Mr. Carlile then described the experiments from which the greater number of the preceding conclusions have been drawn, and having detailed the circumstances of some made upon living subjects, proceeded to relate those which follow.

1. Artificial respiration having been established in a rabbit which had been strangled, and the heart having been exposed, the following observations were made.

The finger being applied successively to the front, back, and each side of the ventricles, conveyed the sensation of hardness and impulse when the ventricles assumed the globular form, and of softness and flaccidity when they became flattened and expanded. The end of a probe being laid on the front surface of the ventricles, was raised nearly a quarter of an inch during the former of these states, and sank, causing a slight depression on the surface, in the latter. The probe was more elevated when placed on the middle point of the surface, or on the front of the apex, than when placed elsewhere.

The right wing being held aside, so as to admit of the right auricle being seen, this was observed to swell during the continuance of the ventricles in their hardened state, and to diminish its size from the instant in which their flaccidity commenced, its degree of contraction being, however, inconsiderable. The contraction of the appendix was preceded by that of the rest of the auricle, and followed by the instantaneous movement and hardening of the ventricles. The contraction of the different parts of the auricle was successive, commencing at the venæ cavæ, and terminating at the appendix, of which last the contraction was much more sudden and distinguishable than that of any other part.

The heart in this subject continued to beat for an hour, when the motions in all its parts ceased, and nearly at the same time; both auricles and both ventricles remaining distended, soft, and full of blood. The heart, separated from the body, was thrown into tepid water, where it remained, soft, and without motion, and had lost the power of contracting itself.

2. A rabbit having been strangled, the heart was exposed while still beating. In about 10 minutes the left ventricle ceased to move, and had contracted itself firmly. In a minute or two afterwards all motion was at an end in the left auricle, which was also contracted. The right ventricle continued its movements for 45 minutes, and during its contraction the apex of the heart was drawn a little to the right side. The right auricle continued to possess motion for an hour and three quarters; and for the last 20 minutes its contraction proceeded slowly, and with a motion apparently vermicular, over its surface; always commencing at the part contiguous to the venæ cavæ, and ending at the appendix. The right auricle and ventricle contained each some blood when their motions ceased; but, the heart having been thrown into tepid water, they gradually expelled their contents, assuming, as those of the left side had done, a firm and contracted state.

The difference of the states in which the hearts were found, after their motions had ceased, in the last two experiments, is remarkable, and appears to admit of the following explanation. In the last experiment, in which no means were employed to continue respiration, the left side of the heart soon ceased to move; because a continuance of the functions of the lungs, as proved by the experiments of Bichat, is necessary to the maintaining of its actions. The firmness of its contraction shows, that although its ordinary motions had ceased, it still retained a considerable share of organic life, as it is known that muscles, whose vitality is quite extinct, have no power of contraction. In the experiment in which respiration was artificially maintained, the left side of the heart continued to beat for an hour, the sustained function of the lungs affording to it a motive for prolonged action; but having been deprived of the influence which the central parts of the nervous system extend to organs in vital connexion with them, its powers of life were exhausted by the long continuance of its motions, and when these ceased, it was quite dead, and incapable of a vital contraction. The right side of the heart in the last experiment seems to have participated in the exhausted state of the left side, because its motions had been performed with much more energy during their continuance than would have been the case had not respiration been artificially maintained.

On the Mechanism and Physiology of the Urethra. By Henry Earle, *F.R.S., Professor of Anatomy and Surgery to the Royal College of Surgeons.*

The author, having been lately engaged in delivering a course of lectures on the anatomy and diseases of the urinary organs, was led to prosecute his inquiries into the minute structure of the urethra, and to avail himself of the aid of comparative anatomy to elucidate the subject. The results of this inquiry he related briefly to the Section, with a view of reconciling some of the discordances of opinion at present existing, and of explaining the double functions of the organ.

On the Nomenclature of Clouds. By —— Burt.

In the course of some meteorological observations, Mr. Burt found the variations in the forms of clouds to be so numerous, that it was difficult, by the use of Mr. Howard's nomenclature, to describe them with sufficient accuracy.

In consequence, he suggests the propriety of defining the leading sections of clouds by peculiarities of their external constitution, and of characterizing the minor divisions by the external forms of the masses.

On the peculiar Atmospherical Phænomena, as observed at Hull during April and May 1833, *in relation to the prevalence of Influenza. By* G. H. Fielding, *M.R.C.S.L.*

The author observes, that the true causes of epidemic diseases being for the most part unknown, all the unusual circumstances which occur during their prevalence, especially if these be capable of estimation by exact comparative measurements, should be carefully recorded. The value of meteorological observations, as tending to determine the most important of the variable conditions of this interesting problem, is insisted on, and the results of his own observations are presented as proving that the state of the atmosphere during the period of the prevalence of influenza at Hull in 1833 was extraordinary. The following are the numerical results.

	1832.			1833.		
	April.	May.	Diff.	April.	May.	Diff.
Mean pressure of the air .	30·063	29·989	0·08	29·799	30·177	0 38
dew point	40·006	40·808	0·80	37·823	45·253	7·43
temperature in shade	46·674	49·767	3·09	44·706	55·393	10·69
temperature in sun .	68·348	72·865	4·52	7·251	84·122	16·97
max. temp. in shade	53·933	57·809	3·87	51·310	63·690	12·38
min. temp.	39·416	41·725	2·31	38·103	47·096	8·99
Quantity of rain in inches	3·820	2·240	1·58	4·530	0·600	3·93

From the columns of differences it will be seen how much more sudden and violent in all respects was the transition from April to May in 1833 than in 1832. The number of hours in which the sun thermometer, which has a blackened bulb, could be used was, in April, 81; in May, 158. The winds in April were easterly at the beginning and end, W., S.W., and N.W. in the middle; in March generally S., varying to the E. and W. Rainy days in April, 23; in March, only 2.

The 16th of May is particularly mentioned as affording a remarkable instance of contrast between the years 1832 and 1833. In 1833, during 14 hours, the thermometer in the shade averaged upwards of 70°; during 8 hours nearly 75°; from 2 to 3 P.M. 77°. The thermometer in the sun for 19 successive hours was upwards of 90°. Minimum temperature of the following night 49°. Range of temperature in the sun 47°·5; in the shade 28°. In 1832, during 13 hours, the thermometer in the shade averaged rather more than 48°; in the sun at 3 P.M. the thermometer reached 62°·8; at 2 and 3 in the shade 50°·8 and 51°, which was the maximum. Minimum of the following night 33°.

In conclusion the author states that he does not offer these data as affording a complete explanation of the prevalence of influenza, but remarks that it is difficult to imagine otherwise than that such sudden changes from cold to heat, from wetness to dryness, from midday heat to cold evening fogs, must have had a very decided and general influence on the health of the human body.

IV. HISTORY OF SCIENCE.

A short Account of some MSS. Letters (addressed to Mr. Abraham Sharp, relative to the Publication of Mr. Flamsteed's Historia Cælestis,) laid on the table, for the inspection of the Members of the Association. By FRANCIS BAILY, *V.P.R.S., President of the Royal Astronomical Society.*

THESE letters belonged to the late Mr. Abraham Sharp, and were found some years ago in a box deposited in an old lumber. room, filled with various books and papers, which had been considered as of so little use that they were frequently taken out by the servants to light the fire, and were otherwise destroyed and lost. The present collection of them, which was preserved from such destruction, consists of above 120 letters from the celebrated Flamsteed, and of about half that number from Mr. Crosthwait, his assistant at the Royal Observatory, all addressed to Mr. Sharp, who at that time lived at Little Horton, in Yorkshire, on an estate of his own. It is probable that these are the letters alluded to in the life of Mr. Sharp, inserted in Dr. Hutton's *Mathematical Dictionary*, the particulars of which, however, have never yet been made public. They are now the property of a relation of the late Mr. Sharp, residing in London, by whose permission they are exhibited for inspection.

It is well known that Mr. Sharp divided the mural arc that was erected at Greenwich for Flamsteed's use, and that he was for some time the assistant there. He afterwards retired to his estate at Little Horton, where he lived a very secluded life, passing most of his time in astronomical calculations. Flamsteed employed him to compute the places of several of the stars in his Catalogue, from the original observations; and an extensive and friendly correspondence was kept up between them till the time of Flamsteed's death, and was afterwards continued with Mr. Crosthwait, who superintended the printing of Flamsteed's works. This correspondence embraces a variety of subjects; but the principal, the most novel, and the most interesting is the account of the repeated difficulties and impediments which delayed and almost prevented the printing of the *Historia Cœlestis*.

The date of the first letter, in the present collection, is February 6th, 1701–2, at which time it appears that Flamsteed was preparing to publish his work, which was not completed till twenty-four years afterwards, being six years after his decease. He commenced the publication at his own cost and risk; but after he had expended a considerable sum of money, the subject was mentioned to Prince George of Denmark, who undertook to defray the expense of bringing out the work: and here his troubles began; for, in the first place, the Prince declined the publication of the maps, which Flamsteed considered the most important part, and such as, in his opinion, would tend most to the "glory of the work;" and secondly, the committee of the Royal Society, to whom the superintendence of the business was intrusted, appear, from the whole tenor of these letters, to have thrown every obstacle in the way to prevent the progress of the printing. It is not directly stated who were the members that formed that committee, but it is evident from the correspondence that Newton and Halley formed a part of it; and Flamsteed can never touch on this subject (and it forms a prominent portion of his letters,) without expressing his opinion, in no very courteous language, of their unfriendly and hostile conduct towards him.

It was in 1704 that the Prince offered to undertake to defray the expenses of the printing; but so many impediments were thrown in the way (oftentimes frivolous and vexatious,) that it was not till the end of the year 1707 that the first volume only, the least interesting part of the work, was completed. Before the second volume was commenced, the committee required Flamsteed to deposit in their hands a duplicate copy of the *Observations*, as well as of his *Catalogue*, which he accordingly did, *sealed up*. New causes for delay, however, were brought forward, and before the second volume was sent to the press Prince George died. During the whole of this time Flamsteed had received only £125 towards the expenses of the work; and as he saw no prospect of any further support from Government, he resolved to wait for better and more favourable times.

He then demanded from the committee the return of the manuscript *Observations* and *Catalogue* which he had deposited in their hands, which request they appear to have refused.

The breach was now complete, and the subsequent letters are filled with complaints of the conduct of the committee; and Flamsteed eventually commenced legal proceedings against Sir Isaac Newton for the restitution of the MSS. But it is principally on Dr. Halley that the force of his indignation falls; and if the circumstances referred to in the letters be correct, (of which there does not seem to be any doubt, although the motives of the parties may have been misinterpreted,) Flamsteed had just cause for complaint and redress; for he charges Halley, in direct terms, with having surreptitiously purloined the manuscript *Observations* and *Catalogue* deposited with the committee, and with having published them in a garbled and incorrect manner. It is acknowledged that the seals were broken; but it is pretended that this was done by an order from the Secretary of State, for what purpose, however, does not appear. It is notorious that Halley did publish an edition of Flamsteed's *Catalogue*, and extracts from his *Observations*, in the year 1712, which is the work alluded to by Flamsteed; and as Flamsteed could never recover back the MSS., there is no doubt that these were the documents made use of. In fact, the matter is not disguised by Halley, in the preface. Flamsteed remonstrated against this conduct; calls Halley "a malicious thief," and bestows on him other opprobrious epithets. In the year 1716, Flamsteed obtained an order from the King to have the remaining (unsold) copies of this work delivered up to him, for the purpose of being destroyed: 300 copies were consequently sent to the Observatory, which, he says, he "sacrificed to truth"; and he appears to have missed no opportunity of destroying every copy that came into his possession. Such is Flamsteed's history of the edition of 1712.

During all this time, no further progress had been made in printing the *Observations*. The first volume only was completed, but this did not contain any of the observations made with the mural arc at Greenwich; the second, which was to commence with those observations, was not yet begun. Flamsteed, however, had printed, for private circulation only, a correct copy of his *Catalogue of Stars*, to counteract the effect of Halley's spurious edition; but no steps had been taken towards forwarding the main work, which had now lain dormant upwards of ten years, and which was much increased by the numerous observations made during that period. At length, not being able to regain possession of the MSS., he was obliged to copy them again from the original entries, which was a great trouble and expense to him; and towards the end of the year 1717, he sent the first sheet of the second volume to the press; resolved to proceed in the work at his own cost. Before his decease, which happened on Dec. 31st, 1719, he had completed that volume, having been occupied nearly nineteen years in the prosecution of the work, struggling with difficulties of various kinds, and thwarted and opposed in various ways. It is to his perseverance and public spirit, supported afterwards by the gratuitous exertions of Mr. Sharp and Mr. Crosthwait, that we are indebted for the *British Catalogue*, and for that vast mass of observations made at the Royal Observatory, which are still of use in various branches of astronomical research, and which will render his name illustrious as long as the science exists.

The correspondence of Mr. Crosthwait relates principally to the difficulties, impediments, and delays which still prevented the work from being brought to a final conclusion; and it may be safely stated, that had it not been for the extraordinary exertions of Mr. Sharp and Mr. Crosthwait, the whole would never have been completed. The Catalogue was reexamined and compared with the observations, and afterwards reprinted with several amendments. The preface cost him much trouble: it was required to be translated into Latin, but no one could be found adequate to the task, though repeatedly attempted. Mr. Pound undertook it, but eventually declined it; and it was at last accomplished by a Dissenting minister. The third volume was at length finished, and the whole work published in 1725, six years after Flamsteed's death.

There remained now only the maps, the construction and engraving of which appear to have cost as much trouble and vexation as the letter-press. Only one of them was finished (Orion) when Flamsteed died. For the rest we are indebted to Mr. Sharp, who constructed them anew, according to Flamsteed's principles, from the Catalogue. Sir James Thornhill drew the figures of the constellations, and recommended engravers for the work; but the charges of the English artists were considered so enormous, that Mr. Crosthwait went over to Holland for the express purpose of engaging some of the best Dutch engravers to complete the work. The vexatious delays which necessarily occurred by adopting this method, its increased expense, and the constant attention requisite to prevent mistakes, dispirited Mrs. Flamsteed, and a temporary stop was consequently put to the work, although Mr. Sharp (now much advanced in years) and Mr. Crosthwait were willing to continue their services. At length, some English engravers being found who offered to execute the maps at a more moderate expense, the labours of these gentlemen were renewed, and continued till the time of Mrs. Flamsteed's death, which took place on July 29th, 1730.

Here the correspondence ceases, probably on account of the circumstances mentioned in the last letter, whereby it appears that Mrs. Flamsteed did not leave either Mr. Sharp or Mr. Crosthwait a single farthing for all their services; neither had they received any remuneration since Mr. Flamsteed's death for their unparalleled exertions in her behalf.

*** Since the above statement was written, Mr. Baily has discovered amongst Flamsteed's manuscript papers, deposited at the Royal Observatory at Greenwich, all the *Answers* of Mr. Sharp to the above letters of Flamsteed; thus constituting a complete correspondence between the parties for nearly eighteen years.

CORRIGENDUM IN PROFESSOR POWELL'S PAPER.

In the abstract, given in the Proceedings of the Physical Section, of Professor Powell's paper a formula is introduced (p. 377) which the author finds, since the paper was printed, is incorrect; this however does not affect the rest of the paper: but the whole will shortly appear in detail in another form.

RECOMMENDATIONS

OF

THE BRITISH ASSOCIATION

FOR THE

ADVANCEMENT OF SCIENCE.

THE following Reports on different Branches of Science have been drawn up, at the request of the Association.

Vol. I.

On the progress of Astronomy during the present century, by by G. B. Airy, M.A., Plumian Professor of Astronomy and Natural Philosophy, Cambridge.

On the state of our knowledge respecting Tides, by J. W. Lubbock, M.A., Vice-President and Treasurer of the Royal Society.

On the recent progress and present state of Meteorology, by James D. Forbes, F.R.S., Professor of Natural Philosophy, Edinburgh.

On the present state of our knowledge of the Science of Radiant Heat, by the Rev. Baden Powell, M.A., F.R.S., Savilian Professor of Geometry, Oxford.

On Thermo-Electricity, by the Rev. James Cumming, M.A., F.R.S., Professor of Chemistry, Cambridge.

On the recent progress of Optics, by Sir David Brewster, LL.D., F.R.S., &c.

On the recent progress and present state of Mineralogy, by the Rev. Wm. Whewell, M.A., F.R.S.

On the progress, actual state, and ulterior prospects of Geology, by the Rev. Wm. D. Conybeare, M.A., F.R.S., V.P.G.S., &c.

On the recent progress and present state of Chemical Science, by James F. W. Johnston, A.M., Professor of Chemistry, Durham.

On the application of Philological and Physical Researches to the history of the Human Species, by J. C. Prichard, M.D., F.R.S., &c.

Vol. II.

On the advances which have recently been made in certain branches of Analysis, (Part I.,) by the Rev. G. Peacock, M.A., F.R.S., &c.

On the present state of the Analytical Theory of Hydrostatics and Hydrodynamics, by the Rev. John Challis, M.A., F.R.S., &c.

On the state of our knowledge of Hydraulics, considered as a branch of Engineering, (Part I.,) by George Rennie, F.R.S., &c.

On the state of our knowledge respecting the Magnetism of the Earth, by S. H. Christie, M.A., F.R.S., Professor of Mathematics, Woolwich.

On the state of our knowledge of the Strength of Materials, by Peter Barlow, F.R.S.

On the state of our knowledge respecting Mineral Veins, by John Taylor, F.R.S., Treas. G. S., &c.

On the state of the Physiology of the Nervous System, by William Charles Henry, M.D., F.R.S.

On the recent progress of Physiological Botany, by John Lindley, F.R.S., Professor of Botany in the University of London.

The following Reports have been undertaken to be drawn up, at the request of the Association.

On the theories of Capillary Attraction and of the Propagation of Sound as affected by the development of Heat, by the Rev. John Challis, M.A., F.R.S., &c.

On the state of our knowledge of Hydraulics, (Part II.,) by George Rennie, F.R.S.

On the present state of our knowledge respecting the connexion of Electricity and Magnetism, by S. H. Christie, M.A., F.R.S., Professor of Mathematics, Woolwich.

On the state of the science of Physical Optics, by the Rev. H. Lloyd, M.A., Professor of Natural Philosophy, Dublin.

On the state of our knowledge respecting the application of Mathematical and Dynamical principles to Magnetism, Electricity, Heat, &c., by the Rev. Wm. Whewell, M.A., F.R.S.

On the recent additions to our knowledge of the Phænomena of Sound, by the Rev. R. Willis, F.R.S., &c.

On the state of our knowledge respecting the relative level of Land and Sea, and the waste and extension of the land on the east coast of England, by R. Stevenson, Engineer to the Northern Light-houses, Edinburgh[1].

[1] Communications of facts relative to this subject are much wanted, and may be addressed to Mr. Stevenson, Civil Engineer, Edinburgh.

On the state and progress of Zoology, by the Rev. Leonard Jenyns, M.A., F.L.S., &c.

On the state and progress of Systematic Botany, by G. Bentham.

On the state of our knowledge respecting the influence of Climate upon Vegetation, by the Rev. J. S. Henslow, M.A., Professor of Botany, Cambridge.

On the state of Physiological knowledge, by the Rev. William Clark, M.D., F.G.S., Professor of Anatomy, Cambridge.

On the state of Pathological knowledge, by John Yelloly, M.D., F.R.S.

RECOMMENDATIONS

OF

THE COMMITTEES,

WITH NOTICES OF DESIDERATA IN SCIENCE BY THE AUTHORS OF REPORTS.

[*The Recommendations adopted at the Cambridge Meeting have an asterisk prefixed.*]

ASTRONOMY.

The Committee for Mathematical and Physical Science stated, that it would tend much to the advancement of astronomy and the art of navigation, if the observations of the sun, moon, and planets, made by Bradley, Maskelyne, and Pond, were reduced.

It was resolved by the General Committee, that a representation to this effect from the British Association be submitted to Government, in the hope that public provision might be made for the accomplishment of this great national object; and that a deputation, consisting of Professor Airy, Mr. Baily, Mr. Davies Gilbert, and Sir John Herschel, be appointed to confer with the Lords of the Treasury on the subject.

The application was immediately complied with by the Government, and an advance of 500*l.* has been made by the Treasury towards the reduction of the observations from the year 1750 to the present day.

Desiderata noticed in Professor Airy's Report, p. 187.

1. Directions for placing a thermometer so as to indicate correctly the Temperature of the Air at the place of observation, for Refraction-corrections, the external and internal temperatures being supposed as nearly as possible equal.

2. Experimental Data for the Theory of Refraction—

What is the law of the decrease of temperature, or of density, in ascending?

How does this vary at different times?

Can any means be contrived for indicating practically at different times the modulus of variation?

Does the refractive power of air depend simply on its density, without regard to its temperature?

Is it well established that the effects of moisture are almost insensible?

Can any rule be given for estimating the effect of the difference of refraction in different azimuths, according to the form of the ground?

When the atmospheric dispersion is considerable, what part of the spectrum is it best that astronomers should agree to observe?

3. An investigation of the coefficient of Nutation from the Greenwich circle-observations.

4. The reduction of Bradley's and Maskelyne's Observations of the Sun and Planets, on a uniform plan.

5. Remeasurement[1] of the elongation of Jupiter's Satellites, to correct the estimate of the mass of Jupiter.

6. Separate investigations, from observations, of the diminution of the aphelion distance and perihelion distance of Encke's Comet, for the purpose of testing the truth of Encke's assumed law of density of the resisting medium.

7. Calculations of the perturbations of Biela's Comet for the interval between 1772 and 1806, and of those of the node and inclination from 1806 to 1826, for the purpose of ascertaining the identity of the comet of 1772, and examining whether this comet gives any indication of a resisting medium.

8. Verification of Burckhardt's formulæ in the *Mémoires de l'Institut* for 1808, and extension of them to terms depending on the inclination.

9. Theory of the perturbations of Pallas, and of Encke's Comet.

[1] Professor Airy himself has since made the required measurements, and given a determination of the mass of Jupiter.

TIDES.

* That a sum not exceeding 200*l*. be devoted to the *discussion* of observations of the Tides, and the formation of Tide Tables, under the superintendence of Mr. Baily, Mr. Lubbock, Rev. G. Peacock, and Rev. W. Whewell.

That the Association should endeavour to procure the general establishment of systematic Tide Observations along the coasts of Great Britain and Ireland, and that the standing Committee on Tides be requested to select such places[1] as may appear to them most important for this purpose; that the direction, and, if possible, the intensity of the wind should be observed, as well as its critical changes after having set for some time in a particular direction; and that the altitude of the currents of air should also be, as far as possible, remarked.

METEOROLOGY.

1. That the Committee in India be requested to institute such observations as may throw light on the horary oscillations of the barometer near the equator.

2. That the Committee in India be requested to institute a series of observations of the thermometer during every hour of the day and night.

3. That a similar hourly register be established at some military or naval station in the South of England[2].

4. That the decrease of temperature at increasing heights in the atmosphere should be investigated by continued observations at stated hours and known heights. The hours of $9\frac{1}{4}$ A.M. and $8\frac{1}{2}$ P.M., as giving nearly the mean temperature of the year, are suggested for the purpose. (See *Report*, p. 218.)

5. That persons travelling on mountains, or ascending in balloons, should observe the state of the thermometer, and of the dew-point hygrometer, below, in, and above the clouds, and determine how the different kinds of clouds differ in these respects. (See *Report on Meteorology*, vol. i. p. 245.)

6. That the temperature of springs should be observed at different heights above the mean level of the sea, and at different depths below the surface of the earth, and compared with

[1] Directions for observing the Tides, extracted from Mr. Lubbock's Report, and Mr. Whewell's Memoranda, are inserted in the Appendix.

[2] Observations in agreement with this recommendation have been commenced at Plymouth and Devonport, under the directions respectively of Mr. G. Harvey and of Mr. Wm. Snow Harris.

the mean temperature of the air and the ground.—Detached observations on this subject will be useful, but a continued and regular series of results for each locality will be more valuable[1]. (See *Report*, vol. i. p. 224.)

7. That series of comparative experiments should be made on the temperature of the dew-point, and the indications of the wet-bulb hygrometer, and that the theory of this instrument should be further investigated. (See *Report*, vol. i. p. 243—246.)

8. That particular attention be paid to the improvement of the instruments of meteorological research.

9. That Mr. Phillips, and Mr. Wm. Gray, jun., be requested to undertake a series of observations on the comparative quantities of rain falling on the top of the great tower of York Minster, and on the ground near its base; and that similar observations be instituted at other places[2].

A standing Committee was appointed, consisting of Professors Airy, Christie, and Forbes, Dr. Dalton, Dr. Robinson, Mr. Potter, and Mr. Scoresby, to draw up instructions[3] for observing Auroras, and to endeavour to establish corresponding observations in every part of the kingdom.

Desiderata noticed in Prof. Forbes's Report.

1. Verification of Dr. Dalton's theory of the constitution of the atmosphere, by direct experiment. (*Report*, vol. i. p. 206; *Phil. Trans.* 1826.)

2. Experiments in various latitudes upon the temperature of the earth at moderate depths, by means of thermometers with long tubes; with a view to determine the position of the "invariable stratum," where external causes cease to produce any effect. (*Report*, vol. i. p. 221.)

[1] The height of the springs may be determined with sufficient accuracy by a common portable barometer.

[2] The observations at York have been made at three adjacent stations of known height, with gauges made on the same mould, and measured by one graduated glass vessel: they have been continued from the 1st of February 1832 to the present time. From the results, it has been inferred by Mr. Phillips that the diminution in the quantity of rain, at the higher stations, has a certain constant dependence on the height of the station, and on the condition of the air as to moisture in the different periods of the year. For the further elucidation of this subject, it is desirable that experiments upon the same plan should be tried in other situations, and especially where the climate is of a different character from that of York; in the humid atmosphere of Cornwall, for example, and in the drier air of the midland counties. Gauges exactly similar to those in use at York will be supplied from thence to persons undertaking to try these experiments, on application to the Secretaries.

[3] An abstract of the directions which have been drawn up by the Committee, is given in the Appendix.

3. Experiments on the solar and terrestrial radiation. (*Report*, vol. i. p. 222.)

4. Observations on the horary oscillations of the barometer, at considerable heights above the sea. This more particularly applies to places near the equator[1].

5. Additional observations to determine what is the influence of the moon on the height of the barometer. (*Report*, vol. i. p. 234. See also Arago, *Annuaire* for 1833.)

6. The application of the hygrometric correction to the barometric formulæ for heights. (*Report*, vol. i. p. 254.)

7. Observations on the phænomena of wind at two stations, at considerably different elevations, (p. 249.) The direction of the wind should be noted in *degrees*, beginning from the south and proceeding by the west.

8. Magnetical observations, regularly conducted, especially with a view to auroral phænomena.

OPTICS.

* That a sum not exceeding £50 be appropriated to the construction of a telescopic Lens, or Lenses, out of rock-salt, under the direction of Sir David Brewster.

Desiderata noticed in Sir David Brewster's Report.

The determination of various *constants*, namely,

1. The refractive indices of the two pencils in all crystallized bodies, measured in reference to definite points of the spectrum.

2. The angles at which light is polarized by reflection from crystallized and uncrystallized surfaces.

3. The inclination of the resultant axes of crystals having double refraction, for different rays of the spectrum.

4. The dimensions of the ellipse which regulates the polarization of metals and their alloys.

5. The circularly polarizing forces of fluids and solutions.

6. The refractive and dispersive powers of ordinary solid and fluid bodies, measured according to the method of Fraunhofer.

7. Experimental determination of the effects of the absorption of light by gases upon the light of the fixed stars. (p. 322.)

[1] Those who may possess such observations, continued for one or more weeks, with observations of the temperatures of the mercury and of the air, and *the probable corresponding temperatures of the air at the level of the sea*, are requested to transmit them to Professor Forbes, Edinburgh. The local position of the point of observation should also be noticed.

MAGNETISM.

1. That a series of observations upon the intensity of Terrestrial Magnetism be executed in various parts of the kingdom, similar to those which have been carried on in Scotland by Mr. Dunlop. (Some experiments, made in consequence of this recommendation, by Dr. Traill, are given in the published Reports of the Association, page 557.)

2. That observations should be made in various places with the Dipping-needle, in order to reduce the horizontal to the true magnetic intensity.

* A standing Committee, charged with promoting these objects, has been appointed, consisting of Professors Christie, Forbes, and Lloyd. The latter gentleman has undertaken to make observations on the magnetic intensity in Ireland, before the next Meeting of the Association.

Desideratum noticed in Prof. Christie's Report.

* A regular series of observations conducted in this country on the diurnal variation of the needle.

ELECTRO-MAGNETISM.

The Committee recommend for further examination the Electro-magnetic condition of mineral veins. (Consult on this subject the paper of Mr. Fox, *Phil. Trans.* 1830.)

RADIANT HEAT.

Desiderata noticed in Professor Powell's Report.

1. Improvement of the means of obtaining accurate indications of small degrees of radiant heat: the thermo-multiplier of MM. Nobili and Melloni to be subjected to examination[1]. (vol. i. p. 297, &c.)

Determination of the following questions (p. 298.):

2. Do the ratios of the conducting powers of substances remain the same for all thicknesses?

3. It is alleged that in certain cases simple heat is radiated *freely and directly* through transparent media: Is it meant that

[1] Professor Forbes gave an account of the performance of this instrument at the Cambridge Meeting.

the manner of its transmission in such cases is strictly analogous to that in which light is communicated; or is it only an *extremely rapid* communication by conduction? What circumstances can be fixed upon to determine our view of the matter?

4. Taking into account the thickness, state of surface, &c., of a body exposed to radiant heat, does any portion of time elapse before it acquires heat from the source; or before it begins to radiate it again, when acquired? and how soon will it commence radiating on the opposite side; or according to what law does the heat distribute itself over or through the body? These questions are put in reference chiefly to the action of the body as a *screen*, and to the possibility of accounting for an *apparently direct* transmission of heat without the necessity of supposing any other principle than that of *conduction*.

5. What are the modifications which radiant heat undergoes in passing through small apertures? (p. 299.)

6. Sir J. Leslie found that the focus for simple heat, in the concave reflectors he used, was different from and nearer to the reflector than that for light: Is this confirmed by more extensive and exact observations? and what is the precise focal distance in different cases? (Leslie's *Inquiry*, p. 14.)

7. What is the proportion of heat reflected at different incidences?

8. What radiation takes place *in vacuo*? (p. 300.)

CHEMISTRY.

1. That British Chemists be invited to make experiments for removing doubts respecting the proportions of Oxygen, Azote, &c., in the atmosphere; for determining the proportions of Azote and Oxygen in Nitrous Gas and Nitrous Oxide; and for more accurately investigating the specific gravity of the compound gases in general.

2. That Dr. Dalton and Dr. Prout be requested to institute experiments on the specific gravities of Oxygen, Hydrogen, and Carbonic Acid, and that a sum not exceeding 50*l*. be appropriated to defray the expense of any apparatus which may be required.

3. That Dr. Turner[1] be requested to extend his researches into the atomic weights of the elementary bodies, and to report on the progress recently made in this branch of chemical science.

[1] Dr. Turner reported the progress of his researches to the Meeting at Cambridge.

4. That Mr. Johnston[1] be requested to undertake the experiments which have been suggested to the Committee, into the comparative analysis of Iron in the different stages of its manufacture.

*5. That a series of experiments on the effects of long-continued heat be instituted at some iron-furnace, or in any other suitable situation; and that a sum not exceeding 50*l.* be placed at the disposal of a Sub-Committee, consisting of Professor Daubeny, Rev. W. V. Harcourt, Professor Sedgwick, and Professor Turner, to meet any expense which may be incurred[2].

*6. That inquiry be made as to the most perfect method of purifying Mercury, and that the true specific gravity of the metal be determined.

*7. That an examination be made into the nature and quantity of the gases given off from thermal waters, whether there be any variation in these respects according to season of the year, hours of the day, or condition of the atmosphere; and whether there be any changes of temperature in the same waters.

*8. That the gaseous products which are discharged from the chimneys of smelting and other furnaces and fireplaces be examined, at various periods of the operations carried on in them, with a view of ascertaining the compounds which are formed when the processes are most successfully conducted, and also of detecting the existence of compounds which may perhaps be new or valuable.

MINERALOGY.

1. That Professor Miller be requested to undertake an examination of the form and optical characters of those Crystallized Bodies which have not been previously determined, and *that Chemists be invited to send him specimens of perfect artificial Crystals.*

2. That Dr. Turner, Professor Miller, Mr. Brooke, and the Rev. Wm. Whewell, be requested to cooperate in prosecuting and promoting the following inquiries, with a view to examine the theory of Isomorphism, and the connexion between the crystalline forms and chemical constitution of Minerals:

[1] Mr. Johnston reported the progress of his researches to the Cambridge Meeting.

[2] These experiments have been instituted by Mr. Harcourt, in Yorkshire, at the Low Moor Iron Works, the property of Messrs. Hird and Co., and at the Elsecar Furnace, belonging to Earl Fitzwilliam.

1.) To determine whether the angles of *varieties* of the same species (in the usual acceptation of identity of species) are identically the same, under various circumstances of colour, appearance, and locality; and if not, what are the differences.

2.) To determine the chemical constitution of such varieties,—the specimens, mineralogically and chemically examined, being in all cases the same.

3.) To determine what quantity of extraneous substances may be mixed with a crystalline salt, without altering its form.

4.) To determine the angles of the various species or varieties of isomorphous or plesiomorphous groups, and their respective chemical composition[1].

Desiderata noticed in Mr. Whewell's Report.

1. To determine the optical differences on which depend the distinctions of the different kinds of lustre, *metallic*, *adamantine*, *vitreous*, *resinous*, *pearly*.

2. To determine whether the oblique rhombic prism constitutes a real system of crystalline forms, or is a hemihedral form of the right prism.

3. To determine the limits of magnitude and simplicity in crystallometrical ratios.

4. To determine whether chemical groups are strictly *isomorphous*, or only *plesiomorphous*.

5. To determine whether the angles of plesiomorphous crystals are separated by definite or by indefinite steps.

6. To determine what are the differences of chemical composition corresponding to differences of optical structure in resembling minerals, as apophyllite, tesselite, leucocyclite.

GEOLOGY.

*1. That measurements should be made, and the necessary data procured to determine the question of the permanence or change of the relative level of Sea and Land on the coasts of Great Britain and Ireland; and that for this purpose a sum not exceeding £100 be placed at the disposal of a Sub-Committee, consisting of Mr. Greenough, Mr. Lubbock, Mr. G. Rennie, Professor Sedgwick, Mr. Stevenson, and the Rev. W. Whewell:—the measurements to be so executed as to furnish the means of reference in future times, not only as to the relative levels

[1] Professor Miller reported the progress of these inquiries to the Cambridge Meeting.

of the land and sea, but also as to waste or extension of the land.

*2. That Mr. Rogers (Professor of Chemistry in Philadelphia) be requested to furnish an account of the progress which has been made in investigating the Geology of the United States America.

3. That Professor Phillips be requested to draw up, with such cooperation as he may procure, a Systematic Catalogue of all the organized fossils of Great Britain and Ireland, hitherto described, with such new species as he may have an opportunity of accurately examining[1].

4. That Mr. John Taylor be requested to collect detailed sections of the Carboniferous series of Flintshire, with a view to a comparison with the same series in other parts of England; —with a view also of ascertaining the circumstances under which the Mountain Limestone is developed, after its suppression in certain coal-fields in the central parts of England.

5. That the attention of Geologists be invited to those coal districts in the midland counties of England, where, the Carboniferous Limestone and Old Red Sandstone being deficient, the coal measures rest immmediately on the Grauwacke and Transition rocks;—with a view to discover whether any circumstances connected with the physical structure of that part of the island can be stated, explanatory of the local absence of the two great formations above mentioned.

6. That sections and plans should also be collected of the Coal-fields of Worcestershire, Shropshire, Staffordshire, Cheshire, Lancashire[2], and the south-western part of Yorkshire[3].

7. That the Faults or Dykes in the carboniferous rocks in Flintshire should be examined, with a view to ascertain whether some remarkable differences in their character may not be observed, as compared with that of veins and dykes in other districts.

*8. That collections be made of accurate plans of "heaves" in the Veins of Cornwall and the North of England, with a view to determine how far the apparently horizontal heaves may be explained by vertical motion.

*9. That the direction, intersection, inclination, and breadth

[1] This catalogue is commenced, several monographs are composed, and a general basis is arranged. Communications, lists of organic remains, notices of localities, and *specimens of new or undescribed species* may be addressed to Mr. Phillips, Museum, York.

[2] Mr. Elias Hall of Castleton has constructed a map and sections of the Lancashire coal-field.

[3] Mr. Hartop exhibited at the Cambridge Meeting a correct map and detailed section of the coal strata on the river Dun.

of the non-metalliferous fissures which cross the planes of the strata, and in some instances divide many contiguous strata, should be observed, in relation to the same circumstances in the dykes and mineral veins of the vicinity; with a view to ascertain whether there be any and what connexion between these phænomena[1].

*10. That the history of ancient vegetation should be further examined, by prosecuting the researches into the anatomy of fossil wood which have been exemplified in Mr. Witham's recent volume.

11. That the quantity of Mud and Silt contained in the water of the principal rivers of Great Britain should be ascertained, distinguishing, as far as may be possible, the comparative quantity of sediment from the water at different depths, in different parts of the current, and at different distances from the mouth of the river; distinguishing also any differences in the quality of the sediment, and estimating it at different periods of the year; with a view of explaining the hollowing of valleys, and the formation of strata at the mouths of rivers.

12. That the experiments of the late Mr. Gregory Watt, on the fusion and slow cooling of large masses of stony substances, should be repeated and extended by those who, from proximity to large furnaces, have an opportunity of trying such experiments on a large scale; and that trial should be made of the effect of long-continued high temperature on rocks containing petrifactions, in defacing or modifying the traces of organic structure, and of the effect of the continued action of steam or of water at a high temperature, in dissolving or altering minerals of difficult solution.

*13. That the dimensions of the bones of extinct animals should be expressed numerically in tables, so as to show the exact relations of their dimensions to those of animals now living; and also to show what *combinations of dimensions in the same animal* no longer exist.

* 14. That the following Geological queries be proposed:

1.) Are there any instances of contorted rocks interposed between strata not contorted?

2.) Is there any instance of secondary rocks being altered in texture or quality by contact with gneiss or primary slates?

3.) Is the occurrence of *cannel* coal generally connected with faults or dislocations of the strata?

[1] Mr. Phillips has undertaken to state the results of his examination on this subject in certain parts of the North of England, and requests to be favoured with communications relating thereto.

4.) What is the nature of the pebbles in the new red sandstone conglomerate in different districts: do they ever consist of granite, gneiss, mica-slate, chert, millstone grit, or any other sandstone which can be distinctly traced to the coal series?

5.) Is the Red Sandstone of Kelso contemporaneous with that of Salisbury Crags; and what relation do they respectively bear to the adjacent coal-fields?

6.) What is the exact northern boundary of the coal-field of the River Liddle?

7.) What are the relations as to age of the two series of whin rocks, one running north-east along the Liddle in Roxburghshire, the other south-east in the neighbourhood of Melrose and Jedburgh?

8.) Can the Limestone of Closeburn in Dumfriesshire be recognised beyond that valley?

9.) Does the Wealden formation exist in the midland counties of England?

10.) What is the character of the districts in which ores of manganese occur?

11.) What is the history of the Hæmatite of Dalton in Lancashire, in relation to the beds in which it occurs?

12.) What are the mineralogical characters of the several beds comprised in Forster's Section of the Strata in the North of England; and what are the fossils contained in each?

Desiderata noticed in Mr. Conybeare's Report.

1. An accurate examination of the conclusions deducible from the known density of the earth, as to the solid structure and composition of the interior.

2. The attention of residents in our remote foreign dependencies is invited to the two great questions of comparative Geology and Palæontology. 1. Is there or is there not such a general uniformity of type in the series of rock-formations in distant countries, that we must conceive them to have resulted from general causes of almost universal prevalence at the same geological æra? 2. Are the organic remains of the same geological period specifically similar in *very* remote districts, and more especially under climates actually different; or are they grouped together within narrower boundaries, and under restrictions as to geographical *habitats* analogous to those which prevail in the actual system of things? (p. 410.)

3. An examination of the geological structure of the countries constituting the great basin of the Indus, where, if in any part of India, it is supposed a complete series of secondary strata may be expected. (p. 396.)

Desideratum noticed in Mr. Taylor's Report on Mineral Veins.

A correct account of the affinity that the contents of a vein bear to *certain* of the rocks in which the fissure may be situated.

ZOOLOGY.

The Committee recommend to the consideration of Zoologists the following subjects of inquiry:

*1. The use of horns in the class Mammalia; the reason of their presence in the females of some, and their absence in those of other species; the connexion between their development and sexual periods; the reason of their being deciduous in some tribes, and persistent in others.

*2. The use of the lachrymal sinus in certain families of the Ruminantia.

*3. The conditions which regulate the geographical distribution of Mammalia.

*4. The changes of colour of hair, feathers, and other external parts of animals; how these changes are effected in parts usually considered by anatomists as extra-vascular.

*5. The nature and use of the secretions of certain glands immediately under the skin, above the eyes, and over the nostrils, in certain species of the Grallatores and Natatores; the nature and use of the secretion of the uropygial gland.

*6. How long and in what manner can the impregnated ova of fishes be preserved, for transportation, without preventing vivification when the spawn is returned to water.

*7. Further observations on the supposed metamorphosis of Decapod Crustacea, with reference to the views of Thompson and Rathke.

*8. Further observations on the situation of the sexual organs in male spiders, and on their supposed connexion with the palpi.

*9. The use of the antennæ in insects. Are they organs of hearing, of smell, or of a peculiar sensation?

*10. The function of the femoral pores in Lizards, and the degree of importance due to them as offering characters for classification.

BOTANY.

1. That Botanists in all parts of Great Britain and Ireland be invited to compose and communicate to the Meeting of the

Association, Catalogues of County or other local Floras, with indications of those species which have been *recently introduced*, of those which are *rare or very local*, and of those which thrive, or which have become, or are becoming extinct; with such remarks as may be useful towards determining the connexion which there may be between the *habitats* of particular plants, and the nature of the soil and the strata upon which they grow; with statements of the *mean winter* and *summer temperature* of the air and the water, at the highest as well as the lowest elevation at which species occur; the hygrometrical condition of the air, and any other information of an historical, œconomical, and philosophical nature.

*2. That Professor Daubeny be requested to institute an extended inquiry into the exact nature of the secretions by the roots of the principal cultivated plants and weeds of agriculture; and that the attention of Botanists and Chemists be invited to the degree in which such secretions are poisonous to the plants that yield them, or to others; and to the most ready method of decomposing these secretions by manure or other means.

* A Committee was formed to conduct a series of experiments on the growth of plants from seeds, and to preserve the results of their experiments, in order to establish the identity or confirm the specific distinctions of certain allied plants, and to communicate the results obtained, from year to year, at the Meetings of the Association.

Mr. Don, Librarian to the Linnæan Society, has undertaken to be the channel of correspondence on this subject.

Desiderata noticed in Professor Lindley's Report.

1. An accurate account of the manner in which the woody part of plants is formed. "Perhaps there is no mode of proceeding to elucidate this point, which would be more likely to lead to positive results, than a very careful anatomical examination of the progressive development of the mangel wurzel, beginning with the dormant embryo, and ending with the perfectly formed plant."

2. An investigation of the comparative anatomy of flowerless plants, with a view to discover in them the analogy and origin of their organic structure.

3. The cause of the various colours of plants.

4. The nature of the fæcal excretions of cultivated plants, and of common weeds; the degree in which those excretions are poisonous to the plants that yield them or to others; the most ready means of decomposing such excretions by manures or other means.

ANATOMY AND PHYSIOLOGY.

*1. That the effects of poison on the animal œconomy should be investigated and illustrated by graphic representations; and that a sum not exceeding *25l.* be appropriated for this object. Dr. Roupell and Dr. Hodgkin were requested to undertake this investigation.

*2. That an experimental investigation should be made of the sensibilities of the Nerves of the Brain; and that a sum not exceeding *25l.* should be appropriated to this object. Dr. Marshall Hall and Mr. S. D. Broughton were requested to undertake these experiments.

ARTS.

* That Mr. Dent be requested to communicate to the next Meeting of the Association, a statement of the performance of his *chronometer with a glass balance-spring.*

Desideratum noticed by Professor Barlow in his Report on the Strength of Materials.

A set of experiments on the application of a straining force on vertical columns (of timber, iron, &c.).

STATISTICS.

*1. That Colonel Sykes be requested to prepare for publication his valuable statistical returns, collected by himself in India, relative to the four Collectorates of the Deccan, subject to the Bombay Government.

*2. That Professor Jones be requested to endeavour to obtain permission to examine the statistical records understood to exist in great number in the archives of the India House, and to prepare an account of the nature and extent of them.

*3. The inquiries of this section are restricted to facts relating to communities of men which are capable of being expressed by numbers, and which promise, when sufficiently multiplied, to indicate general laws.

* A permanent Committee of this section was appointed. Professor Babbage was requested to act as Chairman, and Mr. Drinkwater as Secretary.

In a Report since addressed to the Council by this Committee, it is stated, that the Committee having deemed it expedient to promote the formation of a Statistical Society in London, a public meeting was held on the 15th of March, 1834, at which it was resolved to establish such an institution. The Society already includes more than three hundred members, and has issued a statement of its objects and regulations, which is subjoined in the Appendix.

The Committee remark, that "though the want of such a society has been long felt and acknowledged, the successful establishment of it, after every previous attempt had failed, has been due altogether to the impulse given by the last Meeting of the Association. The distinguished foreigner (M. Quetelet) who contributed so materially to the formation of the Statistical Section, was attracted to England principally with a view of attending that meeting; and the Committee hail this as a signal instance of the beneficial results to be expected from that personal intercourse among the enlightened men of all countries, which it is a principal object of the British Association to encourage and facilitate."

GENERAL SCIENCE.

* That a sum not exceeding 100*l.* be appropriated towards the execution of a plan proposed by Professor Babbage, for collecting and arranging the *Constants* of Nature and Art[1].

APPENDIX

CONTAINING DIRECTIONS FOR OBSERVATIONS ON THE TIDES, AURORA BOREALIS, &c.

TIDES.

Observations of Tides along the coasts of Great Britain and Ireland will be valuable, both in the construction of more accurate tide tables, and as data towards the perfection of the theory of tides.

Observations of the tides should record particularly,

The time in hours and minutes, and height of high water daily, or if convenient every tide.

The time and height of low water.

[1] See Appendix, p. 490, for an Abstract of Mr. Babbage's Plan.

The direction of the wind, and the height of the barometer and thermometer.

The direction and velocity of the stream of flow and ebb.

At what hour (with respect to the time of high water and low water) the slack water after the stream of flood, and after the stream of ebb, respectively occur.

The height of the water must be given *from some fixed mark or line,* which should be described accurately, so that it may be easily found again at a future time. The observer ought to state the manner in which the height was measured; the manner in which the moment of high water was fixed upon; the time employed, *whether apparent or mean solar time,* and how it was obtained.

The height of the water at the end of every minute, for half an hour before the expected time of high water, and until there can be no doubt that the time of high water is past. Machines to dispense with this minute attention are described in the *Philosophical Transactions,* 1831, and in the *Nautical Magazine* for October 1832 [1].

The uncertainty occasioned by waves may be avoided by making the observation in a chamber, to which the water has access by a small opening, or by fixing in the water an upright tube, (of wood or iron, for instance,) the bottom or sides of the tube being perforated; in either case an upright measuring rod, carefully graduated, and connected to a float, will rise and fall with the tide, and permit, at any moment, the height of the water to be read off against the collar through which it works. This rod may be so constructed as to leave a moveable index at the highest and lowest points.

A long series of continued observations can alone be of use towards the determination of the dependence of the time, height, and other circumstances of high and low water upon the places and distances of the sun and moon; but a smaller number of observations will often be sufficient to determine the *establishment* of any place, with more or less accuracy, according to the number of observations; and the best mode of doing this is by comparative observations with some place of which the establishment is accurately known, or where observations are continually carried on. A few sets of comparative observations of neighbouring places will give the *relative* time of high water at these places with considerable accuracy; and thus the motion of the tide-wave and the arrangement of the

[1] Tide-guages may be seen in operation at St. Katharine's Docks, London. An excellent one has lately been set up near Bristol by the Literary and Philosophical Institution of that city.

cotidal lines (or lines along which it is high water at the same instant,) will be discovered. It would be very desirable for those who have the opportunity, to combine so as to effect the detailed description of the tides through some small extent of coast, such as that which has been effected by M. Daussy for the west coast of France.

AURORA BOREALIS.

Notwithstanding the attention which has been paid to the phænomena of the Aurora Borealis, and the various hypotheses which have been imagined to explain them, it will be found that there is a want of information on the points which are most necessary as bases of induction; and the British Association have therefore been induced to appoint a Committee in the express view of directing observers to the really important features of this meteor, and of obtaining, by a system of contemporaneous observation, data which experience shows cannot be derived from insulated exertion.

The following are the most important points which demand the attention of observers:

1. The elevation of the auroral arches and streamers above the surface of the earth.
2. The determination of the question whether the auroral exhibition is accompanied by sound.
3. The existence of recurring periods of frequency and brilliancy in the Aurora.
4. The influence of arches, streamers, and other auroral phænomena upon the magnetic needle.

1.) It is recommended to all who intend to observe Auroras, to make themselves well acquainted with the names of all the principal stars to the north of the equator, especially those which do not set here. This will be most easily done by studying a celestial globe. Good maps of the stars may also be consulted with advantage. Either the proper names or the Greek characters with the name of the constellation will be sufficient.

Persons who may prefer to determine the angular elevation and position of the arches and streamers by graduated instruments, must be supposed well accustomed to the use of them; they may, however, be reminded, that telescopic sights are for this purpose useless, and that steady instruments, which can be handled with ease and expedition, are much more avail-

TABULAR CONSPECTUS OF OBSERVATIONS ON THE AURORA BOREALIS

OF THE 12th OF OCTOBER, 1833, REDUCED TO GREENWICH MEAN TIME.

DENT, near Sedbergh, Yorkshire.

Rev. Prof. Sedgwick

N. Lat. 54° 18′ Long. W. 2° 26′

h. m.
7. 25.—Strong illumination to the N.W., which gradually expanded to a point a few degrees E. of N., and to a point nearly W. The places of greatest illumination [illegible] but chiefly near the magnetic meridian. After some time ascending [illegible] light in several directions. No well defined luminous masses.

8. 5.—The light assumed the form of an ill-defined arch; the vertex was nearly in the magnetic meridian, the western end reached the horizon under Arcturus, the curve passed under β and γ Ursæ Majoris. Its breadth was [illegible] the lower edge ill-defined, and seemed to blend itself with the common light of the region.

Streaks of light rose occasionally towards the zenith, especially from N. by E., N. by W., and W. Clouds of extreme blackness were scattered through the [illegible] of the Aurora. Stars distinctly, but rather dimly seen through the brightest part of the Aurora.

8. 40.—The black clouds had disappeared.

8. 55.—The upper part of the arch better defined than before, passed between [illegible] and ξ Ursæ Maj. and very near ζ Ursæ Maj. Its vertex in or near the magnetic meridian. Lower edge still very ill-defined. For a few minutes the [illegible] region was divided by a concentric band of a dark dirty grey colour [illegible] arches: the lower arch was ill-defined and unsteady.

[illegible] A great expansion of light where the N. E. [illegible] the arch touched the horizon, spreading out to a place a few degrees [illegible] the centre of the bright [illegible] mass of light was about 35° [illegible] A few patches of clouds in this region were faintly [illegible] as if by the light of the Aurora transmitted through [illegible] of cloudy light where the N.W. limb [illegible] (55° W. of the magnetic north.) [illegible] lower edge better defined, breadth nearly [illegible] pointers of Ursa Major, and the upper [illegible] the upper and lower curves. [illegible] Below the luminous zone, the [illegible] horizon, was a most remarkable dark dirty grey colour [illegible] through a fog. The great mass of [illegible] vertical masses, separated from [illegible] arch. The dark grey part under [illegible] These gradually increase in number [illegible] waves or zig-zags, being on the [illegible] During their passage they sent up spires of in[illegible] of a faint reddish tinge. Soon after, the arch [illegible] observed to break up in points like the teeth of a comb, and the whole space [illegible] dark grey colour was broken up into ribs and [illegible] especially in the western region. This appearance was succeeded [illegible] undulations propagated towards a spot S. E. of the zenith. The undulations [illegible] to be propagated in interrupted arches of light, being visible only in those [illegible] of undulation crossed certain vertical streamers. All [illegible] however, to have one common relation; the vertex of [illegible] arch being nearly in the magnetic meridian. For several seconds there was a well-defined arch passing through α Lyra.

9. 40.—Great streaks of light parallel to the magnetic meridian; dark clouds [illegible] of light in region S. E. of the zenith.

[illegible] Succession of streamers ascending from the W. and N. This illumination seems to be propagated by them to the region between Jupiter and [illegible] It seemed as if certain portions of the heavens in this region reflected the [illegible] streamers; as the same definite portions of space were illuminated [illegible]

[illegible] illumination towards the N. Dark patchy clouds to the N. W., [illegible] horizontal streaks of clouds illuminated at the edges. At this [illegible] were many streamers appearing to diverge from a point below the horizon, [illegible] below the end of the Bear's Tail (η Ursæ Major.): those which were nearly [illegible] and N. declining 15° from the perpendicular. Soon afterwards undulations [illegible] propagated along these diverging lines, and amorphous blotches of [illegible] in one direction as far as Jupiter, were seen in the region S. E. of [illegible] zenith. Some of these were stationary and constantly illuminated; others were [illegible] only illuminated at intervals. At that distance from the N [illegible] which seemed to be the seat of propagation, the luminous blotches never [illegible] of streamers or spires of light.

10. 10.—Stars and spaces of light nearly in the same direction as before, but [illegible] bright. No light propagated to, or reflected from, any cloudy spaces S. of [illegible]

[illegible]—Greatest [illegible] in the Mag. N. Bright streaks of light and [illegible] shone brightly through the brightest light

10. 20.—No spires or beams of light, but undulations propagated from the Mag. N. Phenomena repeated till 10. 40. nearly.

10. 40.—Greatest illumination due N. Bright space extends to Gemini and to the W. Fine streamers from the N. The bright space arranges itself into an arch, commencing nearly N., passing through α Ursæ Major.; about 25° high near the magnetic meridian (measured only by a geological clinometer.) Below the arch [illegible] dirty grey [illegible] noticed before. For a short time here appeared a [illegible] passing through the four stars of the Great Bear.

[illegible]—The arch [illegible] followed by streamers and beams of light ascending from Mag. N.

11. 5.—Strong illumination towards the N. W. Fine undulations propagated [illegible]

11. 27.—[illegible] greatest illumination in the N.

11. 40.—Very luminous to Mag. N., brightest part defined above by an arch 21° high; the light seemed to extend to the horizon. This was afterwards broken [illegible] ascending spires of light, and then [illegible]

12. 10.—The light was dispersed in fleecy ill-defined masses, extending from [illegible] as far as the W.

12. 40.—A very strong illumination continued till nearly this time, when the [illegible] were discontinued, as the phenomena presented nothing of a definite [illegible]

DENT.—GENERAL REMARKS.

During nearly the whole time of observation, the watch-face could be read with [illegible] and several of the notes were written with no other light than that of [illegible]

It may, I think, be observed generally that the places of greatest illumination [illegible] to be broken up into parts, separated from each other by spaces of the [illegible] and quite distinct from the inky colour of the [illegible]

I [illegible] several times, to ascertain the variation of the needle, but found the mounting was not sufficiently delicate to give results worth recording.

MANCHESTER.

Three sets of Observations by R. Potter, jun. P. Clare, and W. Hadfield.

N. Lat. 53° 29′ Long. W. 2° 13′

h. m.
6. 3½.—Faint arches and streamers noticed. (*R. P.*)
6. 41½.—Long and large streamer directed towards the zenith. (*R. P.*)

7. 0½.—Irregular nebulous masses to 60° alt. and more.

7. 2½.—Portion of arch S. of zenith covered the principal stars in Delphinus and Aquila, it was 5° broad, passed southwards and vanished.
7. 40½.—Light in the north which had been 20m. stationary, at 15° or 20° alt. and about 100° extent on horizon is now disappearing.

8. 10½.—Bright light in the N.

8. 39.—An arch ill-defined, about 18° high in the middle.
8. 44.—The arch has risen a little, is better defined, and a dark shade appears [illegible]
8. 53½.—The arch has its vertex under ζ Ursæ Maj. and its upper edge touches γ Ursæ Maj., altitude about 19° 30′. (*R. P.*)
8. 54.—Arch about 24° above the horizon at its vertex, 5° broad; below it is a dark parallel space 5° broad; and below this a second parallel arch of light 3½° broad. The space below this arch to the horizon is very dark like a dense black cloud. One foot of the exterior arch is nearly west, and the other foot about 35° E. of N. the vertex is in the magnetic north. (*P. C.*) Mr Hadfield says 20° alt. and horizontal extent 120°.
9. 9.—The two arches remain in the same position. Stars are distinctly seen in the upper luminous arch, and also in the space within the lower luminous arch.
9. 14½.—η Ursæ Maj. in the upper edge of the arch, the height of which by measure = 21° 10′. (*R. P.*)

9. 21.—Beam or streamer, the first seen this evening, darted suddenly upwards from the higher arch about 30° E. of N. It was visible 30 seconds. (*P. C.*)
9. 22.—A beam or streamer in the same position, visible 10 minutes.
9. 27.—A small black cloud visible in the dark space within the lower arch, about 15° E. of N. and 10° high.
9. 29.—A beam or streamer, rose from the dark space within the lower arch 10° E. of N. and continued visible for 2 minutes. Clouds are rising in N. W. so as to cover a portion of the light.
9. 31.—Streamers are shooting up rapidly in the W. and the boundary of the arch is much less distinct.
9. 33.—Streamers and gleams or broad flashes of light are rising from various parts of the arch, they soon covered almost the whole of the northern part of the hemisphere, and passed to the south a little beyond the magnetic zenith. The arch has disappeared, but a strong light remains in the north.

9. 41.—For the last 10 minutes the streamers have been very active, and broad flashes of light have succeeded each other with scarcely any intermission, in the northern part of the hemisphere: the clouds have increased so as to obscure most of the sky.

10. 9.—Clouds disappearing, streamers active.

10. 39.—Since the last observation the display of streamers and coruscations has been fine. Arch observed at this time, its lower edge had its vertex in Benetnasch (15° alt.) and passed a little below β in Bootes, and a little above λ in the foot of Urs. Maj.

11. 9.—Since the last observations the streamers and coruscations have diminished in number and brilliancy. Now very few are to be seen, but a bright light continues in the magnetic N.
11. 39.—The light in the north increasing.

12. 9.—It has continued to increase. Another arch is visible in magnetic N. the highest point of which is about 15° above the horizon. Vertex in Mizar.
12. 24.—Streamers begin to arise from various parts of the arch.
12. 39.—Streamers much increased in number and activity: also coruscations and large masses of light like white clouds, that almost cover the northern part of the hemisphere.
12. 49.—The light is more brilliant, the arch has disappeared; streamers very bright and active, particularly in the N. E. The sky is covered with streamers and gleams of light from nearly the northern horizon to a little beyond the magnetic zenith. The streamers form a corona at the magnetic zenith, from which point the Pleiades are about 1° south.
12. 54.—Many bands or fillets of light have been instantly formed, and then immediately rose toward the zenith; their first appearance was in the direction and form of the luminous arch of 8. 54.; then they suddenly rose in the centre towards the zenith and disappeared. The east and west extremities of these bands seemed to move through a very small space and gave the appearance as if the extreme points were stationary, whilst the highest point moved round a centre with a velocity that carried it through 60° in about one-third of a second of time; the other portions of the bands passed through a less space as they approximated to the E. and W. ends. For about ten minutes these bands of light succeeded each other with great rapidity. The streamers and coruscations of light continued very active.
13. 9.—The coruscations, streamers, and bands of light have been diminishing in activity and brilliancy for 10 minutes, but during the last half hour the brilliancy, extent, and activity of the aurora far exceeded the appearances in any previous part of the night.
13. 24.—The streamers and coruscations have ceased, but there is a broad light in the north.
13. 39.—This light diminishes in brilliancy.

YORK.

J. Phillips.

N. Lat. 53° 58′ Long. W. 1° 4′

h. m.
6. 1.—Mean time. The appearance of the northern sky [illegible]
6. 44.—By this time the doubt was entirely removed; beams had been seen playing, and bright fluctuating light was at intervals excited at detached points along an arch, rectangular to the magnetic meridian, as if along this line thin vaporous clouds or smoke-wreaths were illuminated from behind. One continual beam was seen N. 20 W. The phenomena were chiefly on the eastern side of the magnetic meridian.
6. 52.—A falling star was seen, passing nearly horizontally from E. to W., through an arch of 10° east of the Pole star. It was bright. Another faint falling star seen through a shorter arch above the Great Bear.
7. 14.—A falling star from W. to E., declining 40°, colour red.
7. 14.—A beam of light N. by W.
7. 39.—Falling star, directly downwards in the line of the magnetic meridian, seemed to fall through 15° from the arch which was [illegible]
7. [illegible].—An arch of light was now distinct (above a dark horizon band), its lower edge one-third of the height of β Ursæ Majoris.
7. [illegible].—The arch had risen higher, its lower edge was now half the height of β Ursæ Maj., and its upper edge two-thirds of that [illegible] No streamers were visible at this time, nor had many been seen before.
7. 50.—The summit of the arch was now 3° below the stars β and γ Ursæ Maj. It had been rather bright and of a yellowish hue, not unlike some distant clouds, a resemblance augmented by its great breadth, which was twice as great as that of the black space beneath. A singular notch was noticed in the upper edge of this arch, bearing N by E. It now began to fade and to sink a little.
7. 57.—Suddenly it appeared double, in consequence of the production of a very narrow faint arch above that seen before, and separated from it by a dark band.
7. 58.—The upper arch rose, so as to include β and γ Ursæ Maj., in its middle.
8. 2.—It had vanished away, after rising still higher.
8. 4.—A beam of light, slightly arched, and directed obliquely upwards, from about 4 degrees (by estimation) to the right (south) of the Pleiades, between α Arietis and Jupiter. This singular beam, brightest in the middle, was so unlike the others, that I hesitated for some time to record its appearance. It lasted, with hardly a perceptible change of place towards the south, for half-an-hour, but its brightness was variable.
8. 6.—The arch appeared divided by a transverse black cloud.

8. 16.—The broad belt of light in the north was divided by several transverse arched bands.
8. 21.—It had become higher, but was more confused.

8. 29.—Falling star from just under Jupiter, its course N. declining 20°,—rapid.

8. 54.—Arch irregular, banded, the upper edge between γ and β Ursæ Maj.
9. 19.—Falling star from Polaris direct through Dubhe.

9. 29.—At this time the aurora assumed a totally new and far more imposing character. Beams of greater brilliancy shot up in the west, and the dark horizon band was traversed, in arched lines, from W. to E., by roundish masses of strong light, orange-coloured beneath, from which sprung, irregularly, many short beams of paler colour: then at intervals a wild fluctuation of almost connected light from W. to E., with beams suddenly arising from a general arched base of light, below the more regular arch previously noticed, which became less and less distinct, as the undulations grew more frequent, and the beams rose higher. The undulations of light became every moment more rapid and extensive, lost their transverse movement and rose higher into the sky, till they covered with their pale and restless illumination the whole of the northern hemisphere, flashed over the zenith stars, and even to a considerable distance south. As soon as the strangeness of the spectacle permitted me to examine it with calmness, I found that these flashes *seemed* all to spring from an arched luminous base (probably the perspective union of their images) with a rapid but limited undulation; the higher above this base the appearances were examined, the more decided was the evidence of intermitting action, the more disjoined became the upward waves of light, and at length it was evident, that all these waves rose towards the zenith with a motion continually accelerated, and an individuality always growing more complete. It was seldom that any two waves flashed at the same instant across the culminating point; when they did so, the light was sometimes augmented there. * (*Note.*)
9. 40.—At this time, the flashes frequently extended much beyond the zenith stars.
9. 50.—The arch in the N. N. W. became again distinct, including α Ursæ Maj., its centre being in line with ι Ursæ Maj.

9. 57.—Now, and also several times before, the flashes extended over Jupiter. A beam was seen passing through ι Ursæ Maj.
The flashes now became lower and less extensive.

10. 10.—Beams of light travelling to the westward.

10. 13.—The zenith has been clear for some minutes.

10. 34.—Arch noticed again, its centre under ζ Ursæ Maj., its height in the middle of the light equal to one-half that of ι Ursæ Maj.
10. 44.—And later, till near twelve o'clock. The arch in the north was subject to much variation. Beams and faint flashes rose at intervals.

12. 4.—There remained only a vague illumination in the N. W.
Towards 13 the phenomena were repeated with greater illumination; the waves again passed the Zenith, and at Rokeby, (43 m. N. W. by N. of York) were seen to pass over Jupiter.

YORK.—MAGNETIC OBSERVATIONS.

7. 14.—Till this time, the needle closely watched was certainly unaffected by the aurora.
8. 9.—The southward end of the needle deviated six and seven minutes E.; beams travelling east.
9. 34.—From this time it deviated very rapidly to east.
9. 44.—It had reached its maximum deviation to the east. It was at this time that the flashes began to cross their culminating point. Deviation E. 50 minutes.

9. 48.	45	10. 5.	55
9. 51.	13	10. 9.	45
9. 54. Deviation W.	1	10. 10. Beams travelling west	34
9. 57.	3	10. 13.	17
10. 0.	12	10. 34.	11
10. 2	25	10. 44.	7

On these results I shall make a few remarks. 1. The needle, in all my experiments, has remained unaffected by auroral arches and stationary beams. 2. It was very suddenly affected by the elevation of the flashes, apparently in proportion to their height; after the flashes had reached and crossed the zenith far to the south, the needle began to return to its usual place. During the eastward deviation of the south end of the needle, the flashes were much more abundant on the eastern side of the magnetic meridian. 3. It appeared to me probable, that the deviation of the needle was affected by the movement of the beams, in such a way that its southward end deviated to the same side as the beams moved.

YORK.—GENERAL REMARKS.

For eleven days before the grand aurora of the 12th of October, the barometer had been very high, and the weather remarkably dry; the sun powerful, the nights chill and dewy; on the 11th of Oct., the barometer began to fall, and continued to fall till midday on the 12th. There was rain on the 11th, and on the 12th till 2 p. m. From midday on the 12th, the barometer rose rapidly (and continued to rise until 9 a. m. on the 13th), the air became colder and drier.

* *Note.*—I call them *waves*, not only because of their apparent origin in distant undulations of a luminous mass, but also because of a very singular [illegible] of light and darkness, by which each of them was affected in its upward course through the sky. The line along which any one wave passed upwards, was not illuminated throughout its whole length at once, or in succession, but only in certain detached nearly rectangular spots, which successively grew larger towards the zenith. These luminous spots,—resembling nothing so much as scattered masses of transparent white smoke,—were visible for an instant; and as one wave followed another in rapid succession, the path of each wave seemed alternately light and dark. It appeared to me, that the luminous spots in adjacent waves were alternate, and thus the sky was irregularly chequered with light, so as to cause my imagination to compare it to a chess-board. To me, it seemed on a gigantic scale, not unlike the appearance which the cirro-stratus cloud assumes, when it is called "mackerel sky." The culminating point of the waves was at times in a continual flutter, in consequence of the various arrival of waves from different quarters; but it perhaps never happened that any thing like twisting light was seen there. The illuminating power of all this splendid phenomenon was not by much so considerable as that on the 17th of September.—My attention was so occupied by observations of the flashes, and of the magnetic needle, that I was obliged to neglect one fine beam which disappeared in various directions between N. W. and N. by E.

GUISBOROUGH.

W. L. Wharton.

N. Lat. 54° 31′ Long. W. 1° 3′

h. m.
8. 20.—Well defined arch, passing between α and β Ursæ Majoris its summit somewhat above ζ Ursæ Majoris, no radiations.
8. 35.—Arch 2° or 3° higher; faint radiations.
8. 50.—Radiations and coruscations, all converging to the head of Andromeda.

9. 0.—Brilliant radiations with prismatic colours, converging to α Andromedæ, accompanied with sheets of light moving with vast rapidity to the same part of the heavens; summit of arch moved considerably eastward.
9. 10.—Continuous sheets of light and radiations from the whole horizon, between W. N. W. and E. N. E., all converging to the same part of Andromeda, but upon reaching that point apparently checked in their further progress, and (those from the N. W. especially) twisted as it were for a short distance toward the S. E. and then dissipated in space.

9. 45.—Similar appearances; *arch* ill defined.

10. 45.—Arch very distinct, its summit considerably lower than when last observed.

11. 0.—Sheets of light still flying toward the head of Andromeda.
11. 15.—Some clouds (cirro-strati) slowly approaching from the N. Sheets of light and radiations being distinctly *above* the region of these clouds.
11. 35.—Splendid sheets of light; ill defined arch.
11. 50.—Arch ill defined, its summit near η Ursæ Majoris, more cirro-strati, evidently floating below the radiations.

CAMBRIDGE.

Professor Airy.

N. Lat. 52° 13′ Long. E. 0° 6′

h. m.
6. 32.—Several faint streamers directed to a point S. of α Cygni, the elevation of their bases being perhaps 15°, and of their tops 60°; moving slowly towards the W.; a faint bank of light in the N.

6. 53.—No streamers; generally a faint light in the north.

7. 27.—Much the same.

7. 52.—In the north a banked yellow cloud, its height half that of γ Ursæ Majoris. One degree to the right of the Pleiades a streamer about as high as the Pleiades, directed toward α Cygni. This streamer remained steady about half an hour.
7. 54.—The upper boundary of the bright cloud was extremely sharp; it began to the left of Arcturus, passed a very little above Arcturus, below γ Ursæ Maj. at exactly half the elevation of γ Ursæ Maj. (which was its highest point) and terminated E. of the N. at about half the azimuth of β Aurigæ. The black bank below reached to about half the height; it was very black.

7. 59.—A black line was discoverable very near the upper boundary and parallel to it. The upper part rose and the lower fell a little, thus widening the black line. About Arcturus the upper part rose most.

8. 2.—The upper part after rising considerably had wholly disappeared, and the lower had sunk a little under γ Ursæ Majoris so that its elevation was perhaps ⅓ of that of γ Ursæ Maj. The highest part was now a little further west. A streamer through α Arcturus, parallel to that above mentioned (7. 52) but hitched.

8. 7.—A streamer exactly below Polaris, which plunged downwards into the black.
8. 12.—More diffuse to the west, the dark part more illuminated.

8. 44.—No remarkable alteration.

9. 38.—The eastern boundary of the arch as *before*: the western had advanced nearly to the W. The lower edge of the bright cloud sharply defined, and its height not quite half the height of γ Ursæ Maj.; the upper edge extended upwards about two-third of the remainder of the space towards γ Ursæ Maj.
9. 42.—A gentle appearance of waving was seen, confined at first entirely within the limits of the bright cloud, and looking like very long horizontal waves, very narrow in the vertical direction, running vertically upwards. In a few minutes the waves became considerable, reached as high as α Ursæ Major., and still increasing. Throughout the appearance of the waving, it was evident that the waves or pulses and the streamers were wholly unconnected; the pulses flashed over the streamers without altering them.
10. 0.—In the N. a very bright patch from which vivid streamers broke. Towards the W. it was more lumpy. Pulses continued.
10. 12.—The bright bank had almost entirely lost its smooth outline, and was broken into irregular toothed lumps. Pulses continued. The pulses when highest extended up to β U. Minoris.
10. 27.—Little light; the black below much diminished in breadth. No pulses (I believe.)

11. 47.—Rather lighter.

13. 0.—Pulses were seen.

HERON COURT, 4m. N. W. of Christchurch, Hants.

Hon. Charles Harris.

N. Lat. 50° 43′ Long. W. 1° 50′

h. m.
8. 22.—Bright, irregular arch, like a luminous bank of fog, about 8° above the horizon.

8. 37.—Its greatest intensity in N. N. W., was a little below α in Canes Venatici, which was seen, but faintly, through it. The lower edge of the arch was about 3° below this star. The base of the arch about this time became irregular, and coruscations were shot up along the whole line.

9. 57.—Large bodies of light traversing from E. to W, across the N., and shooting up brilliant coruscations in N. N. E., N. and N. W., some of them reaching an altitude of nearly 60°; they were accompanied by faint flashes of light, at right angles to the coruscations or beams, and apparently behind them, resembling narrow luminous bands of smoke, perpetually rolling up and disappearing.
10. 7.—A bright coruscation shot through the star δ in Ursa Major. The main body of light now faded away, but the flashes still continued.
10. 22.—Flashes more brilliant, and accompanied by other flashes or rapid passages of light from E. to W. and W. to E. along apparently a line of vapour, as if they were electric discharges conducted along it: three or four successive flashes seemed to light up the same irregularities in the form of the conducting medium, as if a band of vapour existed, invisible except when the electric current passed through it.
10. 37.—A low arch again formed, its base scarcely 5° above the horizon, extending to about 7°; between this and 17° the flashing continued, but never rose above Benetnasch in Ursa Major.
10. 52.—A broad coruscation remained *some time immediately under Mizar and Alioth in Ursa Major., extending* further to the horizon, the flashing being vivid across its upper portion.
The coruscation moved slowly westward and faded away.

ARMAGH.

Rev. Dr. Robinson.

N. Lat. 54° 21′ Long. W. 6° 30′.

h. m.
8. 11.—Above a luminous mass on the horizon are four parallel arches, 3° asunder. The upper edge of the lower arch passed below γ and α Ursæ Maj., [illegible] and as much below ζ.

9. 1.—Three parallel arches, the principal one has its upper edge on Polaris, and midway between Capella and β Aurigæ: its lower a little above β and γ Ursæ Minoris.
9. 6.—Arch in same place, but interrupted by what seem like black streamers, directed one toward β Aurigæ, the other as far above Capella, their edges being perfectly rectilineal.
9. 11.—Upper edge of arch has risen to Lyra and Capella, and a new arch has risen beneath it, its upper edge on ζ and β Ursæ Majoris. In the first are now three of the black streamers, the upper crossed by stripes of light E. N. E.
9. 31.—*Sheet* aurora rolling in *waves* from all points of the northern horizon, to a part 1½° W. and 1° N. of α Andromedæ.

10. 14.—Streamers converging to the same point. In that region distinct portions of sky become repeatedly luminous when streamers reach them, retaining the same outline like invisible clouds suddenly lighted up by them.
10. 26.—The mass of light in the N. W. (below which no dark space was visible at any time this evening, perhaps on account of haze) nearly extinct, and a few faint streamers alone shew themselves.
11. 6.—A renewal of streamers low down on the N. W.; above Lyra and γ Draconis, below Ursa there is sheet aurora with a tendency to condense itself in the direction of arches. The motions are much less rapid than those of the streamers, and its waves, even when they reach the zenith, much fainter.
The light of the aurora was sufficient to allow of counting seconds on a chronometer.

CAMBRIDGE.—GENERAL REMARKS.

In the pulses I noticed clearly what I had noticed very well on a former occasion, (I think in 1827, certainly before 1828) that it was not like clouds of light passing upwards and continuing luminous in their course till they vanished; but it was as if certain definite spots only of the sky had the faculty of receiving illumination in the order of vertically upwards. From the odd way in which these spots were distributed, it sometimes appeared as if the pulses flashed obliquely upwards.

In the morning there had been a squall from the N. W. with heavy rain; the afternoon dull till near sun-set, when it became very clear.

FIFESHIRE.

Mr. Lawrence Buchan observed at about 9 P. M. the auroral arch passing near the zenith. (*R. P.*)

AURORA BOREALIS

EAN TIME.

CAMBRIDGE.

Professor Airy.

N. Lat. 52° 13′ Long. E. 0° 6′

h. m.
8. 25.—The aurora appeared in the form of a large bright cloud, bounded on lower side by the horizon, and on the upper side by an arch of a small circle t differing much from a great circle). The extremities of the arch were in N. E. and W. N. W. or nearly W. The upper boundary was lower than β æ Majoris by ⅜ × distance from α Ursæ Majoris to β Ursæ Majoris. Several ll black clouds were scattered over the aurora-cloud, and above it were several tly illuminated, whose light appeared to originate simply in the illumination ne aurora.

8. 35.—No change, except that the whole appeared to have moved a little to west.

8. 58.—The form and brightness of the arch had not sensibly altered; but a black cloud on its face attracted particular attention. The western ex-nity of this cloud was below γ Ursæ Majoris, its horizontal length fully three s the distance from β Ursæ Majoris to γ Ursæ Majoris, its vertical breadth than one-fourth of its length, the eastern end being somewhat broader than western. The aurora-cloud suddenly formed itself into streamers, (or amers were formed in front of it) some perhaps 30° or 40° high, but lasting in state only for an instant, and two streamers of sensible breadth shot up either ront of the black cloud or through it, so as to illuminate it, near its western remity in two nearly vertical lines, corresponding to the course of the amers, whose upper and lower parts were visible above and below the cloud. emarkable change in the constitution of the cloud followed immediately; the tern half became curdly, the upper edge of its small portions being luminous; western half began to disappear; at 9. 15. no trace of the cloud discoverable.

9. 10.—A shooting star from E. to W. very nearly through δ Ursæ Majoris. light of the aurora-cloud gradually diminished; the part which remained gest was a little E. of N. where some light was still visible at 10. 30.

und the *dipping-needle* to be unaffected by this aurora at Armagh. It was the dark segment was formed at about 12½ h.; the phenomena ceased The greatest intensity of the aurora was in the direction of the magnetic

e 18th, at 8. 34. a low arch was seen there passing below the feet of U. (in the middle) above the horizon; at 8. 50. the lower edge of the lower ick.

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TABULAR CONSPECTUS OF OBSERVATIONS ON THE AURORA BOREALIS

OF THE 17th OF SEPTEMBER, 1833, REDUCED TO GREENWICH MEAN TIME.

YORK.

J. Phillips.

Lat. 53° 58′ Long. W. 1° 4′

h. m.

8. 0.—Auroral arches and beams in the N. N. W.

8. 9.—Arch 3° or 4° broad, including in its middle and vertex β Ursæ Majoris. It gradually and constantly increased in breadth and rose in position.

8. 14.—Arch includes, nearly in the middle of its breadth, Dubhe, Arcturus, and Capella. From this time it grew fainter and rose higher.

8. 34.—Beams or streamers in great number and brilliancy, shooting upwards in narrow distinct lines athwart the whole northern sky, *in front of the arch*, from the horizon to about 60° alt.

8. 44.—The arch (which after passing Polaris in its upward movement shewed itself double) was now in two distinct parts; the upper rose most rapidly to within 15° of the zenith, when it vanished. The lower arch became indistinct, and the beams rose higher and more frequently, directing themselves to a point S. of the zenith. Many of these beams were at one time joined at their bases into a singular *reversed arch*, of which the centre was near the Pole star. These streamers shewed momentary traces of colour; in the line of the magnetic meridian, they were vertical, towards the horizon E. and W. their tops were inclined probably 20° to the South. They appeared wholly unconnected with the arch.

9. 4.—No arch visible.

10. 49. to 11. 19. } A low faint arch stationary, its upper edge nearly reaching to ε and γ Ursæ Majoris; its vertex under Mizar (alt. about 18° in the middle.)

MANCHESTER.

P. Clare, W. Hudleigh, and R. Potter.

Lat. 53° 29′ Long. W. 2° 13′

h. m.

8. 9.—Arch at its summit 32° high, very brilliant.

8. 18.—Arch almost exactly includes α and β and γ and δ Ursæ Majoris. *(R. P.)*

8. 24.—The arch 7° broad, includes Dubhe, Arcturus, and Capella, so that Capella is on the extreme upper edge; Dubhe rather above the middle of the breadth, and Arcturus rather below the middle. Centre of the arch a little E. of δ Ursæ Majoris. Extent of the arch 130°. *(P. C.)*

8. 27.—The upper edge of the arch coincides with η and ζ Ursæ Majoris; the lower edge with δ Ursæ Majoris.

8. 40½.—Arch 38° or 39° high, and extending about 160° on the horizon. *(R. P.)*

8. 44.—Many streamers in the N. directed towards the magnetic zenith.

8. 44.—Arch passed over Arcturus, S. of Polaris, 3° or 4° north of Algol, ending obscurely near the Pleiades (alt. about 60°, vertex in the magnetic meridian.) *(W. H.)*

8. 49.—Half the hemisphere illuminated; many bright streamers and flashes of light rose from the magnetic N.

8. 54.—Coruscations frequently ending in an arch 30° or 33° S. of the zenith; the southern edge of the arch passing 1° N. of the Pleiades. 1° N. of Scheat, 2° N. of the highest star in Delphinus, and just touching γ Aquila, and η Serpentis. *(W. H.)* From this time the streams and light diminished, and were subject to slight changes till

11. 0., when the sky became cloudy.

1m. W.N.W. of GOSPORT.

Hon. C. Harris.

Lat. 50° 48′ Long. W. about 1° 8′

Cirro-strati clouds obscured the auroral arch, which appeared soon after sun-set.

h. m.

9. 52½.—A beam in the W. between β and γ Ophiuchi. It seemed to swerve off gradually to the westward.

10. 4½.—It had faded away.

10. 49½. to 11. 4½. } Arch from N. W. to N. N. E. Its vertex under ζ Ursæ Majoris, and the edge of its base half-way between that star and the horizon.

CAMBRIDGE.

Professor Airy.

N. Lat. 52° 13′ Long. E. 0° 6′

h. m.

8. 25.—The aurora appeared in the form of a large bright cloud, bounded on the lower side by the horizon, and on the upper side by an arch of a small circle (not differing much from a great circle). The extremities of the arch were in the N. E. and W. N. W. or nearly W. The upper boundary was lower than β Ursæ Majoris by ⅔ × distance from α Ursæ Majoris to β Ursæ Majoris. Several small black clouds were scattered over the aurora-cloud, and above it were several faintly illuminated, whose light appeared to originate simply in the illumination of the aurora.

8. 35.—No change, except that the whole appeared to have moved a little to the west.

8. 58.—The form and brightness of the arch had not sensibly altered; but a long black cloud on its face attracted particular attention. The western extremity of this cloud was below γ Ursæ Majoris, its horizontal length fully three times the distance from β Ursæ Majoris to γ Ursæ Majoris, its vertical breadth less than one-fourth of its length, the eastern end being somewhat broader than the western. The aurora-cloud suddenly formed itself into streamers, (or streamers were formed in front of it) some perhaps 30° or 40° high, but lasting in this state only for an instant, and two streamers of sensible breadth shot up either in front of the black cloud or through it, so as to illuminate it, near its western extremity in two nearly vertical lines, corresponding to the course of the streamers, whose upper and lower parts were visible above and below the cloud. A remarkable change in the constitution of the cloud followed immediately; the western half became curdly, the upper edge of its small portions being luminous; the western half began to disappear; at 9. 15. no trace of the cloud discoverable.

9. 10.—A shooting star from E. to W. very nearly through δ Ursæ Majoris. The light of the aurora-cloud gradually diminished; the part which remained longest was a little E. of N. where some light was still visible at 10. 30.

GENERAL REMARKS.

This aurora was seen in many parts of Ireland from 9 to 11, and at later hours of the night of the 17th; as at Adare, Limerick, Armagh, and Dublin. Professor Lloyd found the *dipping-needle* to be unaffected by this aurora at Armagh. It was seen at Brussels by M. Quetelet, who in a letter to Professor Airy gives the following description of it. Towards 10 p. m. (Brussels time) an aurora borealis was visible, the dark segment was formed at about 12¼ h.; the phenomena ceased about 3 in the morning. There were no streamers, *(jets lumineux,)* and the light of a yellowish white colour, did not rise above the horizon more than 20 to 30 degrees. The greatest intensity of the aurora was in the direction of the magnetic meridian to the north.

State of the atmosphere—*York*. Temp. 50. Barom. 29.682 rising. On the 12th and 16th of September, auroral beams had been seen at York and Greta Bridge; on the 18th, at 8. 34. a low arch was seen there passing below the feet of U. Maj., its upper edge very near ι and λ of that constellation. At Durham, about 8. 0., two distinct arches were seen, the upper one 16°, and the lower and brighter one 7° (in the middle) above the horizon; at 8. 50. the lower edge of the lower arch was well defined, and 4° above the horizon. Auroral phenomena were also seen on this evening at Lymington, and a low arch was noticed by Lord Adare, near Limerick.

able for observations of these faint and often fluctuating meteors than others of a more refined construction.

2.) It is recommended that a magnetic needle be kept in a proper place, suspended by a silk fibre or slender hair, (a point-support not being delicate enough,) and so mounted that deviations can be observed to the accuracy of 1′. It has been found convenient to fix in a garden a stone pedestal, on which, at three invariable points, the frame of the magnetic needle rests under a glass cover. The needle, 9 inches long, and of such a weight as to perform about 10 vibrations in a minute, is suspended by one slender hair. There are simple contrivances to steady the needle when required, and to adjust the length of the suspending hair. The scale is divided in degrees for 30° on each side of the centre, and in 10′ for 1° on each side. There is no vernier, but the place of the needle on the scale is read off with great ease by looking through a fixed magnifying glass, from an opening at some height above, so as to avoid sensible parallax. Professor Christie has described more complete apparatus for this purpose, in the *Journal of the Royal Institution*, New Series, vol. ii. p. 278. The observer must leave his watch with the assistant, very carefully remove all keys, knives, and other things containing iron, from his dress, and all loose iron tools and utensils, to at least 20 feet distance from the needle. If these precautions are not scrupulously attended to, the results will be fallacious. It is proper to caution the observer that there is a *regular daily* variation of the needle, independent of the Aurora.

Dipping-needles, unless constructed with the utmost care, cannot be considered very satisfactory instruments; yet, if their suspension be sufficiently delicate, they may probably very well answer for observations during Aurora, of which the object is to determine not the *absolute dip of the needle*, but the *change of dip* occasioned by the Aurora. The same precautions of one certain position, removal of iron, &c., are necessary, as in the use of the horizontal needle.

3.) It is recommended that arrangements be made for ascertaining the error of a watch. If near an observatory of any kind, the watch should be compared with the transit clock there, immediately after an Aurora: if there is a good meridian line or good dial, the error of the watch on mean time should be found as soon as possible. If a watchmaker in the neighbourhood has a good regulator, the watch should be adjusted by it, and the mode of keeping the regulator should be ascertained: if a mail-coach from London passes near, the guard's watch may be consulted. The longitude of the place of observation should be ascertained from a map or otherwise. The attention of observers is especially called to the point of ascertaining the time correctly, as it is one of the most important points, and the one which probably will require the longest forethought.

4.) In default of intelligence of an Aurora, the observer should go out of doors to some station where the horizon is pretty clear, and look about every evening at 10, Greenwich mean solar time, as near as may be. He should keep a journal, noting for this time every evening whether there was an Aurora: a single word will be sufficient.

5.) As soon as the observer perceives or receives notice of an Aurora, he should, if accustomed to magnetic observations, observe the magnetic needle, and should go to some commanding situation with his watch in his hand, and a note-book. A person so prepared will have little difficulty in fixing on the appearances most worthy of notice. We may, however, point out the following:

I. If there is an arch, the positions of its two boundaries, its terminations, &c., should be noted by the way in which they pass among the stars, (the proportion of distances between the stars admitting of very accurate estimation by the eye). If, as rarely happens, the sky is cloudy, the observer may notice the elevation and extent of the arch, by moving till it appears to touch the top of some terrestrial object, noting his situation as well as he can, and the next day observing with a theodolite the angular elevation and azimuth of the object; or ascertaining the height and horizontal distance, and thence computing the angular elevation, and observing the azimuth by a common compass: but it is recommended not to adopt this method when the observation of stars is practicable. Notice should be taken whether one edge is better defined than the other; whether there is a clear sky or dark cloud above or below; whether it terminates at the end in sky or in cloud; whether there is any dark band in it; whether in its general composition it is uniform or striated; whether stars can be seen through it, &c.

II. If any change takes place in the situation or appearance of the arch, the observer should instantly look at his watch and set down the time, and then proceed to note the change.

III. If there are beams or streamers, the time should be noted; then their position among the stars; then their height among the stars; their motion (whether vertical or horizontal); the velocity of motion (by the time of passing from one star to another); their changes; their permanency; whether they appear to affect the arch, or to be entirely in front of it.

IV. If there are any black clouds in the luminous region, notice should be taken whether the streamers seem to have any relation to them; whether the arch seems to have any relation to them; whether and in what manner they increase or disappear.

V. If there are waves or flashes of light, the observer should notice the time of beginning and of finishing; the general extent of the flashes (up and down, as well as right and left); whether the flash is a real progress of light or successive illumination of different places; and anything else that strikes him.

VI. The existence and change of colours will, of course, be noticed.

VII. From time to time, the needle should be observed. If there are two persons capable of accurate observation, it is most desirable that one should steadily watch the needle, and the other the sky.

6.) When all is over, the observer should immediately put his rough notes in form, and as soon as possible should compare his watch with the regulator or other authority for his time.

7.) The next day he should, from a celestial globe, take the altitudes and azimuths by means of the stars; he should reduce his observed time to Greenwich mean solar time, and he should append these reductions to his rough observations. In this state the observations are fit for publication, and adapted for immediate use. It is desirable that they should be transmitted without delay to the *Assistant Secretary of the British Association, Museum, York.*

FALLING STARS.

M. Quetelet's mode of observing and recording the characteristic circumstances of these meteors is contained in the following extract of a letter from him:

"I take my station out of doors, in a situation which commands a good view of the sky, with a good map of the heavens spread out before me. When a falling star appears, I mark on the map the point of its commencement, the line of its course amongst the nearest stars, and the point where it vanished. This is done by an *arrow-line*, which marks the apparent direction and extent of the course of the meteor. The time is carefully noted; a number of reference is placed on the line, and the principal circumstances of the meteor are then registered in tables of the following form:

Epoch.	No.	Magnitude relative to Stars.	Duration of the Appearances.	Time of Appearance.	Remarks.
Aug. 29	1	2	$2''\cdot5$	$10^h\ 6'\ 4''$.	

It is important to remark whether the falling star leaves, or not, any *trace* of its course, as sometimes happens, in the form of reddish scintillations. The condition of the atmosphere, as determined by the usual instruments, should be noted: the time must be accurately ascertained. More than one observer should be engaged at each station, because the meteors sometimes succeed one another very quickly, and the duration of the phænomenon is too short to permit one person to note the position, time, and circumstances of each, with sufficient precision[1]."

CONSTANTS OF NATURE AND ART.

"Amongst those works of science which are too large and too laborious for individual efforts, and are therefore fit objects to be undertaken by united academies, I wish to point out one which seems eminently necessary at the present time, and which would be of the greatest advantage to all classes of the scientific world.

"I would propose that its title should be *The Constants of Nature and of Art*. It ought to contain all those facts which can be expressed by numbers in the various sciences and arts." (Babbage, *Edinburgh Journal of Science*, N. S., No. 12.)

The following extracts from Mr. Babbage's general plan of contents will exemplify the objects and arrangement of the proposed work.

These contents should consist of:

1. All the constant quantities belonging to our system;—as, distance of each planet,—period of revolution,—inclination of orbit, &c.,—proportion of light received from the sun,—force of gravity on the surface of each, &c.

[1] Contemporaneous observations are especially desirable on this subject. Persons desirous of undertaking the investigation are therefore requested to apply to a member of the Auroral Committee, or to the Assistant Secretary at York, for information of the evenings and hours appointed for this purpose.

2. The atomic weight of bodies.

3. List of the metals, with columns for specific gravity,—electricity,—tenacity,—specific heat,—conducting power for heat,—conducting power for electricity,—melting point,—refractive power,—proportion of rays reflected out of 1000,—at an incidence of 90°.

4. Specific gravities of all bodies.

5. List of mammalia, with columns for height,—length,—weight,—weight of skeleton,—weight of each bone,—its greatest length,—its smallest circumference,—its specific gravity,—number of young at a birth,—number of pulsations per minute,—number of inspirations per minute,—period of blindness after birth,—of sucking,—of maturity,—temperature,—average duration of life,—proportion of males to females produced, &c. &c.

After enumerating twenty such general heads of *Constants*, Mr. Babbage observes, that "most of them already exist, and that the difficulty of collecting them consists chiefly in a judicious selection of those which deserve the greatest confidence. It would be desirable, however," he adds, "to insert the heads of many columns, although not a single number could be placed in them; for they would thus point out many an unreaped field within our reach, which requires but the arm of the labourer to gather its produce into the granary of science." Mr. Babbage expresses his opinion, that if any scientific body of men would undertake to form such a collection, and to revise it from time to time, it would be a work fraught with advantages to knowledge, by continually leading to the more accurate determination of established facts, and to the discovery and measurement of new ones.

Persons desirous of undertaking or cooperating in the execution of any of the foregoing Recommendations, are requested to make known their intention to the Secretaries of the British Association, Museum, York.

PROSPECTUS

OF THE

OBJECTS AND PLAN

OF THE

STATISTICAL SOCIETY OF LONDON,

Founded on the 15*th March*, 1834.

The Statistical Society of London has been established for the purposes of procuring, arranging, and publishing "Facts calculated to illustrate the Condition and Prospects of Society."

The Statistical Society will consider it to be the first and most essential rule of its conduct to exclude carefully all *opinions* from its transactions and publications,—to confine its attention rigorously to facts,—and, as far as it may be found possible, to facts which can be stated numerically and arranged in tables.

The first operation of the Society will probably be to subdivide and organize its general council in such a manner as may enable that council to deal conveniently with all the subdivisions of the subject-matter before it. Those subdivisions will necessarily be numerous.

The whole subject was considered, by the Statistical Section of the British Association at Cambridge, as admitting a division into four great classes:

1. Economical Statistics.
2. Political Statistics.
3. Medical Statistics.
4. Moral and Intellectual Statistics.

If these four classes are taken as the basis of a further analysis, it will be found that the class of

Economical Statistics comprehend, 1st, the statistics of the natural productions and the agriculture of nations; 2ndly, of manufactures; 3rdly, of commerce and currency; 4thly, of the distribution of wealth, or all facts relating to rent, wages, profits, &c.

Political Statistics furnish three subdivisions: 1st, the facts relating to the elements of political institutions, the number of electors, jurors, &c.; 2ndly, legal statistics; 3rdly, the statistics of finance and of national expenditure, and of civil and military establishments.

Medical Statistics, strictly so called, will require at least two subdivisions; and the great subject of population, although it might be classed elsewhere, yet touches medical statistics on so many points, that it would be placed most conveniently, perhaps, in this division, and would constitute a third subdivision.

Moral and Intellectual Statistics comprehend, 1st, the statistics of literature; 2ndly, of education; 3rdly, of religious instruction and ecclesiastical institutions; 4thly, of crime. Although fourteen subdivisions have now been enumerated, it is probable that more will be required.

It will not of course be necessary to have a distinct Sub-committee of the Council for each of these subdivisions; but a convenient division of the Council, and an arrangement of the individuals composing it, so as best to deal with all the different portions of the common subject, will be a necessary preliminary to any systematic course of inquiry.

When these subdivisions are established, it will be for them, subject to the approbation of the Council, to sketch the outline of their own operations. A few observations on the more general efforts and objects of the Society are all that need be presented here.

It will be desirable that the Society should, as soon as possible, endeavour to open a communication with the statistical department established by Government at the Board of Trade. Without such a communication, constantly kept up, the Society can never be assured that it is not doing unnecessarily what the Government is doing at the same time and better. The result of such a communication would probably be that the Society would abandon to the care of the Government some part of this very extensive field of inquiry altogether, and more of it partially, which would still leave a very sufficient, though a less overwhelming task to the Society.

The Society, having its own work thus somewhat limited and defined, may next proceed to consider the best means, 1st, of collecting fresh statistical information; and, 2ndly, of arranging, condensing, and publishing much that already exists. Towards collecting fresh statistical information, the first step in order, both of time and importance, would be the arrangement of a good set of interrogatories, to be drawn up under the superintendence of the Sub-committees, and afterwards examined, sanctioned, and circulated by the Council. The careful execution of this task is essential both to afford guidance and aid to individual inquirers, and to protect the Society against the influx of imperfect or irrelevant statements. Willing agents of inquiry exist in abundance quite ready to aid in collecting materials; but few of these agents take a very wide view of all the objects of statistical inquiry, and indeed few have very distinct notions about the precise information the Society may wish to collect, even as to any one object. To sketch, therefore, distinctly by means of interrogatories, carefully and succinctly drawn, the whole outline which it is wished to fill up, is the only way to secure to the Society the full benefits to be expected from their zeal. It is difficult to overrate the importance of the step which will be made towards the accumulation of statistical knowledge from all quarters of the globe, by the publication of such a set of questions; but the operation will be as laborious as it is important. It properly may, and probably will, form the chief object of the exertions of the Council during the first year of the Society's existence.

Obvious advantages may be drawn from communication with intelligent Englishmen about to travel abroad, with residents in the Colonies, and with colonial gentlemen resident in England. The Society has already the satisfaction of knowing, that it will have friends and assistants equally zealous and able in our western colonial possessions. Various societies, foreign and domestic, abound both in our own country and on the Continent, some of them already devoted to this subject, and others very willing to take it up. In addition to those already in existence, the Society may hope to see other local societies springing up in every part of the British dominions, in direct and constant connexion with the London Society, circulating its queries in their immediate neighbourhood, and collecting and authenticating the answers. A body of facts can be thus most conveniently collected, which may properly enter into a common publication, and will afford safe grounds for comparing the present condition and future progress of different parts of the empire. The London Society, therefore, will carefully cultivate a connexion with, and attend to the wishes and suggestions of, such local societies, and will look forward to their multiplication and correspondence as among the best supports of its own continued efficiency.

The collection, by such means and agents, of new statistical materials will form, it will be remembered, only one part of the Society's work. To condense, arrange, and publish those already existing, but either unpublished, or published only in an expensive or diffused form, or in foreign languages, would be a task of equal usefulness. Authentic statistical accounts, even of an old date, may perhaps advantageously receive some attention. Our Oriental dominions alone present a field of statistical research as interesting as it is immense. Many materials, collected from that field by the meritorious exertions of the East India Company, are known to be in existence, and it is to be hoped that, sooner or later, they will be brought through some channel before the public.

To point out such existing collections, old and new, their character, value, and the degree of interest attached to them, will form an appropriate part of the duties of the Sub-committees of the Council, and will itself be a considerable step in statistical knowledge. The extent to which the Society shall deal with the existing materials so pointed out to it, can only be considered when the means and resources it is to possess are better ascertained.

It will of course be one prominent object of the Society to form a complete Statistical Library as rapidly as its funds may admit.

The proposed annual subscription to the Society is two guineas, which may be compounded for by one payment of twenty guineas.

COUNCIL AND OFFICERS

Elected at the General Meeting, 3rd May, 1834.

President.—Marquis of Lansdowne.

Treasurer.—Henry Hallam, Esq.

Secretaries.—Woronzow Greig, Esq. Charles Hope Maclean, Esq. E. Carleton Tufnell, Esq.

Charles Babbage. Esq. William Burge, Esq. Rev. Geo. D'Oyley, D.D. John Elliot Drinkwater, Esq. Howard Elphinstone, Esq. Earl Fitzwilliam. Rt. Hon. H. Goulburn, M.P. Joseph Henry Green, Esq. Edmund Halswell, Esq. Dr. Bisset Hawkins, M.D. Rt. Hon. Fr. Jeffrey, M.P. Rev. Richard Jones. John Lefevre, Esq. Sir Charles Lemon, Bart., M.P. Rt. Rev. Lord Bishop of London. S. Jones Loyd, Esq. Rev. T. R. Malthus. G. R. Porter, Esq. Viscount Sandon, M.P. G. Poulett Scrope, Esq., M.P. N. W. Senior, Esq. Dr. John Sims, M.D. Lieutenant-Colonel Sykes. Thomas Tooke, Esq. T. Vardon, Esq. Rev. W. Whewell.

OBJECTS OF THE ASSOCIATION.

The Association contemplates no interference with the ground occupied by other Institutions. Its objects are,—To give a stronger impulse and a more systematic direction to scientific inquiry,—to promote the intercourse of those who cultivate Science in different parts of the British Empire, with one another, and with foreign philosophers,—to obtain a more general attention to the objects of Science, and a removal of any disadvantages of a public kind, which impede its progress.

RULES.

MEMBERS.

All Persons who have attended the first Meeting shall be entitled to become Members of the Association, upon subscribing an obligation to conform to its Rules.

The Fellows and Members of Chartered Societies in the British Empire shall be entitled, in like manner, to become Members of the Association.

The Office-Bearers, and Members of the Councils or Managing Committees, of Philosophical Institutions shall be entitled, in like manner, to become Members of the Association.

All Members of a Philosophical Institution, recommended by its Council or Managing Committee, shall be entitled, in like manner, to become Members of the Association.

Persons not belonging to such institutions, shall be eligible, upon recommendation of the General Committee, to become Members of the Association.

SUBSCRIPTIONS.

The amount of the Annual Subscription shall be One Pound, to be paid in advance upon Admission; and the amount of the composition in lieu thereof, Five Pounds.

MEETINGS.

The Association shall meet annually, for one week, or longer.

The place of each Meeting shall be appointed by the General Committee at the previous Meeting; and the Arrangements for it shall be entrusted to the Officers of the Association.

GENERAL COMMITTEE.

The General Committee shall sit during the time of the Meeting, or longer, to transact the Business of the Association. It shall consist of all Members present, who have communicated any scientific Paper to a Philosophical Society, which Paper has been printed in its Transactions, or with its concurrence.

Members of Philosophical Institutions, being Members of this Association, who may be sent as Deputies to any Meeting of the Association, shall be Members of the General Committee for that Meeting.

COMMITTEES OF SCIENCES.

The General Committee shall appoint, at each Meeting, Committees, consisting severally of the Members most conversant with the several branches of Science, to advise together for the advancement thereof.

The Committees shall report what subjects of investigation they would particularly recommend to be prosecuted during the ensuing year, and brought under consideration at the next Meeting. They shall engage their own Members, or others, to undertake such investigations; and where the object admits of being assisted by the exertions of scientific bodies, they shall state the particulars in which it might be desirable for the General Committee to solicit the cooperation of such bodies.

The Committees shall procure Reports on the state and progress of particular Sciences, to be drawn up from time to time by competent persons, for the information of the Annual Meetings.

LOCAL COMMITTEES.

Local Committees shall be appointed, where necessary, by the General Committee, or by the Officers of the Association, to assist in promoting its objects.

Committees shall have the power of adding to their numbers those Members of the Association whose assistance they may desire.

OFFICERS.

A President, two Vice-Presidents, two or more Secretaries, and a Treasurer, shall be annually appointed by the General Committee.

COUNCIL.

In the intervals of the Meetings the affairs of the Association shall be managed by a Council, appointed by the General Committee.

PAPERS AND COMMUNICATIONS.

The General Committee shall appoint, at each Meeting, a Sub-Committee, to examine the papers which have been read, and the register of communications; to report what ought to be published, and to recommend the manner of publication. The Author of any paper or communication shall be at liberty to reserve his right of property therein.

ACCOUNTS.

The Accounts of the Association shall be audited, annually, by Auditors appointed by the Meeting.

TREASURER.

John Taylor, Esq., 14, Chatham Place, London.

LOCAL TREASURERS.

Dr. Daubeny, Oxford.
Prof. Forbes, Edinburgh.
Jonathan Gray, Esq., York.
Prof. Henslow, Cambridge.
William Hutton, Esq., Newcastle-on-Tyne.
Rev. Thomas Luby, Dublin.
Dr. Prichard, Bristol.
George Parsons, Esq., Birmingham.
Rev. John J. Tayler, Manchester.
H. Woolcombe, Esq., Plymouth.

INDEX.

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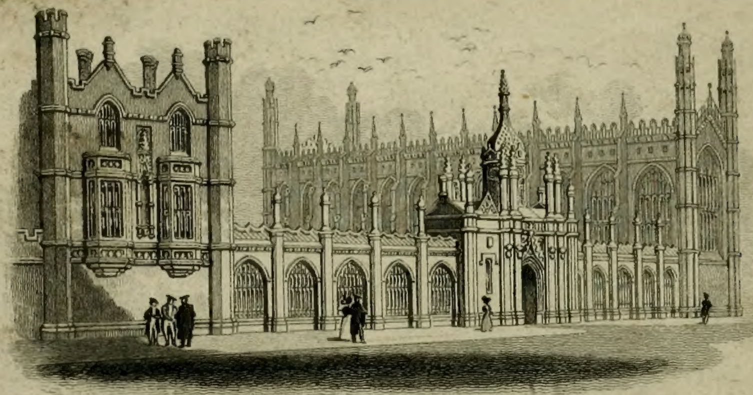

British Association,

for the Promotion of Science.

3.^{d} *Meeting* CAMBRIDGE, *June* 1833.

No 13

W. Clift