THE

FUEL OF THE SUN.

BY

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AUTHOR OF

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TO ZACHARIAH WATKINS, ESQ.,

I Dedicate this Volume,

IN GRATEFUL AND AFFECTIONATE ACKNOWLEDGMENT OF HIS SHARE IN ITS PRODUCTION, BY THE GENEROUS AID AND JUDICIOUS ENCOURAGEMENT HE AFFORDED ME DURING THE EDUCATIONAL STRUGGLES OF MY EARLY YOUTH.

W. MATTIEU WILLIAMS.

PREFACE.

THIS little work is an attempt to explain some of the greatest mysteries of the Universe. I am fully conscious of the boldness of the effort, and that I fairly expose myself to the accusation of extreme presumption. Nevertheless I do not hesitate to publish these speculations, knowing that whatever may be the verdict of the criticism to which they must be subject, they are earnest and conscientious efforts towards the discovery of truth.

In the course of such a work I may have made some mistakes, which, though not visible to myself, will be seen by others whose scientific education has been more complete than my own. I only request that if such errors are detected, they may be corrected in the same spirit as that in which they were made. If so, I shall be the first to acknowledge most gratefully my obligations to whoever may set me right.

Grimesthorpe Road,
Sheffield,
January, 1870.

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THE FUEL OF THE SUN.

CHAPTER I.

THE ATMOSPHERE UNLIMITED.

THE VARYING DENSITY OF THE EARTH'S ATMOSPHERE—THE LIMITS OF THE ATMOSPHERE ACCORDING TO THE FORMULÆ OF LAPLACE AND LESLIE—DISCREPANCIES IN THE ESTIMATES OF THE HEIGHT OF THE EARTH'S ATMOSPHERE—QUETELET'S "STABLE ATMOSPHERE"—ABSOLUTE WEIGHT OF ATOMS UNKNOWN—FRICTION OF "LUMINIFEROUS ETHER" UPON THE SURFACE OF A LIMITED ATMOSPHERE—THE ATMOSPHERE UNLIMITED—INFLUENCE OF DR. WOLLASTON'S PAPER "ON THE FINITE EXTENT OF THE ATMOSPHERE."

1. The atmosphere which clings about our globe, and travels with it in its yearly flight and daily rolling, varies in density as it varies in distance from the earth's surface; each succeeding layer being rarer than that below it. This is so well understood, that I need not describe any of the particular observations upon which the induction is based. We know, by direct observation, how much lighter is the atmosphere at *certain* given distances from the sea level, when the temperature and other conditions are given; but we do not know this at *any* given distance, the limit of our knowledge being the height of accessible mountains.*

* Baloon ascents have added nothing to our knowledge in this direction, barometrical variations being the only measures used in determining the heights attained. It would be an interesting experiment to make a series of trigonometrical observations from the two ends of a measured base line on the earth, simultaneously with the barometrical observations of the aeronaut, and to compare the results.

2. Laplace, by carrying upwards his formula contrived for barometrical measurements, states, that at a height of 52,986 metres, or rather less that thirty-three miles, the atmosphere is thinned out to a density only equal to the utmost degree of rarefaction obtainable in an air pump of the best construction. Leslie's formula for barometrical measurements may be simply stated as follows. Observe the height of the barometer at any two places; then, as the sum of these heights of the mercury is to their difference, so is 52,000* to the difference of the height of the two stations measured in feet. This, with corrections for temperature, affords a fair practical rule for the measurement of attainable elevations in the temperate zone. If, however, we carry it upwards to an imaginary station on the supposed surface-level of the atmospheric ocean where the barometer must stand at 0, it gives us 52,000 feet, or less than ten miles, as the height at which the atmosphere terminates.

3. In meteorological treatises the average height of the atmosphere is usually stated to be forty or fifty miles. Where the subject is lightly treated, or the motto of the book-maker is "aut scissors aut nullus," there is little discrepancy in this estimate; but among authors of original authority the discrepancies are very considerable. For example, in the article "Atmosphere," in the *Encyclopædia Britannica* (8th edition), page 180, the writer says:—"But if air consists of atoms incapable of further division, it is obvious that the height of the atmosphere has a limit, and that limit is the place where the gravitation of the atoms of the air just balance the force of their repulsion. The exact situation of this limit we cannot assign; but it cannot far exceed the height of forty-five miles above the earth's surface." In the article "Pneumatics," in the same edition of the same work, I find the following on page 27. "Now it is easy to show, that the light which gives us what we call the twilight, must be reflected from the height

* Mr. Alex. J. Ellis gives 52,400 instead of 52,000. *Reader*, vol. iv., p. 108.

of at least fifty miles; for we have it, when the sun is depressed, eighteen degrees below the horizon." In the same article, page 56, the data upon which this calculation is made are given and illustrated, and the limit is concluded "to be about forty-five miles;" but the writer, Sir John Robison, adds that "a very sensible illumination is perceptible much further from the sun's place than this, *perhaps twice as far, and the air is sufficiently dense for reflecting a sensible light at a height of nearly two hundred miles.*" Now if this is the case, what becomes of "the place where the gravitation of the atoms of the air just balance the force of their repulsion," and which "cannot far exceed the height of forty-five miles above the earth's surface?"

4. Other observations besides those on atmospheric reflection, afford direct refutation of this usually assumed limit of forty-five miles. As an example of these I make take the detonations and other phenomena presented by luminous meteors, from which M. Quetelet has inferred the existence of what he calls the "stable atmosphere" occupying the region extending from forty to eighty miles above the earth's surface.

5. I am utterly at a loss to understand upon what evidence Dr. Thomas Thompson, the writer of the article "Atmosphere" above quoted, and other writers who use the same expressions, base their determination of "the place where the gravitation of the atoms of the air just balances the force of their repulsion." For such determination they would require to know the absolute weight of the ultimate atom. We are at the present day just as far from a knowledge of that quantity as the wrangling pedants of the middle ages were from the solution of their much vexed question, of how many human souls could stand on the point of a needle.

6. There is a serious objection to this hypothesis of atomic limitation of the earth's atmosphere, which seems to have escaped the attention of all the writers on this subject. The atmosphere is supposed to terminate just where the reduced elasticity of the last layer but one of the atmospheric atoms

exactly equals and balances the gravitation of the atoms of the last layer of all. These surface atoms must therefore rest upon their spring couch with the lightest conceivable touch; their downward gravitating tendency being counterbalanced by the outflying impulse of their own gaseous elasticity and that of the atoms below them. They must be in a state of utterly unstable equilibrium, which, combined with their perfect fluidity, would leave them subject to the smallest conceivable force. Any impulse, however slight, would carry them away in its own direction. Now the carefully observed and calculated movements of Encke's and Halley's comets have proved the existence, in planetary space, of a medium which has been called "the luminiferous ether," offering an appreciable and measurable degree of mechanical resistance to the motion of such attenuated matter as that of which comets are composed. What must be the action of such a resisting medium upon the supposed boundary atoms of the atmosphere of our planet as it is rushing through it with its orbital velocity of nearly two millions of miles per day? Obviously, to brush them off the surface of the atmospheric ocean, and leave them deposited in the midst of the luminiferous ether, as a gale of wind lodges the sea spray on a lee shore. The outer atoms thus removed would leave the next below in the same condition of unstable equilibrium; for, according to this atomic theory, it matters not how great or how little be the extent of such an atmosphere of aggregated atoms, the outer layer must be subject to no pressure beyond that of its own gravitation, and that must be neutralized by its own elasticity and that of the next below. Thus would this newly exposed stratum of atoms be swept away by the merciless ether: then another, and another again, till the planet would be stripped bare to the bottom of its dry-land vallies. Then, the ocean, relieved of the pressure which restrains the volatilizing power of the sun's rays, would spring upwards into the condition of gaseous elasticity, forming another atmosphere subject to the same laws as the first, which would in like manner be swept away from the surface of the earth, leaving it all in a condition of arid lunar barrenness. Even if we regard the luminiferous ether as planetary matter, and suppose that its particles are subject to the laws of planetary motion, and thus travel in company with the earth in its orbital revolution, there still remains the earth's axial rotation (an independent speciality of each planet), which must give varying velocities of translation to the outer portion of this supposed limited atmosphere, from one thousand miles per hour at the equatorial regions to atomic rotation at the poles. In addition, there is the travelling of the whole solar system through space at the rate of four hundred thousand miles per day, all tending to the stripping off of the supposed limited atmosphere.

7. The more I examine the observations and reasonings upon which this forty-five mile boundary of the atmosphere is based, the farther and farther it recedes, and every other limit in like manner widens away when the light of experimental demonstration is brought to bear upon it. I shall endeavour to show, in the course of the following pages, good and sufficient reasons for rejecting *all* the assumptions and calculations upon which the supposed limitation of our atmosphere is based, and for concluding that the gaseous ocean in which we are immersed is but a portion of the infinite atmosphere that fills the whole solidity of space;—that links together all the elements of the universe, and diffuses among them their heat and light, and all the other physical and vital forces which heat and light are capable of generating.

8. This question of the limits of the atmosphere appears to me to be one of the most important and fundamental enquiries that the whole range of physical science includes. It is very remarkable that its critical consideration should have been so entirely neglected during the last half century of unexampled philosophical activity. Instead of examining the question independently, the general habit has been to accept the conclusions of Dr. Wollaston on this subject, as a sort of philosophical

inheritance whose title deeds are so safely registered in the *Philosophical Transactions* that their further examination is unnecessary. I allude, of course, to the conclusions expounded in his celebrated paper "On the Finite Extent of the Atmosphere," the examination of which I propose to undertake in the next chapter.

CHAPTER II.

AN EXAMINATION OF DR. WOLLASTON'S PAPER "ON THE FINITE EXTENT OF THE ATMOSPHERE."

DR. WOLLASTON'S PAPER TO BE EXAMINED—REPRINT OF DR. WOLLASTON'S PAPER—PROGRESS OF VACUUM EXPERIMENTS SINCE 1822—VACUUM EXPERIMENTS SHOW NO LIMIT TO ATMOSPHERIC EXPANSIBILITY—ELECTRICAL DISCHARGES CONTROVERT ATOMIC HYPOTHESIS—INFLUENCE OF THE ATOMIC THEORY IN WOLLASTON'S TIME—THE USE AND ABUSE OF THE ATOMIC THEORY—ATOMS TO BE DISCUSSED HEREAFTER—DR. WOLLASTON'S "REDUCTIO AD ABSURDUM"—DR. WOLLASTON'S METHOD OF CALCULATING THE ATMOSPHERE DUE TO THE SUN UPON THE ASSUMPTION OF UNLIMITED EXPANSIBILITY—FALLACY OF DR. WOLLASTON'S METHOD OF CALCULATING THE SOLAR ATMOSPHERE—DR. WOLLASTON'S CALCULATION OF JUPITER'S ATMOSPHERE—CONTRADICTION IN DR. WOLLASTON'S CALCULATION OF JUPITER'S ATMOSPHERE—DR. WOLLASTON'S CALCULATION OF LUNAR ATMOSPHERE FALLACIOUS—GENERAL CONCLUSIONS RESPECTING DR. WOLLASTON'S PAPER.

9. It may appear to be an act of serious presumption on the part of so humble a student of science as myself, to attempt a refutation of this paper, which has stood since 1822 as an unquestioned portion of established science, and which is the work of so great a philosopher. I attempt it nevertheless, having far too much of just and true respect for the memory of Dr. Wollaston to misuse his great reputation so unnaturally as to allow it to stand in the way of any form or degree of strict investigation. In order to assist the reader in connecting my arguments with the conclusions of Dr. Wollaston, I have reprinted this important paper from the *Philosophical Transactions,* and lettered the paragraphs for convenience of reference.

10. *On the Finite Extent of the Atmosphere.* By William Hyde Wollaston, M.D., V.P.R.S.—Read January 17th, 1822.

(A.) "The passage of Venus very near to the Sun in superior conjunction in the month of May last, having presented an opportunity of examining whether any appearance of a solar atmosphere could be discerned, I am in hopes that the result of my endeavours, together with the views which have induced me to undertake the enquiry, may be found deserving of a place in the *Philosophical Transactions*.

(B.) "If we attempt to estimate the probable height to which the earth's atmosphere extends, no phenomenon caused by its refractive power in directions at which we can view it, or by reflection from vapours that are suspended in it, will enable us to decide this question.

(C.) "From the law of its elasticity, which prevails within certain limits, we know the degrees of rarity corresponding to different elevations from the earth's surface, and if we admit that air has been rarefied so as to sustain $\frac{1}{100}$ of an inch of barometrical pressure, and that this measure has afforded a true estimate of its rarity, we should infer from the law, that it extends to the height of forty miles, with properties yet unimpaired by extreme rarefaction. Beyond this limit we are left to conjectures founded on the supposed divisibility of matter: and if this be infinite, so also must be the extent of our atmosphere. For, if the density be throughout as the compressing force, then must a stratum of given thickness at every height be compressed by a superincumbent atmosphere, bearing a constant ratio to its own weight, whatever be its distance from the earth. But if the air consist of any ultimate particles no longer divisible, then must expansion of the medium composed of them cease at that distance, where the force of gravity downwards upon a single particle is equal to the resistance arising from the repulsive force of the medium.

(D.) "On the latter supposition of limited divisibility, the atmosphere which surrounds us will be conceived to be a medium of finite extent, and may be peculiar to our planet, since its properties would afford no ground to presume that similar matter exists in any other planet. But if we adopt the hypothesis of unlimited expansion, we must conceive the same kind of matter to pervade all space, where it would not be in equilibrio, unless the sun, the moon, and all the planets possess their respective shares of it condensed around them, in degrees dependent on the force of their respective attractions, excepting in those instances where the tendency to accumulate may be counteracted by the interference of other kinds of matter, or of other powers of which we have no experience, and concerning which we cannot expect to reason correctly.

(E.) "Now, though we have not the means of ascertaining the extent of our own atmosphere, those of other planetary bodies are nevertheless objects for astronomical investigation; and it may be deserving of consideration, whether, in any instance, a deficiency of such matter can be proved, and whether, from this source, any conclusive argument can be drawn in favour of ultimate atoms in general. For, since the law of definite proportions discovered by chemists is the same for all kinds of matter, whether solid, or fluid, or elastic, if it can be ascertained that any one body consists of particles no longer divisible, we then can scarcely doubt that all other bodies are similarly constituted; and we may without hesitation conclude that those equivalent quantities which we have learned to appreciate by proportionate numbers, do really express the relative weights of elementary atoms, the ultimate objects of chemical research.

(F.) "These reflections were originally suggested by hearing an opinion hazarded without due consideration, that the non-existence of perceptible atmosphere around the moon might be regarded as conclusive against the indefinite divisibility of matter. There was, however, an oversight in this inference, as the quantity of such matter, which the moon would retain around her, could not possibly be perceived by the utmost power of any instruments hitherto invented for astronomical purposes. For, since the density of an atmosphere of infinite divisibility at her surface would depend on the force of her gravitation at that point, it would not be greater than that of our atmosphere is where the earth's attraction is equal to that of the moon at her surface. At this height, which by a simple computation is about five thousand miles from the earth's surface, we obviously can have no perceptible atmosphere, and consequently should not expect to discern an atmosphere of similar rarity around the moon.

(G.) "It is manifestly in the opposite direction that we are to look for information. We should examine first that body which has the greatest power, and see whether even there the non-appearance of those phenomena which might be expected from such an atmosphere will warrant the inference that our own is confined to this one planet by the limit set to its divisibility.

(H.) "By converse of the same rule which gives an estimate of extreme rarity at the moon's surface, we may form a conception at what distance round the sun refraction from such a cause should be perceived. If we calculate at what apparent distance from the body of the sun his force is equal to that of gravity at the surface of the earth, it is there that his power would be sufficient to accumulate (from an infinitely divisible medium, filling all space) an atmosphere* fully equal in density to our own, and consequently producing a refraction of more than one degree, in the passage of rays obliquely through it.

(I.) "If the mass of the sun be considered as three hundred

* Such an atmosphere would, in fact, be of greater density on account of the far greater extent of the medium affected by the solar attraction, although of extreme rarity; but the addition derived from this source may be disregarded in the present estimate, without prejudice to the argument, which will not be found to turn upon any minute difference.

and thirty thousand times that of the earth, the distance at which his force is equal to gravity will be $\sqrt{330,000}$, or about 575 times the earth's radius; and if his radius be 111·5 times that of the earth, then this distance will be $\frac{575}{111 \cdot 5}$ or 5·15 times the sun's radius; and $15' \; 49'' \times 5 \cdot 15 = 1^{\circ} \; 21' \; 29''$ will be the apparent distance from the sun's centre, on the 23rd of May, when the following observations were made.

(K.) "What deduction should be allowed for the effect of heat, it may be time to consider when we have learned the amount of apparent refraction at some given distance; and we may then begin to conjecture, whether heat can counteract the increase of density that would occur in the approach of only $\frac{1}{10}$ of a second towards his centre.*

(L.) "As I had not any instrument in my possession that I considered properly adapted for the purpose, I requested the assistance of several astronomical friends in watching the progress of Venus to the sun for some days preceding superior conjunction, and in recovering sight of her afterwards. But neither the Astronomer Royal at Greenwich, nor Professor Brinkley of Dublin, nor Mr. South, with the admirable instruments they possess, were able to make any observation within the time required, not being furnished with the peculiar means adapted to this enquiry.

(M.) "Captain Kater, however, who entered fully into my views, and engaged in the prosecution of them with all the ardour necessary for success, by using a reflecting telescope, was able to furnish me with a valuable set of observations three and a half days preceding conjunction, which, together with

* If we attempt to reason upon what would be the progressive condensation of such an atmosphere downwards towards the surface of the sun, we are soon stopped by the limit of our experience as to the degree of condensation of which the atmosphere is susceptible. If we could suppose the common law of condensation to extend as far as forty-six miles in depth, the density corresponding to it would be about equal to that of quicksilver, from whence a refraction would occur exceeding all bounds of reasonable calculation. A space of forty-six miles, at the distance of the sun from us, would subtend about $\frac{1}{10}$ of a second.

those in which I had the good fortune to succeed at nearly an equal interval subsequent to the passage, afford data quite sufficient to show that no refraction is perceptible at the period of our observations; and these come far within the specific distances above estimated.

"A selection from the series given to me by Captain Kater is contained in the following table." (Here follow details of observations upon which the following conclusions are based.)

(N.) "It is evident that in these observations the differences between the observed and calculated places of the planet are not such as to indicate a refraction that can be relied on; and from the observations of Captain Kater, no retardation of the motion of Venus can be perceived in her progress toward the sun, as would occur from increasing refraction; and by comparison of her motion in the interval between his last observation and my own, with her change of place for the same interval given in the *Nautical Almanac,* there seems no ground whatever to suppose that her apparent position has been in the least affected by refraction through a solar atmosphere, although the distance at the time of Captain Kater's last observation was but 65′ 50″ from the sun's centre, and at the time of my own 53′ 15″.

(O.) "Although these distances appear but small, I find that Venus has been seen at a still less distance by Mons. Vidal of Montpellier in 1805. On the 30th of May he observed Venus 3^{m} 16″ after the sun, when their difference of declination was not more than 1′, so that her distance from the centre was about 46′ of space. Since his observations also accord with the calculated places of Venus, they might have superseded the necessity of fresh observations, if I had been duly aware of the inference to be drawn from them.

(P.) "The same skilful observer has also recorded an observation of Mercury on the 31st of March of the same year, when he was seen at about 65′ from the sun's centre." (Here follows description of instrument used by Dr. Wollaston for his observations.)

(Q.) "In the foregoing remarks I have perhaps dwelt more upon the consideration of the solar atmosphere than may seem necessary to those who have considered the common phenomena observable in the occultation of Jupiter's satellites by the body of the planet. Their approach, instead of being retarded by refraction, is regular, till they appear in actual contact; showing that there is not that extent of atmosphere which Jupiter should attract to himself from an infinitely divisible medium filling space.

(R.) "Since the mass of Jupiter is full three hundred and nine times that of the earth, the distance at which his attraction is equal to gravity must be as $\sqrt{309}$, or about 17·6 times the earth's radius. And since his diameter is nearly eleven times greater than that of the earth, $\frac{17\cdot6}{11}=1\cdot6$ times his own radius will be the distance from his centre, at which an atmosphere equal to our own should occasion a refraction exceeding one degree. To the fourth satellite this distance would subtend an angle of about 3° 37′, so that an increase of density to three and a half times our common atmosphere would be more than sufficient to render the fourth satellite visible to us when behind the centre of the planet, and consequently to make it appear on both (or all) sides at the same time.

(S.) "The space of about six miles in depth, within which this increase of density would take place, according to known laws of barometric pressure, would not subtend to our eye so much as $\frac{1}{300}$ of a second, a quantity not to be regarded in an estimate, where so much latitude has been allowed for all imaginable sources of error.

(T.) "Now though, with reference to the solar atmosphere, some degree of doubt may be entertained in consequence of the possible effects of heat which cannot be appreciated, it is evident that no error from this source can be apprehended in regard to Jupiter; and as this planet certainly has not its due share of an infinitely divisible atmosphere, the universal prevalence of such a medium cannot be maintained; while, on the contrary, all the phenomena accord entirely with the supposition that the earth's atmosphere is of finite extent, limited by the weight of ultimate atoms of definite magnitude no longer divisible by repulsion of their parts."

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11. In (C) Dr. Wollaston starts upon the assumption that the limit of actual experimental rarefaction of air is reached when the residual air shall sustain $\frac{1}{100}$ of an inch, and he estimates this to be the degree of atmospheric rarity which exists at forty miles above the surface of the earth. Since this paper was written, wonderful progress has been made in the mechanical refinements of experimental research. Dr. Thomas Andrews, of Queen's College, Belfast, has succeeded in obtaining and measuring a degree of rarefaction *one thousand times greater than this.* By a more refined elaboration of Dr. Andrews's principle of combining chemical and mechanical forces, Mr. Grove and Mr. Gassiot have subsequently succeeded in obtaining a still higher degree of rarefaction, and M. Geissler has proceeded further than this, and produced what really appears to be a perfect vacuum.

12. Now if the ultimate atmospheric atoms upon which Dr. Wollaston bases his speculations had any such existence as he supposes, these experimentalists must have encountered them;—they must have reached that point of rarefaction "where the force of gravity downwards upon a single particle is equal to the resistance arising from the repulsive force of the medium." At this point the air would cease to manifest the expansive elasticity which is the characteristic of gaseous matter; it would fall to the bottom of the vessel, would "find its level," and otherwise behave like water or any other liquid. Dr. Lardner (*Handbook of Natural Philosophy*), after defining the atmospheric limit in accordance with Dr. Wollaston, says, "If a particle of

air were raised above this height by the application of any external agency, and then disengaged, it would drop by its gravity to the surface of the atmosphere, in the same manner and by the same law that makes a stone drop to the ground." Now if these ultimate atoms, whose gravitation is capable of exceeding their mutual repulsion, have any existence, they must have been found in this condition somewhere between the point of rarefaction, when Dr. Wollaston expected to find them, and the vastly greater rarefaction obtained by Dr. Andrews,— or if not here, between this and the actual vacuum of Geissler. At some such point either Dr. Andrews, Mr. Grove, Mr. Gassiot, or Mr. Geissler, should have observed a sudden termination of gaseous properties, and the conversion of the remaining air into a liquid, whose particles, if raised by the application of an external agency and then disengaged, would, as Dr. Lardner says, drop by their gravity to the liquid surface. M. Geissler would thus be able to produce tubes in which the upper portion should be a perfect vacuum, while on the lower part would be a layer or layers of these atoms, which, on turning the tubes around, would roll about (I will not say rattle) like the liquid in a water-hammer. To the experimental physicist who is practically acquainted with the actual properties of rarefied gases, the suggestion of such a state of things appears ridiculous. Nevertheless the meteorologist takes it for granted that it must be so.

13. According to the atomic theory, the cessation of residual elastic force must in the course of these experiments have been sharp and sudden, and could not have escaped the keen observing powers of the experimentalists above named. It would have become especially and strikingly manifest by the phenomena of the electrical discharges, for the investigation of which these high degrees of rarefaction were especially devised. Such discharges from the Rumkorff coil were found by Mr. Gassiot to be the most delicate of all tests of the presence of ponderable matter; at each further step of exhaustion, changes in the

appearances of the streams and stratification of the electric current were found to occur, until at last, when all matter was removed, the passage of the electric discharge was stopped altogether for want of matter to convey it. When the potash in the vacuum tube was heated, and the smallest conceivable quantity of matter was thereby diffused through it, this infinitesimal atmosphere again transmitted the discharge, which again was stopped upon the recompletion of the vacuum. Now had there been a stratum of atmospheric atoms resting by preponderant gravitation on the lower portion of the tube, the electric discharge would have been transmitted by these alone, and their boundary would be indicated by their consequent illumination, while the upper vacuous space must have remained in darkness.

14. It will be seen, by reference to (E), that one of Dr. Wollaston's objects was to find further support for the atomic theory, and it must be borne in mind that at the time this paper was written the atomic theory was a recent scientific conquest, and had great hold upon the minds of most philosophers; for by it the hitherto wild, undisciplined and unmanageable multitude of chemical facts were reduced to order, and made subject and submissive to human mathematics. Philosophers are not altogether free from the pride of conquest and other forms of human weakness, (they would be miserably unpleasant people if they were), and the surface of philosophy is not absolutely level: it has its undulations, and though far from partaking of the turbulence of the ever-troubled waters of passion and prejudice, still there are certain long low waves of philosophic fashion which are perceptible when observed from a distance. At this period, ultimate atoms of definite weight were as fashionable as molecular oscillations and gyrations are at the present time. In recent works, the sceptically suggestive terms, "chemical equivalent" and "equivalent quantity," have largely usurped the place of "atom" and "atomic weight;" and I think I may venture to say that philosophic conviction is beginning

gradually to slide away from this decisive belief in the actual existence of ultimate atoms.

15. For my own part I plead guilty of an utter and most uncompromising infidelity to this atomic theory. I can nowhere find any sort of *direct* evidence in support of the existence of any ultimate atoms, or of any necessary limit to the divisibility of any kind of matter. It is true that we can form no idea of infinite smallness, just as we fail to grasp the idea of infinite space, but our reason compels us to admit the existence of the latter, and to attribute its incomprehensibility by ourselves merely to the limited range of human intellect. As a conventional scientific hypothesis, an elegant device to assist the intellect in understanding and generalizing the quantitative laws of chemical composition, the atomic theory has unquestionably done good service in feeding the human intellect. In this way it has performed, and is still performing, the same function that *phlogiston, caloric, the electric fluid*, etc., did in their days; but to set up such an hypothetical creation as a fact, and to make it the basis of reasoning up to great conclusions respecting the physical order of nature, is to erect another of those idols which Lord Bacon, the great intellectual Iconoclast, laboured so powerfully to destroy. All those writers on Pneumatics and Meteorology who, with Dr. Lardner, say that the atmosphere is limited *because* its atoms, if raised above a certain point, must fall back again by their own gravitation, are, even at this date, still in destitute need of lessons from the "*Novum Organum.*"

16. I am sorely tempted to proceed further with the discussion of this atomic theory, which I regard as not only unsupported by any direct evidence as a matter of fact, but to be altogether unphilosophical as a hypothetical view of the constitution of matter; but as such discussion would lead me too far away from the main object of this paper, I must reserve it for a future essay.

17. Dr. Wollaston next proceeds to follow up the conse-

quences of that assumption of unlimited atmospheric expansibility which I maintain to be no longer a matter of mere hypothesis, but to have been experimentally demonstrated; and according to which every sun, planet, and satellite must accumulate about itself an atmosphere proportionate to its gravitating power. He calculates the quantity of atmosphere which should thus be attracted to the sun and to Jupiter, and endeavours by observations on the movements of Venus and Mercury when near the Sun, and by a "*reductio ad absurdum*" in the case of Jupiter, to refute the hypothesis (it *was then* merely an hypothesis) of unlimited expansibility. I think I shall have no difficulty in proving that Dr. Wollaston's method of calculating the amount of atmosphere which the gravitation of each body should, according to the hypothesis of an universal atmosphere of unlimited expansibility, gather around it, is altogether fallacious, and that all the arguments he has based upon these calculations are worthless.

18. In paragraph (H) he calculates the quantity of solar atmosphere by finding the distance at which the force of solar gravitation is equal to that of the earth's at its surface, and then assumes that solar gravitation should at that distance produce an amount of atmospheric condensation equal to that which the earth obtains upon its surface. The distance at which solar gravitation equals the earth's at its surface is estimated at 5·15 times the sun's radius. For the sake of simplifying the argument I will omit the fraction, and call it five times (which, according to recent rectifications of the sun's magnitude, is nearer the truth than the fractional figure). At this distance the force of the sun's gravitation upon any mass of matter occupying any unit of space will be $\frac{1}{25}$ of that upon his surface; and according to Wollaston it would there, under the conditions assumed, gather an atmosphere weighing 15 lbs. per square inch of its base.

19. Carrying out this method of calculation, what will be the density of the atmosphere at ten times the sun's radius?

Obviously $\frac{1}{4}$ of 15 lbs. per square inch; at fifteen times the radius it will be $\frac{1}{9}$ of 15 lbs.; at 20 times $\frac{1}{16}$ of 15 lbs., and so on. But then, at all these distances, the base of the supposed atmosphere will be a spherical shell whose area or number of square inches will go on increasing in the ratio of the square of its radius or distance from the sun's centre. Let a be the area in square inches of the imaginary sphere which forms the base of the atmosphere at five times the sun's radius. The total weight of the atmosphere calculated according to Dr. Wollaston's method, and stated in lbs., will there be $=15\,a$. At ten times the sun's radius, it will be $=\frac{15}{4}\times 4\,a=15\,a$. At fifteen times radius, it will be $=\frac{15}{9}\times 9\,a=15\,a$. At twenty times radius, it will be$=\frac{15}{16}\times 16\,a=15\,a$ again; and so on it will be the same, at any and every distance that may be taken, whether proceeding further outwards from the sun, or approaching inwards to his surface, where the total weight of atmosphere will be $(15\times 25)\times\frac{a}{25}=15\,a$, as before. We may thus take an infinite number of distances from the sun, upon any and every one of which we have as much right to calculate an atmosphere as Dr. Wollaston had to calculate one at 5·15 times the sun's radius. All these atmospheres will be exactly equal; and thus we are landed upon the enormous absurdity of proving that, under the conditions stated, the sun would gather around himself an infinite number of atmospheres of equal weight, one above the other, each resting and pressing on those below it, without increasing their density or adding anything to the general pressure upon the sun's surface.

20. The same fallacy is repeated and applied to Jupiter. In paragraph (R), it is calculated that, at a distance of 1·6 times his own radius, the gravitation of Jupiter will equal that of the earth at its surface. Dr. Wollaston infers that therefore Jupiter should at this distance have an atmosphere of a density equivalent to a pressure or elasticity of 15 lbs. per square inch. Further on, in paragraphs (R) and (S), he contradicts this prin-

ciple of calculation, by telling us that, "according to the known laws of barometric pressure," this atmosphere would be increased to 3½ times that density, or to 52½ lbs. pressure, by descending only six miles nearer to Jupiter. Had he started his estimate at this lesser distance instead of the six miles higher, and directly calculated the density of the atmosphere at this point, by comparing the gravitation of Jupiter there with that of the earth, he would have arrived at a result ridiculously at variance with this 52½ lbs. Taking the radius of Jupiter in round numbers at 46,000 miles, the distance at which Dr. Wollaston places its gravitation and its atmospheric pressure as equal to the earth's would be 73,600 miles from his centre. Six miles below this is 73,594 miles. Now the distance of 73,600 miles has no special property which should make it the basis of calculation rather than 73,594 miles; we have, therefore, as much right to start from this as from the larger number. Let us do this, and see what will be the result.

21. The gravitation at 73,594 miles will be to that at 73,600 as $73{,}600^2 : 73{,}594^2$. This gives us the fraction ·00016 as the increase of gravitation due to the greater depth. This difference is less than that between the gravitation of different parts of the surface of the earth; far less than the difference between our equatorial and polar gravitation. It increases the pressure of the supposed atmosphere from 15 lbs. to 15 lbs. 17·6 grains. But Dr. Wollaston tells us that, "according to the known laws of barometric pressure," the increase should be from 15 lbs. to 52½ lbs. Thus, according to his fundamental mode of calculation, we get a difference of 17·6 grains, while "the known laws of barometric pressure" give a difference of 37½ lbs., or fifteen thousand times as much. All this discrepancy *in only six miles*, when we are dealing with planetary measurements.

22. The same fallacy is repeated in paragraph (F) in reference to the moon, where a calculation is made on the same basis, but inversely applied.

23. With errors of such enormous magnitude I have not thought it at all necessary to reprint the details of Captain Kater's observations, nor to discuss the question whether any such refraction as Dr. Wollaston looked for is observable when Venus approaches the sun, either within 65′ 50″ or 53′ 15″, or even 46′ from his centre. Seeing also that Dr. Wollaston starts at a height of 27,600 miles from the surface of Jupiter, and there places the *base* of an atmosphere due to the *total gravitation* of the planet, all the results of his calculations respecting the refraction of Jupiter's inner satellite lose their bearing upon the subject in consequence of the enormous exaggeration upon which they are based. I need not follow them any further, as it is obvious that if he had correctly estimated the extent of Jupiter's atmosphere in accordance with the hypothesis he attacks, he would have concluded that, far before reaching such an elevation, the atmosphere of Jupiter would practically become merged into that of the general planetary medium; and that instead of finding there a condensation producing an elastic force of 15 lbs. per square inch, he would have concluded that the condensation must have become far too small to produce even the slightest appreciable amount of refraction upon the satellite. I think I am fully justified in affirming that all the conclusions which have been based upon this celebrated paper fall to the ground, and that our elementary treatises on Pneumatics and Meteorology will require revision as regards this part of their subject.

CHAPTER III.

THE DISTRIBUTION AND COMPOSITION OF THE UNIVERSAL ATMOSPHERIC MEDIUM.

THE PROGRESS OF ACTUAL ATMOSPHERIC RAREFACTION—OUR ATMOSPHERE A PORTION OF THE GENERAL MEDIUM WHICH UNITES ALL THE UNIVERSE—ACTION OF TWO GRAVITATING BODIES ON A GENERAL MEDIUM—THE LAW OF ATMOSPHERIC ACCUMULATION BY THE RESPECTIVE GRAVITATION OF COSMICAL BODIES—RULE FOR CALCULATING RELATIVE ATMOSPHERES OF THE MEMBERS OF THE SOLAR SYSTEM—THE SURFACE OF THE BODY THE STARTING-POINT OF CALCULATION—THE ATMOSPHERE OF THE SUN—IS WATER ATMOSPHERIC OR PLANETARY MATTER ?—THE WATER OF A PLANET MAY COMMONLY EXIST AS SOLID, LIQUID, OR GAS —WATER THE MOST IMPORTANT CONSTITUENT OF THE UNIVERSAL ATMOSPHERE—THE TERRESTRIAL UNIT UPON WHICH THE CALCULATION OF THE SOLAR AND PLANETARY ATMOSPHERES SHOULD BE BASED.

24. Having discussed, and, I think, removed Dr. Wollaston's objections to an infinite atmosphere,* I will now proceed upon the assumption that the confessions of those portions of atmo-

* The reasoning which places the extreme limits to the atmosphere at that distance where the centrifugal force of the earth's rotation would (if communicated to the atmosphere) become equal to gravitation, need not here be discussed, as it obviously applies only to the assumption of an atmosphere bounded by vacuum; for if our atmosphere is but a portion of a general medium, only that film which is immediately clinging to the earth's surface will partake fully of its rotation, which would be communicated to it by the friction of the earth's surface. In the upper regions this would be resisted by the friction (or rather, the visiosity or shearing force) of the general atmosphere, and the velocity of translation would diminish, instead of increasing with increased elevation.

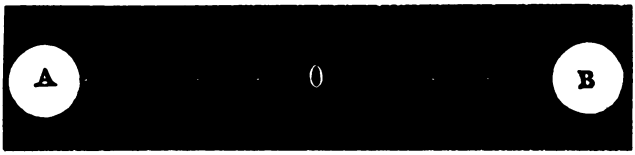
spheric matter which have been subjected to the torture of experimental rarefaction, are truthful statements of the general conduct of the great atmosphere, regarded as a whole. If so, it must go on expanding and expanding continually, as its distance from the earth increases, and the pressure of the portion above consequently diminishes. Where the pressure is but 1 lb. on the square inch, it must be fifteen times more rare than at the surface of the earth; when the pressure is reduced to one ounce, it must be expanded 240 times. Still higher, where the column of an inch base presses with only the force of one grain, the bulk of an equal weight will be 105,200 times greater than at the earth's surface; one grain there will occupy the space of 15 lbs. here, and so on; we may still go upward, and the limit still evades us by continually multiplying expansion, until the gravitation of the earth is neutralized by that of some other orb, toward which our imaginary course is tending. If we cross this neutral line, and continue further in the same direction, we enter the domain of another world, where the order of proceeding density is reversed.

25. I firmly maintain that if we carry out, without the prejudice of any idolatry of hypothetical atoms or hypothetical formulæ, a strict induction based only and directly upon all ascertained facts, we must conclude that our atmosphere is but a portion of an universal medium, which is distributed by gravitation, as above indicated, among the countless orbs of space. The philosophical consequences of such a conclusion are of overwhelming importance, and the object of this Essay is to indicate some of them, and to open thereby what appears to me something like a new world of philosophical enquiry.

26. Having pointed out the fallacy of Wollaston's method of estimating the share of such an universal atmosphere which each gravitating body should take to itself, I will now endeavour to establish a correct method of making such calcula-

tion. A little consideration will show that the problem is really a very simple one.

Let A and B represent two solid gravitating masses, and the intermediate line a portion of the atmospheric medium solicited by their respective gravitation.



It is obvious that somewhere on the line A B there must be a neutral point where the gravitation of A and B will exactly neutralize or counterbalance each other. Let this neutral point be 0. All that portion of the general atmosphere lying to the right of 0 will gravitate towards B, while that to the left of 0 will, in like manner, gravitate towards A. We may, therefore, call the distance A 0 the depth of A's atmosphere, and B 0 the depth of B's atmosphere. It is also obvious that if A and B are of equal size and mass, the point 0 will be midway between them and A 0=B 0.

If A and B are of unequal masses, they will act in a corresponding manner, but unequally, and the lines A 0 and B 0, or the depths of their respective atmospheres, will no longer be equal. These depths will be determined, as before, by the position of the neutral point 0. Now the law of gravitation teaches us that the relative distances of this neutral point from the centres of gravity of two such gravitating bodies will be respectively as the square roots of their masses. Thus, if A have nine times the mass of B, the neutral point 0 will be three times as far from A as from B, or A0=3B0, and so on.

27. With an infinite number of orbs floating amidst the infinity of space, filled with such an elastic medium, every orb will be surrounded on every side by others, and its power of

accumulation from the general atmosphere will be controlled and limited by the general resultant of their counteracting gravitating forces. Each will appropriate the contents of a sphere, the radius of which will have the same ratio to that of any other as the square roots of their respective masses. As the cubic contents of a sphere varies with the cube of its radius, the cube of the square root of the mass of each orb will express its relative atmosphere. As the cube of the square root of any number is equal to itself by its square root, we obtain a very simple measure of relative atmosphere by multiplying the number representing the mass of each body by its own square root.

28. As we know the relative masses of our own earth and those of the other members of the solar system, and we also know the absolute amount of atmosphere which our earth has appropriated from the general medium, we have the data for calculating, according to the above, the absolute atmospheres of the sun, the moon, and our companion planets and their satellites. Taking the mass and the atmosphere of the earth as our units, we get the following simple rule for calculating the atmosphere of any of the bodies of the solar system. *Multiply the mass of the body expressed in units of the earth's mass by its own square root, and the product is the total weight of the atmosphere of the body, expressed in units of the earth's atmosphere*, or $x=m\sqrt{m}$, where x is the atmosphere of the body expressed in units of the earth's atmosphere, and m is the mass of the body expressed in units of the earth's mass.

29. The figures thus obtainable express the theoretical total pressure of the atmosphere upon the surface of the body in question. In every case we must thus start from the surface, and not, like Dr. Wollaston, take any arbitrary distance beyond it, as we should thereby over-estimate the total atmosphere by just the quantity that would lie between our arbitrary starting-point and the surface of the body. I will now proceed to apply this method to the estimation of the solar atmosphere.

30. According to recent corrections of the sun's distance and magnitude, his mass is to the earth's as 316,047 to 1. The square root of 316,047 is (omitting minute fractions) 562·2. The weight of the sun's atmosphere should therefore be $316{,}047 \times 562 \cdot 2 = 117{,}681{,}623$ times that of the earth's atmosphere. If this were concentrated upon a body of the same dimensions as the earth, the pressure upon every square inch of its surface would be 117,681,623 atmospheres, or $117{,}681{,}623 \times 15 = 1765{,}224{,}345$ lbs. per square inch, or 244,192,305,680 lbs. per square foot. But the diameter of the sun is 108 times greater than that of the earth, and therefore his surface is $108^2 = 11{,}664$ times greater, and his atmosphere must rest upon a base of proportionally greater area. Its pressure upon any given area will thus be $\frac{117{,}681{,}623}{11{,}664} = 15{,}233 \cdot 3$ atmospheres. This gives a pressure of 32,903,928 lbs. per square foot, or about 230,000 tons on the surface of the human body.

31. In the above estimate I have only considered the gaseous constituents of the earth's atmosphere; but a very important question here presents itself. Are we to regard the waters that cover the lower vallies of the earth as planetary or atmospheric matter? Is the water of the ocean one of the special constituents of the earth, or only a portion of the general atmospheric matter which the earth's gravitation has condensed around it?

32. This question I think may be solved by considering the known properties of water. Such consideration at once shows us that the position occupied by the water on our own, or any other planet, is entirely dependent on comparatively moderate variations of temperature and pressure. If the temperature of the earth were raised, or the pressure diminished in a sufficient degree, the whole of the water of the ocean would rise from its present bed, and take its place in the atmosphere as one of its constituent gases, and would there exist in a state exactly corresponding to the carbonic acid of our actual atmosphere. We only require the conditions which actually exist on the planet

Mercury to bring this about. On that planet, if my views are correct, water can only exist as a gas, and a regular constituent of the atmosphere. If Mercury has had its Faraday, he may have succeeded in demonstrating the condensibility of this gas; he may, by refrigeration and pressure, have produced liquid water and water-snow, and by the aid of the mechanical devices of a Mercurian Thilorier, have obtained sufficient quantities to exhibit them as lecture-table curiosities. The Mercurians would certainly regard water as purely atmospheric matter.

33. I think, therefore, that we may safely include water among the original constituents of the general atmospheric medium. We might go much further in the same direction, and finally reach a full development of the nebular hypothesis, but, I propose in this enquiry, to carry my speculations but one step beyond demonstrable terrestrial phenomena; to keep one foot always on the *terra firma* of direct knowledge, to stretch out the other nowise beyond the limits of one fair stride of direct and necessary causation, and to plant it upon the demonstrable results of ascertained facts. Without infringing this limit, I think I may conclude that the water upon our earth is but a portion of the matter which its gravitation has collected from the all pervading medium of the universe. Whether it may have received more or less than the share equivalent to its gravitation is a most desirable question for a physical debating society. I should be rather disposed to take the affirmative side of excessive accumulation in the case of our particular planet, but must not now be tempted into a discussion of this question. At present, I merely wish to show that there is good reason to believe that gaseous water is one of the most important constituents of the general atmospheric medium. If it exists in the same proportion that the water of our globe bears to the permanently gaseous constituents of our atmosphere, it must constitute above 99 per cent. of the whole.

The revelations of the spectroscope afford the strongest possible confirmatory evidence of an universal distribution of water. Whether directed to the envelopes of the sun, to the stars, the nebulæ, the luminous matter of comets or meteors, its general reply is, "Water, water, everywhere," and Professor Graham has even found occluded hydrogen in meteoric stones that have reached the earth. I shall, therefore, assume that water belongs to the atmosphere, and, in the present order of things, should be found as a constituent of the atmosphere of the sun, and all the other orbs of space; the state of its existence, whether solid, fluid, or gaseous, whether combined as water, or separated into its constituents of free hydrogen and free oxygen, being dependent on the physical conditions to which it is subjected.

34. A very important question now presents itself: it is this. If the water of our ocean is derived from the general atmospheric medium, and is atmospheric matter, are we to add its weight to that of the gaseous atmosphere in order to obtain our terrestrial unit for measuring the atmosphere of the other bodies of the solar system? If so, it will magnify our unit enormously. In order to reply to this question, we must remember the basis upon which the assumption of a general atmosphere rests, and what is the link which binds together, as a whole, all those portions of it which the aggregated masses of the universe have gathered around themselves. It is the property of *gaseous elasticity*. If by any means whatever any portion of this atmospheric matter loses this property, it becomes at once detached from its original sphere, and goes over to the enemy; it joins the aggregating forces of gravitation in their struggle against the diffusive force of gaseous elasticity. The water when condensed upon the surface of the earth simply increases its mass, and, to that extent, increases its power of accumulating the atmospheric medium. My reasons for supposing that our earth may have more than its proportionate share of water will now be understood. It will also be seen that different regions of space may possibly vary in

this respect according to the prevalence of condensing or non-condensing orbs among their solid inhabitants. But I must stubbornly resist the temptation to proceed further with the speculations here suggested, and keep to the immediate object I had in raising this question, viz., the justification of the unit which I adopted in calculating the solar atmosphere. This unit is the equivalent of gaseous repulsive energy which the earth's gravitation is capable of overcoming.

CHAPTER IV.

THE SOURCE OF SOLAR HEAT AND LIGHT.

THE ESTIMATED ATMOSPHERIC PRESSURE ON THE SUN IS BELOW THE ACTUAL PRESSURE—EVOLUTION OF HEAT FROM COMPRESSION OF THE SOLAR ATMOSPHERE—THE SOLAR ATMOSPHERE IS NOT LIQUEFIED BY ITS OWN PRESSURE—LESLIE'S FORMULA FOR HEAT EVOLVED BY COMPRESSION—HEAT EVOLVED BY COMPRESSION OF THE SOLAR ATMOSPHERE—CALCULATION BASED ON DALTON'S EXPERIMENTS—ESTIMATE OF SOLAR HEAT FROM COMPRESSION—WATER THE PREPONDERATING CONSTITUENT OF THE SOLAR ATMOSPHERE—DISSOCIATION OF WATER AND CARBONIC ACID—LATENT OR CONVERTED HEAT OF DISSOCIATION—HEAT EVOLVED BY COMPRESSION CONVERTED INTO DISSOCIATING FORCE—LIMIT OF DISSOCIATION AND ORIGIN OF THE PHOTOSPHERE—NOTE ON LATENT HEAT.

35. In this chapter I propose to trace some of the necessary consequences of the pressure of such an atmosphere as I have estimated to be accumulated upon the surface of the sun. This estimated pressure of 15,233·3 atmospheres must be considerably below the truth, inasmuch as all our measurements of the sun's diameter are made from the limits of his luminous atmosphere, which constitutes the only outline visible to us. There can be little doubt that this atmospheric ocean is many thousands of miles in depth, and that the photosphere which determines our measurements belongs quite to upper regions of the solar atmosphere, where the density is probably less than that of our own atmosphere at the earth's surface. Hence the base of the calculated atmosphere must be smaller than I have stated, and the pressure upon any given area of that base proportionally greater. I do not, however, attempt any cor-

rection for this difference at present, as any estimate of the diameter of the body of the sun must, for reasons that will be stated hereafter, be of a most hypothetical and doubtful character. The estimate already given is sufficiently near for my present purpose of general indication, but it must be borne in mind throughout, that this amount of pressure, and all the consequences I shall deduce from it, will err on the side of moderation or understatement. As the magnitude of some of these results is rather startling, it is perhaps better in this preliminary sketch of the subject to keep thus on the safe side.

36. The most important result of such an accumulation of solar atmosphere as I have described, is the evolution of heat which must be produced by the compression of its lower strata. If the pressure that can be exerted by the human hand upon the piston of a condensing syringe is capable, in spite of the surrounding conductors, of producing sufficient heat to ignite a piece of German tinder, what must be the result of the enormous pressure of this vast atmosphere? Have we in this a key to the great mystery of the source of solar heat and solar light? I will endeavour to answer this question.

37. The first problem that presents itself is, of course, the determination of the amount of heat that will be evolved by the condensation resulting from a pressure of 15,233 atmospheres, assuming that the nitrogen and oxygen of the air will retain their gaseous condition. That such will be the case I infer, First, from the fact that no indication of departure from the law that elasticity varies with density has yet been observed, when atmospheric air has been subjected to any amount of compression; whereas in all the gases known to be capable of liquefaction by pressure, a deviation from this law is indicated when any degree of compression is applied, as shown by the experiments of M. Despretz and Pouillet; and this deviation becomes very considerable as liquefaction is approached. We are thus supplied with a clear brand of distinction between the condensible and incondensible gases. Secondly, That even if

liquefaction were *possible*, the conditions of such liquefaction would not be fulfilled in the case supposed. In order that compression shall produce the liquefaction of any gas, it is necessary that the heat evolved shall be removed, as it is in the ordinary experiments by which we demonstrate the liquefaction of certain imperfect gases. I think I may safely assert that if the *whole* of the heat of condensation were retained by the gas itself, no amount of compression would produce the liquefaction of any gas. Only a very small fraction of the whole heat can be radiated away from the vast solar atmosphere, and this, as I shall show hereafter, is continually restored.

38. By a series of experiments upon the rise of temperature produced by exhausting a large receiver in which a thermometer was suspended, and observing the loss of heat from rarefaction, and the heat gained when the air was re-admitted, Leslie found (after due allowance for absorption of receivers, etc.) that the increase of the temperature of air due to its own condensation may be expressed in degrees of centigrade thermometer by the following simple formula, where B represents the greater pressure, and b the lesser :—$X=(\frac{B}{b}-\frac{b}{B})25$.

39. Taking the atmospheric pressure on the earth as unity for b, and the pressure on the lower portions of the solar atmosphere for B, we get $(\frac{15,233\cdot3}{1}-\frac{1}{15,233\cdot3})\ 25=380,832°$ Cent., or 685,529° Fahr., as the temperature of the lower regions of the solar atmosphere. This is so enormously beyond any temperature of which we have any terrestrial experience, that it is quite impossible to form any rational conception of such intensity of heat.

According to Dalton's experiments* "in letting in air to an exhausted receiver, something more than 50° of heat is produced," and in restoring the equilibrium of air which has been condensed to two atmospheres, "about 50° of cold is pro-

* *Memoirs of the Literary and Philosophical Society of Manchester*, vol. v., part ii., p. 515.

duced," *i. e.*, each atmosphere of additional pressure produces a rise of temperature of the compressed air=50° Fahr. Assuming that this continues true of higher pressures than one and two atmospheres, the heat evolved by a pressure of 15,233·3 atmospheres will be 15,233·3 × 50 = 761,665° Fahr., which is 76,136°, or about one-ninth more than the result obtained according to Leslie's formula.

Dalton's figures are very nearly in accordance with the more recent results obtained by Mr. Joule in determining the mechanical equivalent of heat. By calculating the amount of work done in compressing 15,233·3 atmospheres, I obtain much higher figures than either of the above, but the specific heat of such compressed air and vapour at a constant pressure being still a debateable question, I am unable to satisfactorily calculate how much of the heat thus obtained will remain as free temperature, after it has done the work of heating and expanding the compressed atmosphere itself.

41. As these estimates are based upon experimental results obtained with such relatively low pressures, they must only be regarded as crude approximations. This is all that is required for my argument, which will be no more affected by the corrections that will be made hereafter than are the general laws of physical astronomy by the recent correction of the estimated distance between the sun and the earth. On this account I have neglected the correction due to the difference between the real and apparent diameter of the sun, before referred to, and have not included any additional pressure from atmospheric water. These indeterminable quantities would make a considerable addition to the calculated solar temperature.

I have already (sections 32 and 33) stated reasons for regarding water as atmospheric matter, and concluding that the general medium of space contains a large proportion of either aqueous vapour, or the uncombined elements of water, or more probably of both. That the atmosphere of the sun contains the elements of water in profuse abundance, is shown

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by spectroscopic researches, especially by the recent observations on the composition of those enormous mountains of gaseous matter, the solar prominences. These being ejections from the body of the solar atmosphere, and being viewed in *profile, the prisms receive little else than the radiations from atmospheric matter,* and thus reveal only the composition of the solar atmosphere, whereas the observations made perpendicularly upon the photosphere take in all those radiations that may pass through the solar atmosphere from its lower depths, where a large proportion of the materials of the body of the sun must exist in a fused and volatilized condition. The reply of the spectroscope is, that these samples of the solar atmosphere which are thus submitted to it for analysis are mainly composed of hydrogen and aqueous vapour. The same reply is radiated from the steady and the flashing stars, from the mysterious nebulæ, and even, in a modified form, from the light of comets. If the temperature of our atmosphere were raised above the boiling point, its composition in relation to aqueous vapour would be exactly the reverse of what actually exists. Instead of a mixture of nitrogen and hydrogen, with a little aqueous vapour diffused through it, there would be an atmosphere consisting mainly of water in the gaseous state, with an insignificant proportion of the free nitrogen and oxygen diffused through it. Without assuming that the sun's atmosphere must necessarily contain so large a proportion as this, there is, I think, sufficient reason to infer that the vapour or elements of water constitute its preponderating ingredient.

43. Let us now consider what will be the effect of the heat evolved by the compression of the solar atmosphere upon this important constituent, the vapour of water. It has been proved that when aqueous vapour, under atmospheric pressure, is raised to a temperature of 2,800° Centigrade, or 5,072° Fahr.,*

* Cailletet, from furnace experiments, fixes the temperature at which oxygen

"dissociation" takes place,—it is separated into its elements, which are incapable of reuniting so long as this or any higher temperature is maintained. At 1,200° Centigrade carbonic acid is, in like manner, separated into carbonic-oxide and oxygen, which, like the elements of water, form a furiously explosive mixture at ordinary temperatures, but which, at the temperature above named, are mutually inert.

44. This work of dissociation by heat is strictly analogous to that done in evaporation. In conferring upon a liquid the new property of gaseous elasticity, a definite quantity of calorific energy is manifested in this newly acquired expansive power, and therefore is not also manifested as temperature. In like manner a still larger amount of *temperature* is withdrawn when heat is converted into the force necessary for separating the vapour into its component gases. The heat thus converted or rendered "latent" amounts, according to M. St. Claire Deville, to about 8,000 Fahr.* This converted heat of dissociation is restored as temperature when re-combination occurs, just as the latent or converted heat of steam is restored when the vapour is condensed into a liquid. The total quantity of heat required for dissociation is therefore 8,000° Fahr., in addition to that which is necessary for raising the vapour to the temperature of 5,072°.

45. It is evident then, that the first result of the great evolution of heat from mechanical condensation of the mixed

refuses to combine with hydrogen and carbonic oxide at a little above the fusing point of platinum, which is between 1,800° and 2,000° Cent. M. St. Claire Deville, whose elaborate and laborious researches on this subject have made it almost his own, determined the temperature of dissociation of aqueous vapour to be 2,500° Centigrade (*Leçons de la Societe Chemique de Paris*), in a flame produced by the mixed gases, where the tension is estimated at 425 millimetres. Bunsen's experiments with a valved eudiometer raises the temperature to 2,800° Centigrade, which temperature has since been adopted by M. Deville (*Comptes Rendus*, Nov. 30, 1868.)

* 2,153 "*calories*" $= \frac{2153}{0{\cdot}475} \times \frac{9}{5} = 8157{\cdot}87^\circ$.

atmosphere of aqueous vapor, carbonic acid, and free oxygen and nitrogen, will be the dissociation of the water and the carbonic acid, which act of dissociation must produce a great reduction of temperature. In the lower regions of the solar atmosphere, where the evolution of heat from condensation attains its maximum, we may assume that this free heat will be more than sufficient to dissociate the whole of the aqueous vapor and carbonic acid, and thus the dissociated gases will still be left at a higher temperature than was necessary to affect their dissociation. (It must, however, be remembered that this temperature of dissociation increases with the pressure, and therefore will be vastly higher in the lower regions of the solar atmosphere than that named by Deville and Bunsen.) The quantity of surplus heat remaining after dissociation is effected, will depend upon the relative proportions of the combined and uncombined gases originally existing in the universal atmosphere.*

46. This surplus heat remaining after the dissociation of the lower strata of the solar atmosphere is completed, will of course be carried upwards, and as it rises and reaches the elevation where the heat of compression was insufficient of itself to effect dissociation, it will supplement the temperature and raise the level of the upper limit of complete dissociation. But there must somewhere be a height at which the temperature capable of effecting dissociation terminates; where the atmosphere of elementary gases fringes upon that of combined aqueous vapour, and where these separated gases must rush into reunion with a furious chemical energy which will be

* As the temperature of dissociation increases with the pressure, a very important and interesting question here presents itself, viz., Does the temperature of aqueous vapour, when submitted to increasing pressures, rise any more rapidly than its temperature of dissociation? If it does not, then all the 8,000° of converted heat must be supplied by the free oxygen and free nitrogen. In any case, the whole of their heat is spare heat that can be stored by conversion into dissociation fuel, and my original calculation is based only upon an unit from which atmospheric water is almost completely excluded.

manifested by violent combustion. Thus we shall have a sphere of dissociated gases and a sphere of compound vapours separated by an interlying stratum of combining gases; a spherical shell of flame, constituting exactly what solar observers have described as the "photosphere," which forms the visible boundary of the solar surface. I shall be able to show hereafter that not only the blazing photosphere that dazzles our unprotected eyes, but all the details revealed by the face of the sun to modern telescopic observation, follow as necessary consequences of the principles already explained, and the other physical conditions of the sun which I shall describe in the following chapters; the spots, the "*faculæ,*" the "*umbra*" and "*penumbra,*" the "*mottling,*" the "*granulations,*" the "*clouds,*" the "*pores*" and "*punctulations,*" the "*platforms,*" the "*promontories,*" the "*willow leaves,*" "*rice grains,*" "*thatch*" and "*things,*" the "*bridges*" crossing the spot chasms thousands of miles wide with a single span, will all, with due allowance for the specialities of eyes, telescopes, and weather, prove to be just the appearances which are theoretically demanded, and which admit of very simple and natural explanations.

Before entering into these details I must devote a few chapters to the great physical laws and conditions upon which these variegations of the photosphere mainly depend. We shall then understand that these telescopic curiosities are no cosmical irregularities or "freaks of nature," but that if they were not there, we should not be here; that, like every other fact which human investigation can discover and assign to its proper place in the order of creation, they are necessary links in the great chain of causation which upholds the existing universe.

NOTE ON "LATENT" HEAT.

47. At the conclusion of the second chapter of M. Deville's *Leçons sur la Dissociation,* he apologises for continuing the use of the term "latent heat," and justifies it on the ground of its great convenience. He quotes the analogous practice of astronomers, who for simplicity sake "substitute the apparent movements of the stars for their real movements," and further advocates the continued use of the term in order to avoid neology. I here find myself in a similar dilemma in reference to this expression, which is at the same time so temptingly convenient, and so atrociously unphilosophical. The entire disappearance of a certain quantity of heat, and its reappearance in precisely the same quantity and in the same place, is so definite and sensible, that the game of "hide and seek" is charmingly expressive of the phenomena, and was perfectly tolerable while heat was regarded as an extravagantly subtle sort of substance quite capable of dodging the philosopher by squeezing itself between those ultimate atoms of matter which his imagination had created and his mathematics had nourished.

But now, having abandoned the idea of "imponderable matter," and regarding heat as a "force," this term latent heat is altogether an absurdity. To speak of a *latent force* is about as reasonable as to describe an achromatic colour, or an unfelt sensation.

It clashes so harshly with the grandest and most fundamental development of modern philosophy, viz., the doctrine of the indestructibility of force, that I think no dread of neology should prevent us from substituting a better term for it. I have therefore been sufficiently presumptuous to perpetrate such a neology. I use the term "*converted*" heat in the place of *latent* heat, and when speaking of the *re*combustion or *re*condensation of the same substance I use the term "*restored heat*" in the same manner as we speak of the blue colour of litmus being *converted* to red by an acid, and *restored* by an alkali. Otherwise, when the conversion of the heat of the particular substance has not been under immediate consideration, the terms *evolution* or *giving out* of heat are used.

CHAPTER V.

THE MAINTENANCE OF THE SOLAR LIGHT AND HEAT.

THE CONSTANCY OF SOLAR HEAT AND LIGHT IN PAST TIME—FUTURE PERMANENCY OF SOLAR HEAT AND LIGHT—THE THEORETICAL CONSTITUTION OF THE SUN—HEAT EVOLVED BY COMBUSTION EQUIVALENT TO THAT CONVERTED BY DISSOCIATION—THE COMBUSTION OF DISSOCIATED GASES IS LIMITED BY THE POSSIBILITIES OF RADIATION AND THE TRANSMISSION OF HEAT—A HYPOTHETICAL SUN—ORIGIN OF THE STOCK OF SOLAR FUEL—THE LIMIT OF SOLAR COMBUSTION—GRADUAL EXTINCTION OF THE SUN IF NOT SUPPLIED WITH FRESH FUEL—FRESH FUEL IS SUPPLIED TO ALL THE CENTRAL SUNS—EFFECT OF THE SUN'S PASSAGE THROUGH THE INTERSTELLAR ATMOSPHERIC MEDIUM—THE DAILY SUPPLY OF FRESH SOLAR FUEL—ATMOSPHERIC BOMBARDMENT OF THE SUN—SOLAR CONCENTRATION OF THE HEAT OF SPACE—ALTERNATE COMPRESSION AND RAREFACTION OF ATMOSPHERIC MATTER—IS THE FRESH ATMOSPHERIC FUEL STIRRED INTO THE SOLAR FURNACE?

48. ALL the records of human experience and observation indicate that during the historical period no sensible increase or diminution of the solar light and heat has occurred; and the incomparably older records of geological history point to the same conclusion. No theory of the sources of solar heat and light can be sound which fails to explain this degree of permanency.

49. In addition to the demands of historical fact there is another, which, though less imperative, is not to be altogether overlooked. I refer to the almost irresistible *à priori* belief in the permanency of the universe, which is usually the strongest in those who have the most deeply studied its mechanism. It is true that this is not a purely intellectual conclusion capable of logical demonstration, but is only an inference supplied by

the intellect, on the basis of probability, to satisfy the cravings of our moral sentiments,—it is a poetical, rather than a scientific conclusion. But the moral sentiments and poetical faculties are as truly and properly a part of man's nature as his sense of touch, and, when properly regulated, their conclusions are probably as near approximations to absolute truth as the impressions derived from his senses. Thus to be *satisfactory* as well as sound, our theory must not only explain the past uniformity of the solar radiations, but must also define the provision for their future permanency. I will now proceed to show how both of these conditions are fulfilled by the theory I am expounding.

50. First. As regards *uniformity of radiation*. The sun, according to my hypothesis, is a hollow flame with dissociated combustible gases within it, and a nucleus of some kind within them. It differs from the hollow flame of a candle in the very important respect of not being dependent upon external oxygen for its support; it contains all the materials of combustion within itself, but this combustion—or combination—is restrained from proceeding explosively inwards by the dissociating force of the high temperature of the inner gases.

51. When two substances, such as oxygen and hydrogen, combine chemically, they evolve an amount of heat exactly equivalent to that which is required to drive asunder the constituents of an equal quantity of the compound which they form; and therefore the amount of combination or combustion that can take place in a given mixture of such elementary gases is limited by the quantity of heat which surrounding bodies are capable of abstracting. To illustrate this, let us conceive the case of a certain quantity of the elements of water heated exactly to the temperature of dissociation, and confined in a vessel the sides of which are maintained externally at precisely the same temperature as the gases within, so that no heat can be added or taken away from the gases. No sensible amount of combination could now take place, as the first infinitesimal

effort of combustion would set free just the amount of heat required to decompose its own result. In like manner, if a quantity of aqueous vapour were subjected to the same conditions, there would be a corresponding equilibrium; and no sensible amount of dissociation could take place, as the first effort of dissociation would be attended with a conversion of heat, and consequent loss of temperature, which would be exactly compensated by the instantaneous reunion or combustion of the infinitesimally small quantity of dissociated water.

52. Let us now suppose a modification of these conditions, viz., that the vessel containing the dissociated gases at the temperature of dissociation shall be surrounded with bodies cooler than itself, *i. e.*, capable of receiving more heat from it than they radiate towards it; there would then take place just so much combustion as would set free the amount of heat required to maintain the temperature of the vessel at the dissociation point; or, in other words, combustion would go on to the extent of setting free just as much heat as the gaseous mass was capable of radiating, or otherwise transmitting to surrounding bodies; and this amount of combustion would regularly and steadily continue until all the gases had combined.

53. We have only to give this hypothetical vessel a spherical form and an internal diameter of 853,380 miles,—to construct its enveloping sides of a thick shell of aqueous vapour, and then by placing in the midst of the contained gases a central nucleus of solid or liquid matter, we are hypothetically supplied with the main conditions which I suppose to exist in the sun.

54. A little reflection upon the application of these laws to the above conditions will show that the primal clash of solar atmospheric condensation (I imagine such a beginning for illustration sake) would not produce an epoch of excessive heat subject to a steady decline consequent upon secular radiation; but that the first effect of the heat developed by compression would be a dissociation of aqueous vapour exactly commensurate with the heat thus evolved, and this would limit the re-

sulting temperature to the dissociation point. The stupendous ocean of explosive gases thus formed would constitute an enormous stock of fuel capable, by its combustion, of setting free exactly the same quantity of heat as had previously been converted into decomposing or separating force, and which, could it all combine at once, would produce an explosion that would shatter the whole of our solar system.

55. Instead of this, the amount of combustion must, from the first, have been limited by the damper of dissociation, and confined to the work of supplying the quantity of heat which the flaming surface could radiate to surrounding matter; and this quantity of radiation must be limited by the outer envelope of aqueous vapour which, as already shown, must exist above the atmospheric level where the temperature of dissociation is reached. This vapour, with the addition of that produced by combustion, would constitute, by its well known resistance to the passage of such heat rays as would be produced by such combustion, an effectual jacket for limiting the amount of radiation.

56. If only these conditions existed, there would be a steady decrease in the amount of solar heat; as the vaporous envelope would go on increasing, and just in proportion as it resisted the radiation, would it diminish the amount of combustion. In this manner the fuel of the sun would be economised, and the date of its final extinction almost indefinitely postponed; but in the meantime all the creatures of the attendant worlds, dependent for existence upon the solar radiations, would gradually languish, and either universal death or a new order of life must be the final result of this slow decline of the life-giving radiations from our central luminary.

57. A further examination of the machinery of the universe shows that no such extinction of the sun—even at the remotest conceivable period—no gradual diminution of his energies, need be feared; but that a certain normal amount of radiation when once attained will, as far as we can learn from the existing order

of nature, be maintained eternally by our sun and all the other suns that are surrounded by dependent planets. This fresh supply of fuel is supplied by the greater orbits of the greater orbs, and the reacting gravitation of their attendant worlds.

58. Our sun is travelling through space with a velocity that has been computed at about four to five hundred thousand miles per day. Now, if the hypothesis of an universal atmosphere is correct, the sun must, of necessity, encounter some resistance in his passage through it. On what part of the sun will this resistance be applied? Obviously on the outer regions of his atmosphere, which, being fluid, will yield to such resistance, and a portion will be left behind.* But his gravitation still remains the same, and he must, therefore, still obtain his share of the general medium. This will, of course, be derived from that portion of space into which he is progressing, while the friction trail will be left in the rear. The retardation due to the drag of the rear must be compensated by the gravitation towards the sun of the atmospheric matter of the region into which he is advancing, for this newly arriving atmosphere will be falling towards the sun, and to the extent of its gravitation must pull the sun towards it.

59. Let us now see what will be the amount of fresh fuel thus supplied to the sun. The daily supply will be equal in bulk to the contents of a cylinder having a diameter equal to that of the sun and his attendant atmosphere, and whose height or depth is four or five hundred thousand miles. Taking this diameter at 900,000 miles, and the length of the cylinder at 450,000 miles, its cubic contents will be 286,278,300,000,000,000 cubic miles. I will assume that the interstellar atmosphere has a density of only one hundred thousandth part of that of our atmosphere at sea level. A cubic mile of such rarified air would weigh rather more than

* The atmospheric envelope of the sun will thus act as a lubricant diminishing the friction of his passage, just as the oil applied to a bearing does the same by the free sliding of its fluidity.

fifty tons. The total weight of the *daily* cylinder of fresh fuel will thus be 14,313,915,000,000,000,000 tons, or, in round numbers, 165 millions of millions of tons per second.

60. Attempts have been made to account for the heat of the sun by an imaginary bombardment of showers of supposed aerolites, a theory which continually alters the velocity of planetary motion by the constant increase of the sun's mass, and consequent gravitation; and which is contradicted by all the records of solar and planetary constancy. My hypothesis supplies a perpetual bombardment of 165 millions of millions of tons of matter per second, without, in any degree, altering the density, the bulk, or any other element of the solar constitution.

61. In addition to this there is the heat that must be evolved when, by the compression resulting from its approach to the sun, this atmosphere is made to restore and concentrate the heat which, during ages of rest, it has absorbed from the radiations of all the suns and planets of the universe.

62. If we force into a strong vessel several atmospheres of air drawn from without, the temperature of the compressed air and of the sides of the vessel will be raised in proportion to the amount of condensation. If the closed vessel be now allowed to radiate until it attains the temperature of the surrounding air, we shall find, upon allowing the condensed air to escape and expand itself to its original bulk, that its temperature will fall by the expansion exactly in the same degree that it was raised by the previous condensation. This kind of action must occur as the sun rushes forward into the thin gaseous matter of the interstellar atmosphere. There must be condensation in the front and rarefaction in the rear of his course. The fresh air into which he is advancing must be compressed by his approach and by its own gravitation towards him, and that which is swept round his surface to fill the void behind must expand after it has left him.

63. If this were the only action of the kind, its heating

effect would be comparatively small, and limited to the outer surface of the solar atmosphere; as the greater portion of the atmospheric matter passing away behind would merely be the same as struck the front, and at once passed round superficially to the rear. But if by any means a portion of this mighty and perpetual flood of newly arriving, highly rarified, and calorific atmosphere could be plunged, or pumped, or whirled into the lower strata of the solar atmosphere, it must, of necessity, eject an equal quantity of the old effete atmospheric matter which had already given up more or less of its heat by compression and radiation; thus the newly arriving air would, by the compression of the lower depths, be forced to yield up some of the heat it had accumulated by ages of absorption, and the old exhausted material it had pushed out would, in consequence of its upward projection, meet the superficial rearward current, and be carried behind to form a cool trail by its subsequent expansion. The question which next arises is, Does there exist in the actual arrangements of the solar system any machinery for thus stirring in an important quantity of the new atmospheric matter, and ejecting the old? If so, the maintenance of the sun's heat may be fully accounted for. I will endeavour to answer this question in the next chapter.

CHAPTER VI.

THE FEEDING OF THE SOLAR FURNACE.

THE SOLAR FURNACE MUST BE CONTINUALLY STIRRED AND FED WITH FRESH FUEL—STATE OF THE ATMOSPHERE OF THE SUN IF HIS ROTATION WERE UNDISTURBED—PLANETARY DISTURBANCE OF SOLAR ROTATION—VARYING ORBITAL VELOCITIES OF THE SOLAR SURFACE—IRREGULARITY OF PLANETARY DISTURBANCES—INTERCHANGE OF OLD AND NEW ATMOSPHERIC MATTER IN THE SUN—OBSERVED IRREGULARITIES OF SOLAR ATMOSPHERIC ROTATION—VORTICOSE MOTION OF THE SUN-SPOTS—OBJECTION TO MY EXPLANATION OF THE CAUSE OF THE SOLAR ATMOSPHERIC DISTURBANCES—GENERAL REPLY TO THIS OBJECTION—ATMOSPHERIC TIDES PRODUCED BY THE MOON—PLANETARY DISTURBANCES OF THE EARTH'S ATMOSPHERE COMPARED WITH THOSE OF THE SOLAR ATMOSPHERE—THE FRICTION OF OUR ATMOSPHERE UPON THE EARTH'S SURFACE—THE INFLUENCE OF THE MOON UPON THE MOVEMENTS OF OUR ATMOSPHERE—OBSERVED CONNECTION BETWEEN THE SUN-SPOTS AND THE POSITIONS OF JUPITER AND SATURN — COMPARISONS OF CARRINGTON'S SPOT-MAPS AND THE KEW PICTURES WITH THE POSITIONS OF THE PLANETS—INFLUENCE OF VENUS ON THE SPOTS—COMBINED ACTION OF JUPITER AND VENUS—OBSERVATIONS ON TOTAL PLANETARY GRAVITATION ARE REQUIRED—THE MEANS REQUIRED FOR MAKING THESE OBSERVATIONS — THE OBSERVATIONS ON PLANETARY INFLUENCE ALL CONFIRM MY HYPOTHESES—MR. GROVE'S QUESTIONS—REPLY TO MR. GROVE'S QUESTIONS.

64. IN the last chapter I have endeavoured to show how the sun in its journey through space is presented with enormous supplies of fresh fuel, and have indicated that some machinery is necessary to fully utilize this; for should it merely strike the surface of the solar envelope, slide over it and pass away, it would only be slightly compressed, and would fully perform only that portion of its possible work which is due to impact and

friction, while its most important capacity of yielding up its accumulated absorptions would be of small avail. To produce its full effect, this fresh fuel must find its way into the lower depths of the solar atmosphere, and there be subject to all the heat and light evolving agencies already described. The furnace of the sun, like our humble furnaces on earth, requires not only a continuous supply of fuel, but a stoker to feed and stir it. I think I shall be able to show that the planetary attendants of the sun perform this duty with untiring vigilance and efficiency.

65. If the sun simply rotated around its own geometrical axis, its atmosphere would be carried with it; and if not disturbed by other causes, might calmly continue this rotation, with a gradual and regular retardation outward, until it merged insensibly into the general medium of space, and, in the same degree, lost, by the continuously increasing counter friction, the original impulse imparted by the friction of the rotating nucleus. In such a case the interchange between the upper and lower regions of the solar atmosphere would be limited to that resulting from the different degrees of retardation produced by the general medium acting upon the varying velocities of the solar atmosphere, due to differences of latitude.

66. This, however, is far from being the actual condition of the sun. His movements are extremely complex and irregular. He probably has such a regular axial rotation as above described, but to this is superadded another motion resulting from the reaction of the planets in their orbits. If all the planets of the solar system were so situated that a line drawn from the centre of the sun to the centre of Neptune should pass through the centres of all the interjacent planets, and if they all moved in the same plane, and in orbits of equal period, so that these relative positions should be constantly maintained, the sun's necessary motion could be easily described. It would have, in addition to its own axial rotation, a small orbital revolution about the common and constant centre of gravity of the

whole system. This common centre would fall somewhere within the sun's own dimensions, and thus his orbital revolution would be equivalent to a rotation upon an eccentric axis. This motion, combined with the strictly axial rotation, would be similar to that of a spinning-top previous to its falling, when it has a reeling revolution in addition to its rotation upon its peg.

67. In order to form some idea of the extent of irregularity due to such disturbance, let us further suppose that the combined gravitation of the planets thus arranged should be just sufficient to throw the centre of gravity of the whole solar system exactly on the surface of the photosphere. The sun would thus have an orbital motion around this common centre of gravity, and the radius of the solar orbit would be equal to the solar diameter. Thus during each revolution the surface of one side of the sun would move through a space of more than five millions of miles, while the surface of the opposite side would at the same time be merely turning upon itself, and thus have no such motion of translation, while all the intermediate portions of the surface would have intermediate velocities. The rotation of the sun upon its axis would be continually interchanging the surfaces subject to these variable velocities. Under these circumstances the atmosphere of the sun (or the whole of the sun if fluid) would assume the form of a prolate spheroid, whose longer axis would be a line which, if continued, would pass through the centres of gravity of all the planets, and it would revolve with them. If the sun had an axial rotation of a different period from this, this atmospheric protuberance or tide wave would travel round the body of the sun.

68. The actual effect of planetary gravitation on the sun is to produce a disturbance of the *kind* above described, but in a most irregular manner. In the case supposed, the centre of gravity of the whole solar system, although not coincident with that of the sun's magnitude, would remain relatively constant, *i. e.*, the degree of its eccentricity would always be the

same. As it is, the planetary motions are so complex, and their relative positions are so perpetually changing, that the position of the general centre of gravity in relation to the mass of the sun is never the same for two consecutive seconds, and therefore the disturbances of the sun's motion, due to the reaction of planetary gravitation, must be most irregular and complex, especially if, as I shall endeavour to show hereafter, the greater portion of the bulk of the sun consists of gaseous matter, and the "nucleus" is but a comparatively small kernel in the midst of the gaseous mass. The gaseous matter would be lifted or bulged or undulated as a tide-wave, and dropped again, but the solid (or more probably viscous nucleus) would be dragged bodily about with an irregular reeling motion inside this profound undulating gaseous ocean, which it would thus stir into eddies, as the stroke of a fish's tail makes eddies in a pool.

69. Without attempting to define the nature of these irregularities, or even to estimate numerically the maxima and minima of the eccentricity of solar motion, I may safely affirm that they are quite sufficient to produce an enormous amount of disturbance of the solar atmosphere; a complication of clashing tides, of irregularities of velocity in the different portions of this vast atmospheric ocean, and the consequent formation of mighty maelströms, vortices and cyclones, hurricanes and tornados, of fury inconceivable to the dwellers upon this comparatively tranquil earth. Whether we regard the nucleus of the sun as reeling irregularly in the midst of his profound fluid envelope, or his atmosphere as dragged here by Jupiter, there by Venus, hither by the earth, thither by Saturn, and everywhere in the meantime by the vivacious Mercury, we cannot fail to perceive in this ever varying resultant of planetary attraction an agent for perpetually stirring up, interchanging, and mingling together the various strata of the solar atmosphere; by means of which the vigorous and newly-arriving fuel must be whirled into the midst of the photosphere, and

deeper still to the lower ocean of dissociated elements, while huge upheavals of the thermally exhausted atmospheric matter must find its way again to the upper regions of the rarefied atmosphere, where by its re-expansion it will be cooled below the general temperature of the interplanetary medium, and then swept round and carried into the wake of the sun.

70. That the solar atmosphere is in the state of agitation which should result from the above *à priori* considerations is so well known that I need scarcely refer to the observations of that great solar anatomist, Mr. Carrington, and that result of his minute and industrious dissections of the photosphere which proves that, while the equatorial portions of the photosphere make a complete revolution in 30·86 days, those at latitude of about 50° revolve in 28·36 days. Now when we consider the great velocity of translation which these figures represent, the amount of atmospheric disturbance due to such differences must be enormous; for whatever theory is entertained of the composition of the nucleus, its proper axial rotation should give to all parts of its surface the same period of revolution, and all the difference between the velocity due to such regularity and that of the atmospheric irregularity, must be represented by furious hurricane.

71. The mighty cyclones witnessed in actual progress, and described and figured by Padre Secchi and others; the abundant evidence now admitted by all solar observers to ratify the conclusion that the spots are but great whirlpools of the photosphere, the proper motion of these spots irrespective of the general revolution of the photosphere; the furious upheavals of the photosphere into the chromosphere, and of the chromosphere into the rarefied region beyond, all unite in proving the existence of a general and furious agitation of the solar atmosphere, which I think I may now regard as freely admitted by all solar observers. I need not therefore dwell any longer upon the question of fact; but my explanation, which attri-

butes these movements to the disturbing reaction of planetary gravitation, requires some further discussion.

72. It may be argued that the mass of the moon being, relatively to that of the earth, so much greater than the combined mass of the planets relative to that of the sun, we ought to have all these effects represented upon the earth. (The mass of the moon is $\frac{1}{80}$ of that of the earth, while that of all the planets is but $\frac{1}{750}$ of that of the sun.) And further, that the variable gravitation of the planets of the solar system being exerted upon the earth as upon the sun, should therefore produce similar atmospheric effects.

73. My reply to this is that the moon and the planets *do* act upon the earth and its atmosphere, and do produce atmospheric movements, and that those produced by the moon are of considerable magnitude, but the conditions of these movements, and the relative amount of their visible effects, is very different from those which must be produced in the vast atmospheric ocean of the sun.

74. First, as regards the great preponderating force of the moon. This must effect an atmospheric prolation such as I have described (67) when speaking of the supposed united and uniform gravitation of the planets on the sun. We are unconscious of its existence, because the lunar gravitation which uplifts the great atmospheric tide-wave, or rather *tide-protuberance,* supports that which it raises, and therefore the barometer is unaffected by it. From the regularity of the lunar action the rising and falling of this atmospheric tide would produce but a *relatively* small amount of disturbance of the atmospheric movements of translation upon the earth, or atmospheric currents. I can only venture to speak very generally upon the atmospheric currents which such tidal action of the moon is capable of producing, as it evidently presents a problem of considerable magnitude and complication, and one which appears to have been altogether neglected. It is not under the apex of the tidal protuberance that horizontal move-

ments of the atmosphere would be produced, but in its front and rear and lateral boundaries; especially where its updraw and outthrust, rendered more or less horizontal by the earth's curvature, confuses the gradations of normal horizontal atmospheric velocity which are due to the earth's rotation.

75. The atmospheric effects of planetary gravitation upon the earth must be very different from those upon the sun. The earth with its mere skin of atmospheric matter must be moved as a whole, retarded or accelerated bodily with its atmosphere in its orbital movements; the whole dimensions of the earth presenting little more than a mere point for uniform dynamical operation. The difference between the force of planetary gravitation upon different parts of the earth, upon the surface as compared to the centre, upon one side and the opposite side, upon the atmospheric matter as compared with the solid nucleus, is so small as to be practically inconsiderable, but upon the immense body of the sun the action is very different. Let us take Venus at mean distance for example. This distance being equal to about 77 solar diameters, the force of her attraction upon the near side of the sun is more than $\frac{1}{39}$, or above $2\frac{1}{2}$ per cent. greater than that upon the opposite side of the sun; thus the nucleus of the sun and all the different parts of the solar atmosphere are subject to sensibly variable degrees of attraction from the same planet. If then I am right in regarding the solid solar nucleus as having only a small fractional part of the visible dimensions of the sun, each planet will effect its own particular irregularity between the movements of the solar atmosphere and those of the nucleus within. As I have already stated, the smallest calculable degree of such internal independent reeling of the solar nucleus must produce tremendous atmospheric eddies, by stirring the lower depths of the solar atmosphere, where the momentum due to the movements of gases of such great density would be communicated to an upper and widening circle of lighter atmospheric matter, until its vorticose out-

break on the surface of the photosphere would be visible to human observers in the form of sun-spots.

76. This communication of momentum from one portion of the matter of the profound solar atmospheric ocean to another, and the consequent permanent retention of the whole of the atmospheric motion as such, is directly suggestive of a subject which has not, I think, received the attention it deserves, and which has a very important bearing upon this comparison between the solar and terrestrial atmospheric disturbances. I allude to *the friction of our atmosphere upon the surface of the earth*. The sum total of mechanical energy which is thus expended must be enormous, and its necessary effects are well worthy of careful study. In roaring up the mountain sides and through the valleys of the dry land; in bowing the trees of the forest and rubbing over the vast surface presented by its twigs and leaves, and in lashing the sea into foaming billows, our atmosphere is expending an amount of force which must have a very great effect in checking the momentum of such light material as its own. All the movements of our atmosphere are thus subjected to a steady continuous retardation, which must effectually, though gradually, prevent the *accumulation* of independent atmospheric motion. A large amount of heat must be evolved in exchange for the motion thus destroyed. This friction is the chain which binds our thin film of atmosphere to the solid earth, and compels it to follow the earth's rotation. Under the atmospheric conditions which I attribute to the sun the results must be very different. All the fury of the tropical tornado (*i.e.*, of the vortices that occur upon those portions of our earth corresponding to the "spot regions" of the sun) which with us is expended so destructively in devastating both sea and land, would there be employed in communicating similar motion to other atmospheric strata, thus developing, deepening, and continuing the cyclone, by expending the sum total of the original momentum on atmospheric movement exclusively.

77. I may here observe in passing, that it appears to me that the recent researches on the laws of storms have quite refuted the usually expounded theories, which attribute the great movements of our atmosphere to differences of temperature, and to the formation and condensation of aqueous vapour. No such explanations can cover the great diameter and the long excursions of the cyclones, to which so many of our storms have been traced. Cosmical causes are demanded to explain these. An island in a tropical sea, especially if its land is low and barren, presents us with about the strongest obtainable case of directly contiguous variations of temperature and moisture, and we know the results of this in the daily sea and land breezes which blow so gently and so gratefully, which extend scarcely a mile or two beyond the shore, and are accompanied with none of the characteristic effects of these great circular atmospheric movements. I suspect that there is more truth in the general popular notion respecting the ruling influence of the moon upon the weather than modern philosophers and their philosophy are at present prepared to admit. These notions appear to be traditional inheritances from the ancient astronomers, those shepherds of the East who rudely gathered so great an accumulation of facts, and whose position on the earth would render their climate more likely to be subject to traceable influences of the lunar atmospheric tides than our climate possibly can be.

78. That an actual connection between the disturbances of the solar atmosphere and the position of the planets does exist is shown by the observations of Mr. Carrington, who finds that the varying distances of Jupiter affect the development of spots, which are more abundant when this planet is furthest from the sun. M. Rudolph Wolf, of Berne, has observed that besides the well known period of 11·2 years there is also another period of maximum spot-development of about fifty-six years, which Mr. Balfour Stewart* has shown to correspond very nearly with

* In a paper communicated to the Astronomical Society, 1864, "On the large sun-spot period of about fifty-six years."

the epoch at which Jupiter and Saturn come to aphelion together.

79. The combined researches of Messrs. Warren de la Rue, Mr. Balfour Stewart, and Mr. Benjamin Loewy, indicate the predominant influence of Venus. In prosecution of these researches "Mr. Carrington's original drawings were examined by two observers noting the behaviour of each spot, and the results again compared with Carrington's published maps, which give the behaviour of spots from day to day; ultimately a list was obtained, no spot available for comparison being left out. A similar process was followed with regard to the Kew pictures." "It is to be observed that in making the examination of the Carrington pictures, both observers were ignorant of the planetary configurations; and that although with regard to the Kew pictures one observer knew the corresponding planetary configurations, yet his judgment being checked by his fellow-observer, could not be biassed by any previous speculative views."*

80. The main conclusions from these observations is thus expressed by the authors. "*Legitimate Deduction*—The behaviour of spots is influenced by something from without, and from the nature of the spot behaviour the authors conclude that this influence travels faster than the earth; and finally, they find that the behaviour appears to be determined by the position of Venus in such a manner, that a spot wanes as it approaches this planet by rotation, and, on the other hand, breaks out and increases as it recedes from the neighbourhood of the planet, reaching its maximum on the opposite side."

81. Upon inspecting the table which is included in this paper, I find that where Venus and Jupiter are opposed to one another the spots are described "uncertain behaviour," and it is quite evident from all the facts that the Kew investigations do not contradict the conclusions of Mr. Carrington respecting the influence of Jupiter, but merely add to it that of Venus. This struggle between the *proximity* of Venus and the *magnitude* of

* *Proceedings of the Royal Society*, February 2nd, 1865.

Jupiter is quite in accordance with my view of the action of planetary gravitation. The predominant influence of Venus is to be noted in connection with what I have said (75) respecting her variable gravitation on different parts of the sun.

82. In order to bring these investigations fully and directly to bear upon my views, the behaviour of the spots should not be compared with the positions of merely one or two of the planets, but the general resultant of the total planetary gravitation should be represented in direction and quantity, and this compared with the position, behaviour, and abundance of the spots. Such a comparison would serve as a fair *experimentum crucis* by which this hypothesis may be tested.

83. I regret extremely that I have not within reach the means of making such a comparison myself. To a professional astronomer, with all the necessary records and skilled assistants at his disposal, the calculations required might readily be made, and the comparisons instituted by reference to Carrington's plates and tables of daily observations, and the Kew pictures. It would also be extremely interesting to go back to those curious records of darkening of the sun and of spots seen before the invention of the telescope, which are referred to by Sir J. Herschel in his *Outlines of Astronomy,* art. 394 *a,* and to compare the positions of *all* the planets at these dates with their positions at spotless epochs, such as 1856; and to make similar comparisons with the more recently observed solar outbreaks of unusual magnitude.

84. In the meantime I may fairly claim the observations already referred to as confirming my explanation as far as they go. The mean gravitation of Jupiter upon the sun is about thirteen times that of the earth; that of Venus about two and a half times, and that of Saturn a little above equal. The gravitation upon the sun of either of the other planets is considerably less than that of the earth. It will thus be seen that the three planets which ought, according to my theory, have the greatest effect on the spots, are just those whose action has been ob-

served to influence them, though the observers appear to have had no idea that the influence is simply that of gravitation, but have looked for more recondite and obscure speculative forces to account for it: such as comparing the approach of two heavenly bodies to the light-producing action of the approach of atoms; or that it may be "merely that arrangement by means of which the visible motion of bodies is converted into light and heat, which we know from Professor Thompson are the ultimate forms to which all motion tends."*

85. Mr. Grove, in his Inaugural Address to the British Association at its Nottingham Meeting, 1866, propounded the following questions. He said, "Our sun, our earth, and planets are constantly radiating heat into space; so in all probability are the other suns, the stars, and their attendant planets. What becomes of the heat thus radiated into space? If the universe has no limit,—and it is difficult to conceive one,—there is a constant evolution of heat and light; and yet more is given off than is received by each cosmical body, for otherwise night would be as light and as warm as day. What becomes of the enormous force thus apparently non-recurrent in the same form? Does it return as palpable motion? Does it move, or contribute to move, suns and planets? and can it be conceived as a force similar to that which Newton speculated on as universally repulsive, and capable of being substituted for universal attraction? We are in no position at present to answer such questions as these: but I know of no problem in celestial dynamics more deeply interesting than this, and we may be no further removed from its solution than the predecessors of Newton were from the simple dynamic relation of matter to matter, which that potent intellect detected and demonstrated."

86. I think I may now venture to answer these questions,

* Mr. Balfour Stewart "On the large sun-spot period of about fifty-six years." A paper communicated to the Astronomical Society and quoted in *The Reader*, vol. iv., p. 77.

having shown that the heat thus radiated into space is received by the general atmospheric medium; is gathered again by the breathing of wandering suns, who inspire as they advance the breath of universal heat and light and life; then by impact, compression, and radiation, they concentrate and redistribute its vitalizing power; and after its work is done, expire it in the broad wake of their retreat, leaving a track of cool exhausted ether—the ash-pits of the solar furnaces—to re-absorb the general radiations, and thus maintain the eternal round of life.

CHAPTER VII.

THE QUANTITY OF SOLAR LIGHT.

ACTUAL AND THEORETICAL INTENSITY OF SOLAR LIGHT AND HEAT—OTHER COMBUSTIBLE GASES DIFFUSED WITH THE HYDROGEN AND CARBONIC OXIDE OF THE SOLAR ATMOSPHERE—METALLIC VAPOURS IN THE SOLAR ATMOSPHERE—COMBUSTION OF METALLIC VAPOURS IN THE PHOTOSPHERE —SIR JOHN HERSCHEL'S ESTIMATE OF THE SOLAR SPLENDOUR—DIFFICULTY OF EXPLAINING THIS DEGREE OF INTRINSIC SPLENDOUR—TRANSPARENCY OF FLAME EXAMINED—RADIATION OF ONE FLAME THROUGH ANOTHER—RADIATIONS FROM A TRIPLE FLAME—RADIATIONS FROM FIVE FLAMES—LUMINOSITY OF COMPOUND FLAMES—DEMONSTRATION OF THE TRANSPARENCY OF A COMMON GAS FLAME—COMPARATIVE AMOUNT OF LIGHT RADIATED FROM THE FACE AND EDGE OF A BAT'S-WING BURNER —CONCLUSIONS FROM PHOTOMETRIC EXPERIMENTS—THE LUMINOUS RADIATIONS FROM ONE FLAME ARE NOT ABSORBED IN PASSING THROUGH ANOTHER FLAME OF THE SAME KIND, BUT ARE FREELY TRANSMITTED —THE DIFFERENCE BETWEEN QUANTITY AND INTENSITY OF LIGHT— THE ILLUMINATION OF THE RETINA—THE BUDE LIGHT AND LIME LIGHT COMPARED—TRANSPARENCY OF FLAME EXPLAINS THE GREAT LUMINOSITY OF THE SUN—THE INTRINSIC SPLENDOUR OF THE SUN MAY BE VERY MODERATE—THE SURFACE OF THE MOON BRIGHTER THAN THE SURFACE OF THE SUN.

86. A VERY serious difficulty will probably have suggested itself to the reader of the previous chapters, viz., that the intensity of the light and heat of the sun, as usually stated, is vastly greater, surface for surface, than that which we obtain experimentally by the combustion of the elements of water. A little further examination of the necessary conditions of the supposed solar combustion will remove this difficulty. It will be convenient to take the *light* first.

87. I must explain at the outset, that though I have hitherto

spoken of the dissociated oxygen and hydrogen almost exclusively as the factors of the photosphere, I do not for a moment suppose that these gases can exist in anything like a state of purity in the solar atmosphere. The high temperature which effects the dissociation of water at the great pressure of the lower atmospheric regions of the sun, must also volatilize all the volatile materials on the surface of the solar nucleus, and the vapours of these must be disseminated throughout the solar atmosphere in accordance with the laws of gaseous diffusion.

88. Supposing the chemical elements of the body of the sun to be similar to those of the earth, we must expect to find the sodium, magnesium, calcium, and potassium of our sea water existing in the gaseous state diffused through the dissociated gases and vapours of the solar ocean, instead of dissolved in liquid water. The spectroscope has found them there accordingly. Besides these, we ought to find all those metals which are abundant on the earth's crust, and volatile at the temperature of melted platinum. With two or three exceptions all such metals have been detected in the solar envelope.*

89. Now the vapours of these substances (dissociated as they would be from oxygen), when thus diffused among the elements of water and burning with them, would give an intense luminosity to the flame of the photosphere. This, as

* Iron is not usually regarded as a volatile metal; and its abundance in the solar atmosphere may therefore be a matter of surprise to those who are only acquainted with this metal through books and laboratory experiments. My daily avocation, however, enables me to bear testimony to its volatility when sufficiently heated. Clouds of brick-red smoke, composed mainly of oxide of iron, are poured forth daily from the Bessemer Department of the works where I am employed (Sir John Brown and Co., Sheffield). I have seen this iron smoke quite unmixed with carbon—sufficiently dense to hide the mid-day sun. This red iron smoke is the most abundant when there is much silicon in the pig, and I suspect that it contains a considerable quantity of silicate of the oxide of iron, as this silicate appears to be more volatile than either metallic iron or its oxide. Silicious iron ores, such as exist so abundantly in the crust of our earth, would thus very readily supply the iron vapours which the spectroscope detects in the sun.

Dr. Frankland has recently shown, would be especially the case under high pressures and high temperatures. The luminosity of such metallic vapours burning in oxy-hydrogen gas, would far exceed that of our ordinary hydro-carbon flames, and would probably approximate to that of the lime-light, or that obtained from the combustion of magnesium.

90. But Sir John Herschel (*Familiar Lectures*, page 66) tells us "that the most intense lights we can produce artificially are as nothing compared *surface for surface* with the sun;" and then comparing the light of the sun with the lime light, he says that "the brightness, the intrinsic splendour of the surface of such a lime ball is only one 146th part of that of the sun's surface." He then proceeds as follows, "That is to say, the sun gives out as much light as 146 balls of quicklime, *each of the size of the sun*, and each heated *all over its surface* in the way I have described." These quotations are from the words addressed to popular readers, but Sir John Herschell is not one of those who sacrifice accuracy to popular illustration; and therefore I may take them as expressing what some of our greatest philosophers understand in reference to this subject. It will be seen from them that 146 suns of lime-ball luminosity, and one sun with a surface of 146 times the luminous intensity of the lime-ball, are regarded as synonymous. If we only consider the effect produced by the sum of their radiations, doubtless they would be equivalent; but in looking for the means by which such effect is produced, a very serious difference is presented.

91. We may easily illuminate a large hall by means of 146 lime-balls, but to concentrate on the surface of one lime-ball of the same size 146 times the amount of intrinsic splendour, is enormously beyond the reach of all known physical powers. In like manner, to suppose that every portion of the sun's surface has this degree of brilliancy is to assume the existence of what is, according to all terrestrial experience, a supernatural luminosity. Of course I do not presume to say that it

is therefore impossible, but I do maintain that before stepping so far beyond the limits of all experience, we should look very searchingly for an explanation more consistent with what we know of the order of nature.

92. This is not difficult to find in the properties of a common gas flame. Such a flame is nearly, if not wholly, transparent to its own light, so that the effective radiation of a flame of given area is proportionate to its *thickness*. This is a most important principle, quite familiar to gas engineers, but scarcely recognized and certainly not properly appreciated in our philosophical treatises. Being unable to find any record of reliable experiments directed to the determination of this question, I have availed myself of the kind assistance of my friend, Mr. Jonathan Wilkinson, of Grimesthorpe, the Official Gas Examiner to the Sheffield Corporation, who kindly placed at my disposal his apparatus,—a Bunsen's photometer of the best construction, with 100-inch graduated bar, erected in a chamber with blackened walls, and provided with pressure guages, experimental meters, etc., etc.

93. A fish-tail burner with a measured supply of gas was lighted at one end of the photometer, and used as our standard in preference to the wax-candle unit, which is very variable. At the opposite end of the scale a single burner of the same kind was lighted, and regulated until its light exactly balanced the first, with the screen midway. A second burner of the same kind was now lighted behind this, with a space of about a ¼ of an inch clear between the flames; this twin arrangement was moved until the two burners were equidistant on each side of the place previously occupied by the one burner. The quantity of gas supplied to these two burners was now regulated to exactly double that consumed by the one burner, and the photometer screen moved till the light of these two burners (the radiations from one of which had to pass entirely through the other on their way to the screen) was balanced by the standard burner. It is obvious from this arrangement that if

the flame nearest to the screen was at all opaque to the radiations of the light behind it, the photometer would measure less than double the illumination due to the single burner. I was surprised to find that it registered more than double. The experiment was repeated with the same result, the average indication being that the two burners gave 2·15 times the light of one.

94. A third burner of the same kind was next lighted, and this triplet arrangement of fish-tail flames so adjusted that the middle burner occupied the place of the single burner in the first experiment, and the face of only one burner was presented to the photometer, so that the light from the hindmost burner had to pass through two flames, that of the middle burner and that facing the screen: while the light from the middle burner had to pass through one flame. On regulating the supply of gas to three times the consumption of the single burner, we found that the light received by the screen was 3·2 times greater than from the single burner.

95. The second series of experiments was with a fixed row of five simple jets, issuing from single round holes. These were arranged in line so that only one of the end jets faced the photometer screen, and all the radiations from the others must pass through it to reach the screen. The middle jet was first lighted and adjusted to balance a standard jet of the same kind. Then three jets—the middle jet and one on each side—were lighted, and the supply adjusted to three times the quantity of gas consumed by one jet. The screen now registered 3·25 times the amount of light received from one jet. When to these three were added the outside jets, making five jets, and the consumption of gas was five times as much as one jet, the photometer indicated 5·35 times as much light as from the single jet. The above figures are the mean of several experiments with these simple jets, which gave very variable results. The lower the flames the greater was the proportionate increase upon increasing the number of jets, and when the one,

three, and five jets were turned up so high as to produce a somewhat smoky flame, there was no increase, the light then varied in nearly the same rates as the number of burners and supply of gas. A similar difference was observable as regards the length of the flames. A short flame, white only half way down, was elongated in its white portion by the proximity of other flames; *i. e.*, three such flames burning three measures of gas were each longer and whiter than a single flame burning one measure; but if the flame was in the first instance turned up to its full height, no such elongation occurred.

96. A third series of experiments was made with a long row of simple jets from round holes. These holes were about a ¼-inch apart, so that the flames touched each other, and formed one flat sheet of flame when combined. A single jet was first lighted, regulated to about 1½ inches long, and the consumption of gas noted. Then three, five, and seven and more jets were lighted and supplied with three, five, and seven, etc., times the quantity of gas. The first thing remarked in this experiment was the surprising development of length of each jet when thus lighted together and thus supplied. While the flame of the single jet was but 1½ inches high, the broad flame of the combination of jets reached to nearly double this height with the same quantity of gas supplied to each jet as to the one singly; the height was very irregular, and the combustion appeared to be less perfect, but more favourable to luminosity. The photometer confirmed this very remarkably; so much so as to open quite another question, viz., the philosophy of the Argand, fish-tail, and other compound burners, which produce so very much more light with a given quantity of gas than would the jets of which they are composed, if burning separately. The results we obtained were very interesting, but require many repetitions and modifications to render them worthy of publication. To have made these would have carried me away from the object upon which I am engaged, and delayed the publication of this Essay; therefore

I proceeded no further, but hope that Mr. Wilkinson and his son will continue them, and publish their results.

97. In order to eliminate from the question of the transparency of flame the interference which, in the above described experiments, was due to the mutual action of one flame upon another, I made the following experiment, which so far as it goes is, I think, satisfactory and conclusive. I took a combination of five simple round jets arranged in line, and about one inch apart; each jet provided with an independent tap, and the whole row of jets turning upon an axis under the middle jet. These were regulated so that when all were burning the length of each jet should be equal to that of its neighbours, and the tops of the flames, if connected, form a straight line. This row of jets was then turned at right angles to the axis of the photometer bar, so that all five jets were presented to the screen. Their light was then balanced by the standard burner. The line of jets was next turned, so that instead of five jets being presented to the photometer screen, only one was presented, and the radiations from all the other burners must pass through this one on their way to the screen. *The photometer indicated exactly the same quantity of light as when all the five jets were directly presented.* The experiment was continually repeated, Mr. Wilkinson, his son, and myself alternately watching the photometer screen, while another was turning the row of jets; the screen observer not seeing which way the jets were arranged. Sometimes we imagined that there was a slight difference in favour of the broad-side presentation of the jets, but on attempting to determine the position of the jets by watching the screen when they were turned, we were as often wrong as right in our guesses based upon the supposed increase of light.

98. When, however, the height of the jets was irregular there was a decided difference in favour of the arrangement which presented all the jets to the screen. This is easily explained by the fact, that in such a case the portions of flame rising above the level of the other jets must radiate their light

across the vapours formed by the combustion of these lower flames, and such vapours would of course absorb a portion of this light. Mr. Joseph Wilkinson had previously observed that a bat's-wing burner, if presented edge-wise to the photometer screen, radiated to it a little less light than when its flat broad surface was presented. The bat's-wing burner is very irregular in its upper outline, and its edges are much thicker than the middle portions; so that the distant projecting portions of the upper parts of the flame, and of the distant edge, would have to radiate through the upper stratum of vapour, and that rising along the face of the flame respectively.

The loss of light due to this amounted to about 1½ per cent. upon the bat's-wing burner with which I repeated the experiments. In like manner, when the sheet of flame formed by the union of seventeen small jets was turned so as to alternately present its edge and face to the photometer, there was about this degree of loss of light with the edge presentation. This flame was very irregular in height, and all the jets being fed by the same tap we could not separately regulate them.

99. From these experiments I conclude, *First*, That under some circumstances an increased body of flame is favourable to increased intrinsic brilliancy of the whole of the flame, as in the case of a continuous flame formed by the union of a number of small jets; but this I attribute merely to the fact that the gas is thus caused to burn under conditions more favourable to luminosity, or to the conversion of obscure heat rays into luminous rays of shorter wave periods. This conversion takes place most remarkably when a number of small jets, burning with excessive supply of oxygen, are united, and the supply of oxygen is thereby reduced to the minimum quantity necessary for combustion. The maximum amount of light—under a given pressure—is obtained from a hydro-carbon flame when it is supplied with such a minimum of heated oxygen, the greater portion of which can only reach it after combustion has commenced and the gases are thereby highly heated.

Secondly, That a hydro-carbon flame burning under given conditions is nearly, if not absolutely, transparent to its own radiations; that its luminosity increases with its depth or thickness; or in other words, its effective luminous radiation varies with the cubic quantity of its flaming matter, and not merely with its superficial area; and that this effect is not accompanied with any increased brilliancy or intensity, but is merely due to cumulative radiations producing effects corresponding to the sum total of their concurrent energies.

100. If we assume that the mutual radiations of different flames or of different strata of the same flame upon each other produce any increased intensity or intrinsic splendour of portions of the flame itself, we are carried into a very curious dilemma. This will be perceived by supposing that in the case of the three flames radiating through each other, the end flame presented to the photometer had radiated three times its own quantity of light in consequence of acquiring that degree of increased brilliancy from the other flames. If this were the case with the flame on one side it must also be the case with the one at the opposite side, and if with these two, it must be also the case with the middle flame exposed to a battery of rays from both sides. Thus each flame would be three times as brilliant, and the three should radiate nine times as much light as one flame. But this is not all, for with this threefold brilliancy of each flame a further threefold augmentation must recommence, and each flame would now be nine times as bright as at first; then these flames of ninefold brilliancy would go on illuminating each other or increasing each other's intensity in ninefold further degree, and so on to infinity, or until the three little gas lights had dazzled us all to death. The same reasoning applies, and the same absurdity follows, if we assume even the smallest degree of mutual intensification of brilliancy from this mutual radiation between flames. We must therefore conclude that the increased luminosity due to increased thickness

or repetitions of strata of flame is a result of transparency of the flame to its own radiations, and not to any absorption producing increased brilliancy or intensity of any portion of the flame.*

101. When, however, the radiations are received upon an opaque reflecting screen the case is quite different. The increased illumination of the opaque surface, which is produced by the radiations from each additional layer of flame, is due to increased intrinsic brilliancy of that surface. We are thus presented with a very intelligible and instructive distinction between *quantity* and *intensity*. The quantity of light radiated from the three flames is three times greater than from the single flame, while the intensity of each flame remains the same. When however the light from all three is received upon an opaque reflecting screen, the result of this increased quantity is only manifested there by increased intensity; the illumination of the screen being necessarily superficial. If the screen were

* The term "incandescent gas" is sometimes used in a manner that leads to considerable confusion by its ambiguous applicability either to flame proper, or merely to gas and vapours intensely heated. The above experiments go to show that the properties of a flame are the opposite of those of heated gases in reference to the absorption and transmission of their own quality of light. The heated or incandescent gases absorb the peculiar quality of light which they are capable of radiating, and convert it into heat. This has been attributed to their power of assuming a synchronous atomic vibration, but I think it better explained by supposing that they have a power of resistance which changes these particular undulations into waves of greater amptitude and longer period;—from the luminous to the calorific.

This important difference between flame and incandescent gases is, I think, worthy of further investigation, and with other considerations may justify the conclusion that flame should be classed as another and distinct condition of matter, in addition to those of the solid, liquid, and gaseous forms. If this be admitted we shall revert to the classification of the ancients; to the four elements of "fire, air, earth and water." Although they used the term "element," their meaning was quite different from that of our modern acceptation of the word. It was not elementary *constituent*, but elementary or necessary *condition*, or state of existence which they intended to describe by the term element. Their methods of study were directed to the necessary and logical, rather than the actual and observable, properties of matter.

partially transparent, so that the radiations received could penetrate some depth and illuminate a measurable thickness, it would, surface for surface, have less intensity of illumination, or intrinsic splendour; but this would be compensated by the increased *quantity* of illumination due to the illuminated thickness. Quantity and intensity of light are thus exchangeable; a given amount of luminous force may be manifested either in one form or the other.

102. The retina of the human eye being an extremely thin film of nervous matter spread upon a nearly opaque screen, can obtain only increased *intensity* of illumination from increased quantity of light; thus the visible splendour of a distant object such as the sun is the same whether its luminosity is due to quantity or to intensity of light. The convoluted retina of the eagle, etc., may possibly act somewhat differently.

103. The "Bude Light," consisting of a number of consecutive hollow cylinders of gas flame, with all the inner cylinders radiating through those exterior to them, produces a high degree of illumination by the *quantity* of its light. The small opaque lime-ball may give an equal illumination by the *intensity* of its light, *i. e.*, by the "intrinsic splendour" of its surface.*

104. I have dwelt thus much upon the above, as it has a most important, indeed a vital bearing upon my hypothesis. If called upon to account for the astounding luminous *intensity* which has been attributed to the visible surface of the sun, I

* The transparency of the white and luminous hydro-carbon flame strongly supports the conclusions of Dr. Frankland, who has recently laid violent hands upon one of the landmarks of science by questioning the theory of Sir Humphrey Davy, which ascribes the luminosity of flame to incandescent solid particles. If the white portion of the flame consisted of such particles of solid carbon, so closely packed together as to appear continuous, they must by their opacity intercept a large amount of light, and no approach to transparency would be possible.

should utterly fail; but when it is understood that a flame may transmit freely the light of other flaming matter like itself, without suffering any disturbance of its own radiation, and that these radiations may be cumulatively received by distant bodies in such a manner that increased thickness of flame may produce effects corresponding to increased brilliancy, the whole difficulty vanishes, and nothing more is required to explain the immensity of the solar luminosity than a photosphere of vulgar hydrogen flame of sufficient depth, differing only from our common gas or candle flame in being highly charged with gaseous metals, instead of gaseous hydro-carbons. If my explanation of the origin and composition of the photosphere is correct, its depth must be measurable in miles, and being supplied, not by superficial oxygen, as our ordinary flames, but by oxygen intermingled throughout, the original dissociated oxygen is exactly sufficient to supply the hydrogen, the additional free oxygen of the mixed nitrogen and oxygen being there to supply any further demand of the metallic vapours. The depth of the flaming photosphere must therefore be limited only by the possibilities of radiation.

105. Thus the difficulty presented by the comparatively low intensity of the theoretical source of solar light is removed by the effective radiation due to its *quantity*. We may conceive the photosphere as subdivided into any number of films or laminæ of the smallest appreciable thickness, and if the total effective radiation is equal to the sum of the radiations of these films, the smallest conceivable amount of intrinsic brilliancy of each film may radiate the greatest conceivable amount of light, provided the number of such layers be sufficiently multiplied. All that is necessary for the complete fulfilment of these conditions is perfect transparency of the flame to its own radiations. I have shown that a common gas flame has a high degree of such transparency, but further and more elaborate experiments upon much greater thicknesses of flame are necessary in order to determine the limits of this transparency, if it be not perfect.

106. In spite of the apparent paradox, I do not hesitate to express my belief that "surface for surface," "the brightness, the intrinsic splendour" of the bright face of the moon is far greater than that of the sun. We know that it is composed of solid and nearly opaque matter, and that such matter can radiate only from a superficial film of merely nominal thickness (if it were *perfectly* opaque it could radiate only from a surface having *no* thickness). The light radiated from the moon is calculated at $\frac{1}{800,000}$ part of that from the sun. Let us suppose that the material of the moon's surface has such a degree of translucency that the light of the sun may penetrate to a depth of $\frac{1}{10,000}$ of an inch, and so illuminate it that all this thickness is concerned in reflecting the light which reaches the earth. Let us now suppose that we have a flame perfectly transparent to its own light, and of such brilliancy that a film or sheet $\frac{1}{10,000}$ of an inch in thickness should, surface for surface, radiate as much light as the moon; it is obvious that we should only require to give a thickness of 80 inches to such a flame in order to obtain from it the full glare of sun light. No flame with which we are acquainted possesses anything approaching to this degree of intrinsic splendour. Thus, if my views are correct, the *surface* of the moon must be brighter than the *surface* of the sun; and the peculiarities of moonlight are mainly due to great intensity and relatively small quantity of light, while those of sunlight are produced by immense quantity with comparatively moderate intensity.

CHAPTER VIII.

THE QUANTITY OF THE SOLAR HEAT.

DIFFERENT WAYS OF STATING THE ESTIMATES OF THE SUN'S HEAT—RADIATION FROM ATHERMOUS AND DIATHERMOUS BODIES—PRELIMINARY EXPERIMENTS ON THE DIATHERMACY OF COMMON GAS FLAMES—FURTHER EXPERIMENTS ON THE DIATHERMACY OF COMMON GAS FLAMES—CONCLUSIONS FROM THESE EXPERIMENTS—THEORETICAL REASONS FOR CONCLUDING THAT ALL FLAMES ARE DIATHERMOUS TO THEIR OWN RADIATIONS—EFFECTS OF ATHERMACY ON THE CONSTITUTION OF A FLAME—THE BESSEMER FLAME—THE PRINCIPLE OF SIR JOHN HERSCHEL'S AND OTHER CALCULATIONS OF SOLAR TEMPERATURE—A VERY HIGH SOLAR TEMPERATURE NOT REQUIRED TO EXPLAIN THE SOLAR RADIATIONS—A SOLAR TEMPERATURE EQUAL TO THAT OF BURNING HYDROGEN IS QUITE SUFFICIENT TO EXPLAIN ALL THE EFFECTS OF SOLAR RADIATION—SIR JOHN HERSCHEL ON "THE REDUPLICATION OF SHEETS OF FLAME"—SIR JOHN HERSCHEL ON THE IMPOSSIBILITY OF COMBUSTION IN THE SUN—THE MORAL INFLUENCE OF THE STUDY OF PURE TRUTH.

107. ACCORDING to my hypotheses the temperature of the visible solar surface or photosphere cannot exceed that of the dissociation of water at a moderate pressure. This is so very much lower than the temperature which has been determined by some calculations, that it presents the same apparent difficulty as that already discussed in reference to the solar luminosity. Sir John Herschel estimates "*the temperature,* that is to say, the degree or intensity of the heat at the actual surface of the sun," as "more than 90,000 times greater than the intensity of sunshine here on our globe at noon and under the equator." Otherwise, that the heat radiated from every square

yard of the sun's surface is equal to that which would be produced by the burning of six tons of coal per hour. Poillet expresses the results of his experiments and calculations by describing the heat of the sun as sufficient to melt in one minute a layer of ice 11·80 metres thickness; or in one day, 16,992 metres or 4¼ leagues.* It has been further represented as capable of boiling 700,000 millions of cubic feet of ice-cold water per hour. Here, as will be at once perceived, we have two distinct methods of representing the amount of solar heat; the first by *intensity of temperature,* the second by *quantity of work done.*

108. If the sun is an athermous solid body, radiating only from its actual mathematical surface, nothing short of "the temperature, that is to say, the degree or intensity of the heat at the actual surface of the sun," stated by Sir John Herschel, will explain the thermal effect of his radiations. A surface temperature exceeding 13,000,000° Fahr. will be required under these conditions. If, on the other hand, the radiating matter is wholly or partially diathermous to its own special quality of heat, the necessity for anything like such a temperature is at once removed. All that I have stated in reference to solar light will then apply with equal force to the thermal radiations. A question exactly analogous to that which I have already discussed in reference to the transparency of flame is thus opened, viz., Whether increasing the thickness of a flame of given temperature increases its effective thermal radiations? My own experience in the working of furnaces generally, and more especially of reverberatory furnaces, where the effective heating agent is a body of flame, convinces me that such is the case; but as such conviction is not readily transferable to those who have not had similar experience, I have made the following simple experiments.

109. A laboratory thermometer with graduated stem, such

* *Eléments de Physique,* tome ii., p. 716.

as commonly used for fractional distillations, etc., was fitted transversely by means of a cork in a tube of long oval section, this shape being selected as nearly corresponding to that of a simple gas jet. The tube was polished inside and outside, stopped at the end near which the thermometer was placed, and open at the other end; the bulb of the thermometer was slightly blackened by holding it in a candle flame. The distance from the bulb of the thermometer to the open mouth of the tube was 4 inches. Some preliminary experiments were roughly made with one Bunsen burner, placed opposite the mouth of the tube, and then when the thermometer had ceased rising, another burner of the same kind was placed behind the first; this produced a further rise; then a third was placed behind this, and a still further rise took place. A similar experiment was made with the three fish-tail burners described in sec. 94; and with the arrangement of five jets, described in sec. 95. In all these trials it was found that the rise of temperature was, as nearly as the rough apparatus could measure, proportional to the quantity of gas consumed, and *that combining several jets into one flame produced no increase in the proportional amount of heat radiations,* as was the case with the light. That is to say, two or more jets consuming a given quantity of gas, radiated the same amount of heat whether their flames touched each other or burned at a short distance apart; but this was not the case with their luminous radiations, which were very much increased immediately the two or more flames, well supplied with air, came in contact.

110. This being the case I was able to use a tube pierced with a row of seventeen holes, ¼ inch apart, which when charged with gas gave jets which united to form one flat sheet of flame. The thermometer in its reflecting tube was placed so that the bulb was six inches distant from the outer jet. The plug of the middle jet was first withdrawn, and the thermometer allowed to rise until it became stationary. It registered 19° Centigrade. Then a jet on each side of this middle

jet was lighted, and the consumption regulated to an additional 0·5 of a cubic foot per hour for these two jets. The thermometer then rose to 23°. Two more jets, one on each side of the first three, were then lighted and regulated to the same additional consumption as the second and third; these five jets raised the thermometer to 27°; seven jets in like manner gave 31°; nine jets 35½; and so on, as shown in the following table.

Number of Jets.	Consumption of Gas in cubic feet per hour.	Highest reading of Thermometer.
1	1·0	19° Centigrade
3	1·5	23° ,,
5	2·0	27° ,,
7	2·5	31° ,,
9	3·0	35½° ,,
11	3·5	39° ,,
13	4·0	43½° ,,
15	4·5	48° ,,
17	5·0	53° ,,

It will be understood that in all these experiments the *mean* distance of the thermometer from the flame remained the same, as the extensions of the flame were equal on both sides of its centre. Three experiments were repeated with no perceptible variation. The higher rate of increase of temperature observable towards the end of the experiment was obviously due to the warming of the tube in which the thermometer was inserted; and the small interval between the 9 and 11 jets was due to irregularity in the size of the jets, the nearer jet being smaller than the further one. The experiment when made with the five jets, each capable of separate regulation, gave remarkably consistent results.

111. These crude experiments, though inadequate to determine the very interesting and important question of whether a flame is *absolutely* diathermous to its own quality of heat, are sufficient for my present argument; they show that to a very

great extent the quantity of effective thermal radiation of a flame is increased by increasing its thickness without any increase of intensity;—that practically a flame may be regarded as diathermous. My experiments simply indicate the absence of any degree of athermacy which a thermometer reading only to half a degree of centigrade is capable of detecting. Having no delicate thermo-electric apparatus at my disposal, I do not attempt the philosophical solution of the question of absolute diathermacy, or in its absence that of the quantitative athermacy of flame.

112. Independent theoretical considerations, however, appear to me strongly to support, if not to demonstrate, the assumption that all flames are absolutely diathermous to radiations of their own special quality and intensity. The theory of exchanges demands that it should be so. The laws of dissociation are equally imperative; for if a flame absorbed any portion of the heat radiated by the adjacent portions of itself without an exactly equivalent and simultaneous exchange of radiations, the temperature must be raised in proportion to the amount of such absorption. Such elevation of temperature would at once produce dissociation, and a suppression of further combustion, and thus the flame of the mixed gases could be only a hollow shell having a thickness proportionate to the degree of its diathermacy.

113. To illustrate this, let us suppose such a flame to be capable of absorbing *all* the heat radiated towards it, and perfectly athermous. In such a case only the absolute external surface of the flame could radiate outwards, as its external athermous film would shut back all radiations from the interior, and the evolution of heat which is necessary for the maintenance of combustion could only take place from the absolute surface of the gas, which would thus be a shell of flame having but an infinitesimal thickness. Any degree of diathermacy permitting radiation to take place from the interior, and across the flame, would increase the possible thickness of this shell,

proportionately to the extent of such possible radiation through it. Perfect diathermacy would confer the possibility of solidity to the flame, provided the surrounding bodies were capable of receiving the sum of the radiations proceeding from every portion of its thickness. Now we know by familiar experiment that a solid flame is readily obtainable by the combustion of the elements of water. This fact is confirmatory of the diathermacy of flame, and its power of cumulative thermal radiation.

114. The same reasoning applies to the flame of carbonic oxide or of hydro-carbons, in fact to all flames. The finest example we have of an artificial solid flame is that which roars from the mouth of a large Bessemer converter. The amount of light and heat radiated from this flame strikingly illustrates the effect of thickness. It is a dazzling object even in the midst of sunshine, although its intrinsic splendour is but small, as may be seen by the thin waifs of flame that are blown aside from the main body.

115. The principle upon which Sir John Herschel's and other calculations of the temperature of the sun has been made, is to ascertain the amount of heat which the solar radiations communicate to a solid body at the equator at noonday. Then, after making allowances for atmospheric absorption, the area of a sphere having a radius equal to the earth's distance from the sun is divided by the area of the solar surface, and the total terrestrial temperature produced by the solar radiations is multiplied by the quotient thus obtained. Now it is obvious that in such calculations, where the solar heat is expressed in *temperature* or *intensity*, this temperature must be limited to the *absolute surface* of the sun, which must, therefore, be regarded as absolutely athermous; for if the slightest conceivable thickness is given to the radiant matter of the sun, the basis of these calculations is disturbed, and their results are altogether fallacious.

116. This will be seen by following up the principle of such calculations. The area of a sphere having a radius of 91,678,000

miles is, in round numbers, about 46,000 times greater than the area of the solar surface. If all the heat radiated to this greater sphere came from the *absolute surface* of the sun, the temperature of this surface must be 46,000 times higher than the total temperature communicated to this outer sphere. But for the sake of illustration, let us suppose that, instead of one athermous radiating surface, the radiant matter of the sun consisted of 46,000 concentric layers, closely packed, each one perfectly diathermous, and nowise disturbing the radiations of those behind it, so that the total effective radiation should equal the sum of the radiations of all these layers. What then must be the temperature of each of these layers? Obviously the same as that of the surface of the outer sphere, *i. e.*, the maximum equatorial terrestrial temperature. In the above, I have, for simplicity sake, omitted the absorption of the chromosphere, etc.

117. Keeping these principles in view, together with the fact that an ordinary gas flame possesses the degree of diathermacy and power of cumulative thermal radiation which my simple experiments have rudely demonstrated, there is no further difficulty in accounting for all the known thermal effects of solar radiation upon the hypothesis that his visible and radiant surface consists of a photosphere of flaming gases of considerable depth, having a temperature not exceeding that which is produced by the combination of the elements of water.

118. I have just met with the following sentence in a foot note to the tenth edition (1869) of Sir J. Herschel's *Outlines of Astronomy* (p. 261). "It would be a highly curious subject of experimental enquiry, how far a mere reduplication of sheets of flame, at a distance one behind the other (by which their light might be brought to any required intensity), would communicate to the heat of the resulting compound ray the *penetrating* character which distinguishes the solar calorific rays." It is evident from the manner in which this important suggestion is dropped by the way, and has never been followed

up,* that Sir John Herschel had but little hope of its application to aid the solution of the solar mystery, for in the same note he suggests electricity and friction as more possible sources of solar light and heat than combustion, and in the paragraph to which the note is appended he says, "The great mystery, however, is to conceive how so enormous a conflagration (if such it be) can be kept up. Every discovery in chemical science here leaves us completely at a loss, or rather seems to remove further the prospect of probable explanation."

119. In his *Familiar Lectures,* published thirty-four years after this note was written, he says, "The light and heat of the sun cannot possibly arise from the burning of *fuel,* so as to give out what we call flame. If it be the sun's substance that *burns* (I mean consumes), where is the oxygen to come from? and what is to become of the ashes and other products of combustion? Even supposing the oxygen supplied from the material, as in the cases of gunpowder, Bengal light, or gun cotton, still the chemical products have to be disposed of. In the case of gun cotton it has been calculated that if the sun were made of it so condensed as only to burn on the surface, it would burn out at the rate of the sun's expenditure of light and heat in 8,000 years. Anyhow, fire kept up by fuel and air is out of the question. There remain only three possible sources of them so far as we can perceive,—electricity, friction, and vital action."

120. This is terribly contradictory to all I have been striving to establish in the preceding pages; and if Sir John Herschel were a political debater, or a theological controversialist, I might expect very severe castigation for my presumption: but fortunately for the human race, the men who have profoundly and faithfully devoted their lives to the study of pure science are so far raised by its moral influence as to stand exceptionally above the mists of prejudice and per-

* It is quoted as a "*Note to the Edition of* 1833."

sonality, and thus their perception of truth is not obscured by the medium through which it is conveyed. Though offered by the humblest outside student, it is accepted by them as frankly, fairly, and courteously as though presented by the highest recognized authorities. The range of their intellectual vision is not limited by any deadening orthodoxy, nor stultified by blind conservatism; and therefore if I am wrong, my errors will not be denounced as heresies, and thereby galvanized and martyrized into a mischievously protracted vitality; but they will be calmly refuted, and thus effectually annihilated. If my views are sound, the previous expression of different opinions will not prevent their free acceptance by Sir John Herschel, or any other true philosopher. This is why I have not burdened my pages with apologetic phrases whenever I have ventured to differ from high scientific authorities.

CHAPTER IX.

THE GENERAL PHENOMENA OF THE SUN-SPOTS.

OTHER CONSEQUENCES OF SOLAR AGITATION TO BE CONSIDERED—THE STRATA OF THE SOLAR ENVELOPE—THE ORIGIN OF THE CHROMOSPHERE—THE SPHERE OF ABSORPTION—THE ORIGIN OF THE SUN-SPOTS—THE ACTUAL TELESCOPIC APPEARANCES OF THE SUN CORRESPOND EXACTLY WITH THEORETICAL ANTICIPATIONS—MOBILITY OF THE SOLAR SURFACE LEVEL OF THE SPOTS AND THEIR RELATIONS TO THE PLANETS—VORTICOSE ORIGIN OF SUN-SPOTS—EXTRAVAGANT HYPOTHESES TO ACCOUNT FOR UMBRA AND PENUMBRA OF SPOTS—THE BODY OF THE SUN HAS NEVER BEEN SEEN—A SOLID OR LIQUID SOLAR NUCLEUS SHOULD BE BRIGHT—THE SPECIFIC GRAVITY OF THE SUN CONTRADICTS HYPOTHESIS OF A VISIBLE NUCLEUS—THE HEAT OF THE SUN MUST CONTINUE INCREASING DOWNWARDS TO HIS CENTRE—PROBABLE DIMENSIONS OF THE SOLAR NUCLEUS—THE DEPTH OF THE SUN-SPOTS—THE UMBRA OF THE SUN-SPOTS—THE PENUMBRA AND THEORETICAL APPEARANCES OF A TYPICAL SUN-SPOT—SOME OF THE NECESSARY DEVIATIONS FROM TYPICAL CONDITIONS—THE GENERAL PHENOMENA OF THE UMBRA AND PENUMBRA ACCORD WITH MY HYPOTHESES.

121. It will now be interesting to follow out in further detail some of the other necessary consequences of the furious agitation of the solar atmosphere, and the stirring up and mingling together of the strata of dissociated gases, flame and vapours, already described. In doing so, I think I shall be able to confirm the statement I have made in Chapter IV. (46) respecting the harmony of the telescopic details of solar phenomena with my theoretical deductions.

122. It must be remembered that, according to what I have stated, there should be three distinct and well defined strata in

the solar atmosphere. 1st. The lower ocean of dissociated gases, which, being restrained from active combustion by the separating force of excessive heat, must, if seen in contrast with the luminous photosphere, appear as a dark abyss of indeterminable depth.

2nd. A stratum of flame produced by the combustion of these gases at the elevation where diminished heat and external radiation renders their combination possible;—this stratum forming the photosphere, or visible surface of the sun, the brilliancy of which varies with its depth or thickness.

3rd. An upper or outer atmospheric region charged with the vapours resulting from the combustion of the photosphere.

123. This sphere of vapour demands some special consideration. From what I have already stated, it is clear that the vapour of water should be its *chief* constituent. It is also evident that this is by no means the only vapour that must be there, if the body of the sun is composed of any such materials as exist upon the earth. That it does contain such materials, is proved by recent spectroscopic investigations. The temperature of 2,800° Cent. is sufficient to volatilize iron, and most of the common metals; and the vapours of these metals should exist in accordance with the laws of gaseous diffusion, amidst the separated hydrogen and oxygen, and, like the hydrogen, be dissociated from oxygen. They will therefore burn in the photosphere, and the vapours of their oxides will be mingled with the gaseous water that envelopes the photosphere. The more volatile metallic oxides, such as sodium, etc., will continue gaseous at perceptible distances from the surface of the photosphere, while the vapours of iron and most of the heavy metals must soon be precipitated in a state of discrete particles or cloudy matter. To those who are conversant with the recent progress of telescopic and spectroscopic investigations of the sun, I need scarcely say that such a stratum of aqueous vapour mingled with the vapours of the lighter metals, and holding in suspension other precipitated cloudy metallic vapours, exactly

corresponds to that observed envelope of the photosphere which Mr. Lockyer has named the "chromosphere." As I shall show more fully hereafter, the quantity of discrete vaporous oxides of refractory metals existing in the chromosphere, must be greatly augmented by the irruptions known as the "solar prominences."

124. It will be understood that, immediately overlying the photosphere, there must be a stratum so highly heated as to retain even the less volatile metals, or their oxides, in a truly gaseous state. This will be the region of absorption to which most of the black bands of the solar spectrum are due. The depth of this absorption sphere will vary for each substance, according to the temperature at which its vapour begins to condense. It will be in such direct contact with the photosphere, and so intermingled by agitation and the different combining temperatures of the different substances, that it must, to the observer, be practically undistinguishable from the photosphere.

125. From what I have stated respecting the planetary disturbances of the solar rotation, the photosphere, or visible surface of the sun, must present all the appearances due to the movements of a fiery ocean raging and seething in the maddest conceivable fury of perpetual tempest. If the surface of a river flowing peacefully between its banks is perforated with conical eddies whenever it meets a projecting rock, or any other obstacle or agency that disturbs the regularity of its course, what must be the magnitude of the eddies in this ocean of flame and heated gases, when stirred to the lowest depths of its vast profundity by the irregular reeling of the solar nucleus within? Obviously nothing less than the sun-spots;—those mighty maelströms into which a world might be dropped like a pea into an egg-cup. I will now proceed briefly to show that these and all the details of their attendant phenomena are but the natural and necessary consequences of the conditions I have already explained.

126. If the telescope had not yet been invented, and I were to follow up the preceding theoretical sketch of the

chemical and mechanical movements of the solar envelopes, by a further theoretical elaboration of what might be seen by a magician, who by means of the enchanted carpet of the "Thousand and One Nights" should transport himself 150 times nearer to our central luminary, I must have used almost the precise words in which our best telescopic observers describe the general appearances of the sun-spots. I should have described the luminous envelope as ripped open by the tearing strain of opposing currents, revealing huge cavities whose sloping banks of flame exhibit a gradual diminution of brilliancy by presenting to the observer a gradually diminished thickness of photosphere; until they reach the bottom of the sea of flame, and there display a deeper shade—a central dark spot—revealing to the sight of the magician the region of dissociated gases. I should have described these rents and vortices in a condition of continual movement, and quoting the description of the imaginary magician, I might have said, "When watched from day to day, or even from hour to hour, they appear to enlarge or contract, to change their forms, and at length to disappear altogether, or to break out anew in parts of the surface where none were before. In such cases of disappearance the central dark spot always contracts into a point, and vanishes before the border. Occasionally they break up, or divide into two or more, and in those cases offer every evidence of that extreme mobility which belongs only to the fluid state, and of that excessively violent agitation which seems only compatible with the atmospheric or gaseous state of matter." These, I say, are the words I might have attributed to my imaginary magician, if Galileo had not taught us how to actually weave the enchanted carpets, of which the Oriental poets only dreamed; but as the case now stands, I quote the above from Sir John Herschel's *Outlines of Astronomy,* article 386.

127. The Padre Secchi, in a letter from Rome, dated August 8th, 1865, and printed in the *Reader,* August 19th,

says, "As to the mobility of the solar surface, you can judge from the two photographs that I send you; they have been made only at an interval of twenty-four hours. I think we assisted at the outbreaking of the spot, and at its arrangement from a general confusion of movements into a regular transformation of an ordinary group of spots. The appearance which I have seen is quite like that which takes place when a great movement is excited in a stream of running water, which finally resolves itself into some vortices which take their course independently. The movement of these spots even alone is capable of demonstrating materially what Mr. Carrington has found with great labour—that there is in the sun a real drift of matter, since without this it would be impossible to explain how the spot had been increased in two days to a length twice as great as its breadth, this remaining almost constant."

128. Mr. Spottiswoode, in summing up the general results of solar observations, in his address as President of the Mathematical and Physical Section of the British Association, at the meeting of 1865, says, "It may be as well to remind the section that it seems now to be established that the solar spots are at a lower level than the penumbræ, the faculæ at a higher; that the photosphere is gaseous, and that the behaviour of the spots as to appearance and disappearance is connected with the position of the planets, and principally with that of Venus."

129. I have collected a formidable array of quotations from the works of the most eminent solar observers, beginning with the unanswerable argument of Galileo (who contends that when two neighbouring spots are observed near the centre of the disc, with a bright interval between them, if the spots were protuberances this line would decrease as the spots approached the limb, and would soon disappear because one of the spots, if it were a projection, would hide it), and concluding with the most recent stereoscopic demonstrations, and the actual measurements of velocity of downrush, obtained by means of the admirable researches and reasoning of Frankland and Lockyer

on the thickening of the spectroscopic lines of hydrogen; in order to support that explanation of the sun-spots which regards them as vortices or eddies in the photosphere; a conclusion which is so confirmatory of what I have said and have yet to suggest; but I abstain from further protracting the argument on this point, the progress of investigation having in the meantime carried the question beyond the discussion stage, and justified us in regarding the violent agitation of the photosphere and the vorticose structure and origin of the sun-spots as established facts.

130. When this is admitted in conjunction with my view of the solar atmospheric strata and their properties, the great difficulties of the umbra and penumbra vanish. That these have presented very tremendous difficulties is evident from the very tremendous hypotheses that have been despairingly ventured by some of our greatest philosophers. Nothing short of philosophic desperation could have dragged forth the idea of a central nucleus enjoying a temperature suitable for the cultivation of cucumbers, by virtue of a non-conducting and reflecting envelope, protecting it from the fierce glare of the fiery atmosphere above; and that a sun-spot is merely a gap in the fiery film, affording us a glance at these pleasant regions below.

131. Putting together and comparing the various descriptions of different observers, I find good grounds for concluding that the body of the sun has never been seen at all. When the photosphere is broken through in any degree whatever, a darker region is presented, but there is no well-defined line of separation between the light of the photosphere and the blackness of the supposed body of the sun; on the contrary, there is every gradation from white to grey, with mottlings of grey and white, from light grey to dark grey, and from dark grey to gloom, and from this again to central blackness. Mr. Dawes has shown that the umbra and penumbra are but degrees of the same, and that the umbra itself is pierced, and shows the still darker "nucleus" underneath. Thus the "nucleus"

has receded beyond the umbra; yet this umbra was regarded as the nucleus until better instruments, or more skilful observation, showed it to be pierced by a still darker depth. As this darkest is so very far from absolute blackness, more penetrating observation, or a deeper spot, may reveal a darker still, and so on for several gradations. Had the lowest depths of the greatest spots revealed a brighter speck, with a rim of darkest umbra separating it from the outlying lighter shades of the penumbra, there would have been better grounds for believing that a central solid or liquid body had been seen.

132. We know that perfectly gaseous bodies may be intensely heated without becoming luminous, and that if a solid be immersed in a gas in this condition, it becomes luminous in proportion to the intensity of the heat. This is also the case with liquids and vesicular vapours. Taking those metals whose vapours have been shown to exist in the sun, viz., sodium, calcium, barium, magnesium, iron, chromium, nickel, copper, zinc, strontium, cadmium, cobalt, manganese, aluminium, and titanium, we know that any of these, whether in the solid or liquid state, if immersed in a highly-heated non-flaming gas, would be more luminous than the gas itself; therefore, if the solid or liquid body of the sun were at the bottom of the spot, it would be seen as a *brighter*, not as a *darker* nucleus, as compared with the umbra of heated and dissociated gases. The absence of such a bright centre to the spot does not, of course, prove the non-existence of a solid or liquid nucleus, but merely that it has not been seen. It might be there yet veiled from human view by intervening gases or vapours filling the spot, and absorbing those particular rays of light which the nucleus radiates.

133. From other considerations I conclude that this idea of finding a solid nucleus of the sun anywhere at all within reach of human vision, however aided by the telescope, is a most unphilosophical expectation. The mean specific gravity of the sun is rather more than one-fourth of that of the earth, or not

quite 1½ times that of water. Now none of the elements which the spectroscope has discovered in the sun could exist in a solid or liquid state, and subject to the enormous pressure which such a mass must exert upon the inner portions of itself, without suffering an amount of condensation which would raise their mean specific gravity far above 1½ times that of water. The "solid body" of the sun, as usually described, makes most unreasonable demands upon the pure imagination of the philosopher, who must invent a new form of matter of which to construct it. Neither can he take refuge upon a solid crust enveloping a central gaseous or liquid mass; such as some geologists attribute to the earth as the result of gradual cooling by radiation. There can be no such cooling down of the surface of the solar nucleus, even if the violent hypothesis of a non-conducting and athermous atmospheric stratum between it and the photosphere were to be revived; for a solid crust thus placed between two fires would speedily be fused.

134. The dissociated gaseous matter, of which I have hitherto spoken merely as a lower atmospheric envelope, must continue to enormous depths, if it does not penetrate to the centre; for even the lightest of the solar gases,—the hydrogen itself, if not subject to the counteracting expansive power of heat, would in the inner portions of such a mass be condensed far beyond the observed specific gravity of the sun. Nothing short of a continuation and concentration of the solar heat down to the very centre of the sun can reconcile what we know of the specific gravity of the sun with what we also know of his chemical composition; and the idea of a *cooler* nucleus composed of any known form of matter, either solid, liquid, or gaseous, should, I think, be finally abandoned.

135. A solid, or rather a viscous nucleus, probably does exist somewhere towards the centre of the sun, but its dimensions in relation to the surrounding gaseous matter must be something like that of a peach-stone to its surrounding pulp. The density of such a solid or viscous nucleus must be many

times greater than the calculated density of the sun; but if it be surrounded by a gaseous envelope vastly exceeding itself in volume, and consisting at its outer or more bulky portions of atmospheric matter many times less dense than water, the *mean* density of the whole orb, measured as we measure it by the outline of the photosphere, may thus be brought down to the calculated specific gravity of the sun.

136. If the views above expressed are correct, the sun-spots are cavities of far greater depth than has generally been assigned to them. If we watch the eddies in a clear deep stream that is flowing past the pier of a bridge or other impediment, we may, if the stream is not too rapid and confused, trace the whole depth and dimensions of these vortices. On doing so we shall see that the depth to which the air is carried down into the water is commonly four or five times—and occasionally eight or ten times as great as the horizontal diameter of the vortex at its widest part. We must remember that in this case the air which is being thus dragged down below the surface of the water is much lighter than the water surrounding it; that it is all the while fighting against this forced immersion by its tendency to rise to the surface; while the eddies of the solar atmosphere are subject to no such healing or levelling force as that which is due to the great difference between the specific gravity of air and water. In the sun the walls of the whirlpool, and the material which it is dragging downward, are of nearly the same composition and density, and therefore we may expect a still greater proportionate depth. This must be still more strikingly the case if the vortices are originally due to the reeling of a deeply seated central nucleus such as I have already described. According to this, one or two hundred thousand miles is not an unreasonable estimate of the depth of some of the largest sun-spots. As will be seen hereafter, this question of the depth of the sun-spot vortices has a very important bearing upon the explanation of other great solar phenomena.

137. As I have already stated, the varying shades of the penumbra, umbra, and so-called nucleus of the spots, present no difficulty at all, if my general explanation of the structure of the sun be accepted. The region of dissociation, though occupied with intensely heated gases, and absolutely luminous, must, by contrast with the flaming photosphere, present just that degree of darkness which is actually observed, for here we have a body of gaseous matter having, in relation to the transmission of light, the opposite properties to those which I have shown to be possessed by flame. The dissociated gases, instead of presenting a cumulative result of all the radiating strata behind their presented surface, resist the passage of such luminous rays by what is usually described as absorption;* thus increase of depth gives no increase of luminosity; and in looking into this region at the bottom of a spot we merely see the

* I have already expressed my scepticism respecting this, and my opinion that "*conversion*" would better describe the action (100). Dr. Tyndall compares absorption to the resonance of a string, or other vibratory solid, taking up the waves of sound of a particular pitch, and re-emitting them in all directions. There is no loss of the particular note in this case, no acoustic "dark band," no "beat," or anything analogous to the "absorption" bands of the spectrum. If the incandescent gases absorbed the light in this manner, and re-emitted it thus, the dark bands could not be thereby produced, as the quantity and quality of light actually emitted would still remain the same. It appears to me that the dark bands must be explained by the *conversion* of the particular kind of light into another form of force such as heat, or into other light of a different wave period; that they are not *absorption bands*, but *conversion bands*. When light passes through a partially transparent substance such as water, and illuminates the water in the course of its transmission, there is true absorption of light; and the amount of light truly absorbed is measured by the intensity and quantity of illumination of the water, but when light is received upon a surface of lamp black, a totally different kind of action occurs. The application of the same term to both of these actions is certainly unphilosophical. If a piece of dry chalk is moistened with water, the chalk *absorbs* the water, which still remains as water in the pores of the chalk. If a piece of quick lime is moistened with water, the lime *combines* with the water, and the water ceases to exist; it is converted into a totally different substance. The chemist would stumble into serious confusion if he described these two actions by the same name.

intrinsic splendour of the exterior film of the gas, nearly as we should see the red-hot surface of one sheet of iron, which would be nowise increased in brilliancy by placing a thousand other sheets of the same temperature behind it; while a thousand such sheets of flame would be a thousand times as brilliant as the one sheet.

The darkest region of the spots, the "umbra" or "nucleus," is thus easily explained, both as regards its relative darkness and its position at the lowest depths of the cavity.

138. The phenomena of the penumbra and all the gradations of shade that have been observed in the spot cavities, admit of equally simple and natural explanation. I have endeavoured to show that the brilliancy of the photosphere varies with the depth of flaming matter from which the luminous radiations are proceeding. For illustration, let us suppose the case of a perfectly symmetrical funnel-shaped cavity of circular outline, and that it is situated on the centre of the sun's disc, so that the terrestrial spectator will be looking perpendicularly down its axis, and that it is deep enough to penetrate beyond the total thickness of the photosphere. It is obvious that the spectator, in thus examining the walls of such a cavity, must be looking through a gradually thinning stratum of the photosphere, as he directs his observation deeper and deeper down its sides, which should thus exhibit a gradually and somewhat regularly diminishing luminosity from the outer edge towards the centre. The limit of this regular gradation would be the depth of the photosphere; beyond this a somewhat sudden increase of darkness should be observed so soon as the sides of the funnel passed the border of the photosphere, and came upon the heated but non-flaming gases. I say a *somewhat* regular gradation, because the perfect regularity due to the varying thickness of the photosphere will be subject to the interference resulting from the vaporous and flaming matter which must be carried down with the vortex, and the gyratory contortion to which the surface of the funnel-shaped opening

would, of necessity, be subject. This would give a streaked, mottled, and irregular appearance to the shading, and would blur the line of separation between the lower edge of the photosphere and the upper surface of the non-luminous gases.

139. The exact fulfilment of all these conditions of symmetry are, of course, not likely to be realized; and all the phenomena described must actually be subject to great disturbances of the supposed regularity. The more irregular the shape of the spot and the greater its distance from the centre of the sun's disc, and the consequent obliquity of the line of vision, the more irregular must be all these appearances which I have only described above in typical form. That the penumbra should be streaked and striated as the best observers describe it to be, and that the umbra itself "always presents the appearance of varied shades, as if the penumbra and umbra were mingled, and mixed up their tints in varied proportions;"* is only a necessary result of the down-rush of the flaming matter of the photosphere, which must be dragged in broken streaky sheets down the vortex, even to its lowest depths. Besides this, there would probably occur some further combustion within the cavity, consequent upon the absorption of the cooler vapours of the chromosphere, which would render possible the combination of a limited quantity of the dissociated gases.

140. All the above theoretical deductions respecting the general appearances of the umbra and penumbra are strictly verified by telescopic observation. In the next chapter I will proceed to examine some of the minor details of these lights and shades of the sun-spots and solar surface; for, if I am right, the lesser and the greater phenomena should receive an equally consistent explanation.

* *The Heavens,* by A. Guillemin, edited by Lockyer, page 35.

CHAPTER X.

THE VARYING SPLENDOUR OF DIFFERENT PORTIONS OF THE PHOTOSPHERE.

THE DARK RIM OF THE SUN-SPOTS—MY EXPLANATION OF THE DARK RIM—ABSORPTION DOES NOT EXPLAIN THE DARK RIM—RADIAL STRIATION OF SUN-SPOTS—MY EXPLANATION OF RADIAL STRIATION—THE "WILLOW LEAVES"—MR. NASMYTH'S DESCRIPTION OF THE WILLOW LEAVES CONTROVERTED—SIR JOHN HERSCHEL'S AND PADRE SECCHI'S DESCRIPTION OF THE SOLAR SURFACE—MR. DAWES'S DESCRIPTION OF THE SOLAR SURFACE—MR. GUILLEMIN'S DESCRIPTION OF THE SOLAR SURFACE—MR. BRODIE'S DESCRIPTION OF THE SOLAR SURFACE—MY EXPLANATION OF THE MOTTLING, ETC., OF THE SURFACE OF THE PHOTOSPHERE—MY EXPLANATION OF THE "THATCH STRAWS," ETC.—FACULOUS PROJECTIONS OVER THE SUN SPOTS—THE SOLAR PROMINENCES—THE GREAT IRREGULARITY OF THE SURFACE OF THE PHOTOSPHERE—THE SUN-SPOT "BRIDGES"—THE "FACULÆ"—MY EXPLANATION OF THE FACULÆ—IS THE PHOTOSPHERE DEEPER ON THE SUMMITS OF THE FACULÆ THAN IN THE VALLIES BETWEEN THEM?—THE EFFECT OF THE VARYING DENSITIES OF THE CHROMOSPHERE UPON THE MAXIMUM RADIATION OF DIFFERENT PORTIONS OF THE PHOTOSPHERE—BRILLIANCY OF THE FACULÆ NEAR THE LIMB OF THE SUN.

141. THERE is a remarkable feature of the sun-spots which no hypothesis I have yet met with has attempted to reach. I refer to the dark rim at the outer edge of the spot which has been described by so many observers. That it is an actual darkening, and not a mere optical illusion due to contrast with the bright platform of facula surrounding the spot, is, I think, proved by its peculiar irregularities. If it were an optical result of contrast, it should form an unbroken line following the edge

of the spot; but instead of this the appearance described and drawn is that of a mottled or striated shading, *the striæ always directed inwards* or towards the centre of the spot. This is shown very clearly in the engravings from Professor Phillips's drawings of spots observed on November 11th and 13th, 1865, published in the *Proceedings of the Royal Society* for December, 1865; and still more distinctly in the drawings which illustrate his paper on "A Zone of Spots on the Sun," published in the *Proceedings of the Royal Society,* vol. xv., p. 68, March 22nd., 1866. He says, "The penumbra had broken edges and an interior mottling of small brighter and darker spaces, *directed variously towards the umbræ.*" I may also refer to the drawings of a spot by Nasmyth, reproduced on page 32 of Guillemin's work on *The Heavens,* already quoted, and to another illustration from Lockyer on page 42 of the same work. The oft-described "serrated edges," the "thatch" and "willow leaves" pointing inwards upon the penumbra, are further illustrations of this breaking of the dark rim.

142. I have no difficulty in explaining these appearances. The initial velocity of a sheet of the photosphere, when projected into a spot-cavity, must be greater than the subsequent velocity. In proceeding towards its journey's end, down such an opening, it must be subject to retardation. Thus in its first bending over, the sheet of flame will be stretched out and thinned, then swollen, or heaped more or less by retardation and concentration; and, finally, extinguished more or less completely by dissociation in the lower regions. From this conflict between varying velocities, diminishing circumference of cavity, cooling action of associated chromospheric matter, and heating from below, the greatest irregularity must result. That amidst all this irregularity such a sudden darkening of the outer edge of the penumbra should *generally* be presented, is quite in accordance with theoretical expectations.

143. This dark rim presents a very serious objection to the explanation which attributes *all* the darkness of the spots to

absorption. That such absorption does play an important part I think Mr. Lockyer's investigations have fairly proved; but this dark region of the circumference is an "*instantia crucis*" refuting its exclusive claim, as it is obvious that the down rushing vapours must be thinned at the edges, and their absorptive efficiency must increase from circumference towards the centre.

144. My hypothesis perfectly explains the striation proceeding towards the centre of the spot, as such a stream drawn from the billowed surface of the stormy photosphere would, of necessity, be furrowed in the direction of its current, and most deeply furrowed at the edge where the waves are first bending over. The ridges would be bright and the furrows darker. The completeness of this explanation will be better understood if I quote the words of Padre Secchi, from a letter dated "Rome, August 11, 1865," and published in the *Reader* of 19th August, 1865. The italics are his own. Speaking of the "willow leaves," he says, "Now let us come to the penumbra of the spots and its structure. According to what I have seen, they are these little *things* which, *flowing bodily from the surrounding photosphere* on to the chasm of the penumbra, and directing themselves towards the nucleus, give to the penumbræ the radiated form which has been observed by many long ago. During this course or voyage, they seem occasionally to increase in bulk, so that they are more plainly visible, and being projected on a blacker field, and isolated, they are seen even in conditions of atmosphere insufficient to show them in the general field of the sun. Sometimes I have seen them detached and melting away, which happened yesterday in the small round spot, when one of these little *things* was detached from the serrated edge, and went near the centre of the spot, where it dissolved in a short time. Sometimes they unite in very long lines, and melt together, forming a round-edged stream, as I described so long since as 1852. I think, therefore, that the willow leaves are to be distinguished from these currents, and that only these are those called *thatch straws*, of

which the Astronomer Royal speaks. But this distinction does not include that they are in reality different things, since the thatch-straw or currents may be produced by the melting and dissolving of the willow-leaves, which are certainly not solid things. These are disposed in a converging direction towards the centre of the spot when the spot is round and regular, but they are disposed in every direction when the spot is irregular or dissolving."

145. I might multiply similar quotations to show that a stream of flaming matter flowing from the photosphere into the spot, and varying in brilliancy according to its thickness, explains even the most minute details of spot phenomena. It will be seen from the above that these regular radial striæ occur just when and where they should according to my explanation; they are symmetrical and regular in their convergence "when the spot is round and regular," and "disposed in every direction when the spot is irregular or dissolving." An irregular spot evidently consists of several pits or eddies combined more or less confusedly; the probable result of several opposing streams due to variations in relative *vertical* velocity, *as well as* to variations in relative *horizontal* velocity. Such a combination of irregularities must exist if planetary disturbance produces the eccentric reeling which I have described. The "round and regular" spot would correspond to the minor eddies of a stream, resulting from the horizontal irregularities of velocity produced by the friction of its banks, or the piers of a bridge; while the large irregular spots correspond to those wave-like and semi-vorticose hollows and surgings which may be seen when a rapid stream passes over a boulder of rock rising abruptly from the bottom, without reaching the surface. In most of the spots the characters of both of these should be combined. The breaking up of the radial striation when the spot is dissolving is in most obvious and perfect accordance with my explanation.

146. All who have watched the progress of the recent un-

veiling of the sun must be acquainted with the famous "willow-leaf" controversy, which followed Mr. Nasmyth's startling announcement that he had discovered that "The bright surface of the sun consists of separate, insulated, individual objects or *things*, all nearly or exactly of one definite size and shape, which is more like that of a willow-leaf, as he describes them, than anything else. These leaves or scales are not arranged in any order (as those on a butterfly's wing are), but lie crossing one another in all directions, like what are called spills in the game of spillikins; except at the borders of a spot, where they point for the most part inwards towards the middle of the spot, presenting much the sort of appearance that the small leaves of some water-plants or sea-weeds do at the edge of a deep hole of clear water. The exceedingly definite shape of these objects, their exact similarity to one another, and the way in which they lie across and athwart each other (except when they form a sort of bridge across a spot, in which case they seem to affect a common direction, that, namely, of the bridge itself), all these characters seem quite repugnant to the notion of their being of a vaporous, a cloudy, or a fluid nature. Nothing remains but to consider them as separate and independent sheets, flakes, or scales, having some sort of solidity. And these flakes, be they what they may, and whatever may be said about the dashing of meteoric stones into the sun's atmosphere, etc., are evidently the *immediate sources of the solar light and heat*, by whatever mechanism or whatever processes they may be enabled to develop and, as it were, elaborate these elements from the bosom of the non-luminous fluid in which they appear to float. Looked at in this point of view we cannot refuse to regard them as *organisms* of some peculiar and amazing kind; and though it would be too daring to speak of such organization as partaking of the nature of life, yet we do know that vital action is competent to develop both heat, light, and electricity."*

* *Familiar Lectures on Scientific Subjects*, by Sir John F. W. Herschel, p. 83.

I quote this account by Sir John Herschel, not having the means of reference to Mr. Nasmyth's own words.

147. This description of the solar surface was warmly controverted by Mr. Dawes and other solar observers, who deny very positively "the definite shape of these objects," their "exact similarity," and "the way in which they are said to lie athwart and across each other;" *i. e.*, all the characters which "seem quite repugnant to the notion of their being of a vaporous, cloudy, or a fluid nature;" but they admit the existence of luminous mottling, which they variously compare to "rice grains," "crystals," "flocculi," "granulations," "straws," "things," "bits of white thread," "cumuli of cotton wool," "clouds," "excessively minute fragments of porcelain," "untidy circular masses," "things twice as long as broad," "three times as long as broad," "ridges," "waves," "hill knolls," etc., etc.

148. Sir John Herschel's description of his own observations in his *Outlines of Astronomy*, art. 386 and 387, does not at all accord with the view of fixity and solidity above stated. Speaking of the spots, he says, "Occasionally they break up, or divide into two or more, and in those cases offer every evidence of that extreme mobility which belongs only to the fluid state, and of that excessively violent agitation which seems only compatible with the atmospheric or gaseous state of matter. Many other circumstances tend to corroborate this view of the subject. The part of the sun's disc not occupied by spots is far from uniformly bright. Its ground is finely mottled with an appearance of minute dark dots, or *pores*, which, when attentively watched, are found to be in a constant state of change. There is nothing which represents so faithfully this appearance as the slow subsidence of some flocculent chemical precipitates in a transparent fluid, when viewed perpendicularly from above: so faithfully, indeed, that it is hardly possible not to be impressed with the idea of a luminous medium intermixed, but not confounded, with a transparent and

non-luminous atmosphere, either floating as clouds in our air, or pervading it in vast sheets and columns like flame, or the streamers of our northern lights."

I have already quoted one of Secchi's accounts of these "things." He further says, "The general ground of the sun is made up of these oblong bodies, *but of every form and dimensions.* A great many black pores show that the photosphere is not a continuous stratum, but at the first sight it appears made up of *little lumps like so many cumuli of cotton wool.*"*

149. The Rev. W. R. Dawes says, "The term *willow-leaves* seemed utterly inapplicable to anything I had ever succeeded in discovering. A far less objectionable term, as it appears to me, is that of *rice grains,* applied by Mr. Stone to those objects with which all careful sun observers must be acquainted, as there is no difficulty in seeing them in a moderately favourable state of the air, and which have been familiar to myself for many years; so much so, indeed, that when they are not discernible, I invariably abstained from any further scrutiny of the solar surface as being useless. Yet even this appellation conveys the idea of uniformity of shape and size which these objects do not possess, and is, I think, on that account objectionable. But I have been led by it to apply the term *granulations,* or *granules,* which assumes nothing either as to exact form or precise character."† In a letter from Mr. Dawes to Mr. Pritchard, published in the *Reader,* 14th May, 1864, he says, speaking of the details of these appearances, "It was nearly four year before I was completely satisfied on the subject; it was not in fact till I had completed the construction of my new solar eye-piece, which enabled me to place any portion of the sun's surface on a small field, and to examine the identical objects with every variety of power, and under circumstances fit for the use of 400

* *Reader,* 19th August, 1865, p. 210.

† *Ibid.,* 27th August, 1864.

to 600 with advantage. I thus arrived at the decided conviction that these brilliant objects *were merely different conditions of the surface of the comparatively large luminous clouds themselves*—ridges, waves, hill-knolls, or whatever else they might be called, differing in *form*, in *brilliancy*, and probably in *elevation*, and bearing something of the same proportion to the individual luminous clouds that the masses of bright *faculæ*, as seen near the sun's edge, bear to the whole disc of the sun. As I arrived at this conclusion twelve years ago, and as my subsequent observations have only confirmed my conviction of its correctness, I fear that *further looking* is not likely to alter it." (The italics are Mr. Dawes').

150. M. Guillemin says, "We may begin by saying that the whole surface of the sun, except those portions occupied by the spots, is *coarsely mottled:* and indeed the mottled appearance requires no very large amount of optical power to render it visible. It has been often observed with a good refractor of only 2½ inches aperture. Examined, however, with a large instrument, it is seen that the surface is principally made up of *luminous masses*, described by Sir W. Herschel as "*corrugations*," and small points of unequal light, imperfectly separated from each other by rows of minute dark dots, called *pores*, the intervals between them being extremely small, and occupied by a substance decidedly less luminous than the general surface."*

151. Mr. Brodie, whose instrument is an equatorial of 8½ inches clear aperture and 11½ feet focal length, constructed by Messrs. Cooke and Sons, referring to the "rice grains" says, "These *waves*, or *ridges of photospheric cloud*, seemed to have an irregular outline of elevation, such as a cumulus cloud generally presents, so that the top of the very wave or ridge that causes the mottled appearance on the sun was itself most irregular in the outline of its upper surface. The sides of these waves or ridges were of very great inclination, not very greatly

* *The Heavens*, p. 41.

removed from the perpendicular; the indentations or valleys between them intersecting each other most irregularly."

152. Any further quotations I think are unnecessary; the foregoing will be sufficient to show that in spite of the apparent contradictions in the details of these observations (contradictions rather in the mode of describing the observations than in the observations themselves), they almost unanimously combine their testimony in favour of the explanation which follows as a necessary consequence of my view of the constitution of the sun. According to this, the flaming ocean of the photosphere must be in a state of furious tempest, with mighty tide waves and billows, and lesser ripples on these, and mountain tongues of flame all over the surface. The crests of these huge waves, and the summits of these flame-alps presenting a greater depth of flaming matter, must be brighter than the hollows and the valleys between; and besides this their splendour must be further increased by the fact, that such upper ridges and summits are less deeply immersed in the outer ocean of absorbing vapours, which limits the radiation of the light as well as the heat of the photosphere. The effect of looking upon the surface of such a wild fury of troubled flame, with its confused intermingling gradations of luminosity, must be most puzzling and difficult to describe; and unless the observers had been in a balloon over a burning city, or a prairie on fire during a hurricane, their terrestrial experiences must have utterly failed to supply any objects with which to compare the appearances presented.

153. The "thatch straws" overhanging the spot borders and producing the serrated edges, otherwise attributed to the sailing of the willow-leaves towards the spot, are necessary consequences which might be predicted were they not observed; for if there is a downrush towards the spot, it must drag the flame-pinnacles towards it, so that their summits must bow over the cavity, and their points be projected within it. It is remarkable that Mr. Dawes and others who deny any regu-

larity of outline or direction to the bright specks on the general surface, all concur in excepting the case where they are near a spot. All admit regularity and elongation there. The radiated arrangement and the thatch-like overhanging are thus described in the Monthly Notices of the Astronomical Society for August, 1865, "You get in the penumbra near the edge of the photosphere, sometimes pointed, sometimes rounded, sometimes truncated cloud masses *with a sharpened portion towards the umbra, and a very blunt portion towards the general solar surface.*" This is exactly the form and appearance which such bending summits of flame-tongues should exhibit.

154. "Mr. Lockyer has observed that on one occasion a tongue of faculous matter, projecting over a spot, lost its brilliancy very rapidly, so as ultimately to seem less brilliant than any portion of the penumbra. At the same time it seemed to be "giving out," as it were, at its end, and a portion of the umbra between it and the penumbra appeared to be veiled with a stratus cloud evolved out of it."* This appears to have been a case in which one of the higher and temporary jets of flame (of which I shall speak more fully hereafter as forming the solar "prominences") has been drawn into the general vortex, and the cause of its great extension not being constant, it has "given out," or burned itself out, leaving behind the aqueous vapour and other combustion products to form the "stratus cloud evolved out of it," which by their absorption "veiled" the penumbra, or left a track "less brilliant than any portion of the penumbra."

155. That the solar surface is thus covered by mountain waves and tongues of flame, is now proved by the direct observations of Mr. Lockyer, M. Janssen and others. The above was written before the particulars of their recent observations with the widened slit of the spectroscope were announced, or I should scarcely have sought so far for indirect evidence of what

* *Reader*, vol. vi., p. 657.

is now so directly demonstrated. Mr. Lockyer finds that the great solar prominences which have so astonished and puzzled the astronomers of this generation are only larger developments of what exists on all parts of the solar surface, and that all of these are really vulgar hydrogen flames, or mixtures of flame with heated gas and vapour. He says, "We may use either the red, or yellow, or green light of hydrogen for the purpose of thus seeing the shape and details of the prominences. I have been perfectly enchanted with the sight which my spectroscope has revealed to me. The solar and atmospheric spectra being hidden, and the image of the wide slit and the part of the prominence under observation alone being visible, the telescope or slit is moved slowly, and the strange shadow-forms flit past, and are seen as they are seen in eclipses. Here one is reminded by the fleecy, indefinitely-delicate cloud-films of an English hedgerow with luxuriant elms; here, of a densely intertwined tropical forest, the intimately interwoven branches threading in all directions, the prominences gradually expanding as they mount upwards, and changing slowly, indeed almost imperceptibly."*

156. Lieutenant Herschel gives similar descriptions, and accompanies them with sketches. From these and from other evidence which has now become too abundant for quotation, it appears that the surface of the photosphere, wherever viewed, is covered with mountains of flame of stupendous magnitude and indescribable irregularity.

I have shown that this undulating surface stretched out and flowing into the spot-cavities may produce the striation of the penumbra and the umbra of the spot; that the taller flame-tongues bending over as they sail down the fiery cascade, explain the "thatch" or "willow-leaf" projections over the edge of the spot, and that still taller jets of flame of temporary existence fully account for all the phenomena that Mr. Lockyer observed in connection with the "tongue of faculous matter projecting over a spot."

* *Macmillan's Magazine*, August, 1869, page 371.

157. The following quotation from a letter by M. Chacornac to the *Reader,* vol. v., page 16, carries us a step further in the same direction:—"The first observation of this kind which I was able to make with precision dates from April 3rd, in 1853. It was in following assiduously the changes presented by a large spot from nine in the morning till four in the afternoon, scarcely taking the eye from the telescope, that I saw how a large portion of the photosphere, in the shape of a horse shoe, joined itself to the *faculæ* at the edge of a spot, and, separating from them, appeared like an isolated cloud over the centre of the nucleus." Here we have a still larger fragment of the photosphere, *in the shape of a horse shoe,* still further overhanging. One step more brings us to the comparatively common phenomena of the luminous bridges crossing the spots, which have been described as willow-leaves or faculous matter "setting sail" across the spot. The *horse-shoe* shape of the portion of photosphere described by M. Chacornac at once suggests the "great horn," the "scimitar-shaped," the "boomerang," the "goat's horn," and other tall curved prominences which have been observed starting from the edge of the sun during total eclipses. As these are recorded by photography, there can be no mistake about their shape. Given such a scimitar or boomerang-shaped mountain of flame some sixty or eighty thousand miles long, (which is below the observed dimensions of some of these,) and let it be near to a spot and its curvature bending over the spot, the observer looking upon it from the earth must see it projected across the spot as a luminous band or bridge. A perpendicular prominence, if it rises between the observer and a spot situated anywhere on the outer portion of the sun's globular disc, must be visually projected across the spot. Such prominences are now proved to be chiefly composed of hydrogen flame; and if I am right respecting the transparency of flame and its light increasing with its thickness, their luminosity must be equal to their own brilliancy plus that of the luminous matter shining through them.

158. The *faculæ* still remain to be explained.—They are described by Mr. Lockyer as follows:—"Near the edge of the solar disc, and especially about the spots approaching the edge, it is quite easy, even with a small telescope, to discern certain very bright streaks of a diversified form, quite distinct in outline, and either entirely separate or coalescing in various ways into ridges and network. These appearances, which have been termed "faculæ," are the most brilliant parts of the sun. Where, near the limb, the spots become invisible, undulated shining ridges still indicate their place—being more remarkable thereabout than elsewhere on the limb, though everywhere traceable in good observing weather. Faculæ appear of all magnitudes; and Professor Phillips, whose description we are quoting, has observed them from barely discernible, softly gleaming, narrow tracts 1000 miles long, to continuous, complicated, heapy ridges 40,000 miles and more in length, and 1000 to 4000 miles broad. By the frequent meeting of the bright ridges, spaces of the sun's surface are included of various magnitudes and forms, somewhat corresponding to the areas and forms of the irregular spots with penumbræ. They are never regularly arched, and never formed in straight bands, but always devious and minutely undulated, like clouds in the evening sky, or irregular ranges of snowy mountains.

"Ridges of this kind often surround a spot, and hence appear the more conspicuous; but sometimes there appears a very broad white platform round the spot, and from this the white crumpled ridges pass in various directions. Towards the limb the ridges appear parallel to it; away from it, this character is exchanged for intermediate direction and lessened distinctness; over the remainder of the surface they are much less conspicuous, but can certainly be traced."*

Sir J. F. W. Herschel describes the *faculæ* "as strongly marked, curved, or branching streaks," among which, "if not

* *The Heavens*, by A. Guillemin. Edited by J. Norman Lockyer. pp. 38, 39.

already existing, spots frequently break out. They may, perhaps, be regarded with most probability as ridges of immense waves in the luminous regions of the sun's atmosphere, indicative of violent agitation in their neighbourhood."*

159. The above are minutely accurate descriptions of what must of necessity occur if my explanations of the constitution of the solar atmosphere are correct. The "mottling," "rice grains," "granulations," "pores," etc., etc., are not sufficient alone; they only give account of the flame-tongues, ripples, and "short seas" of the photospheric ocean; the long "ground-swell" that should exist on such an ocean require to be represented; and they are represented perfectly by these descriptions of the *faculæ*, concerning which there is no controversy; the great magnitude and comparatively orderly nature of the phenomena presenting no serious difficulties either of observation or description. That such a ground-swell should be brighter than the rest of the solar surface follows of necessity from its greater elevation diminishing the depth of absorption-vapours through which it is seen. If, in addition to this, the flaming photosphere be thicker here than in the lower portions, we shall have the additional brilliancy due to the property of flame I have described.

160. Should the ridge of such ground-swell waves be thicker in photospheric matter than the hollows between them? I have assumed as a matter of course that such is the case with the sharp waves and the flame-tongues; but such an assumption must not be too hastily accepted with respect to these greater rounded waves. The flame-tongues must be merely superficial prolongations of the flame-ocean just such as we have all seen darting up from the flaming surface of the spirit when we knew nothing about hydrogen, and played at snap-dragon. The agitation producing "heapy ridges 40,000 miles and more in length and 1000 to 4000 miles broad," must be deeply seated,

* *Treatise on Astronomy*, art. 331.

must reach below the film of flame forming the photosphere, and upheave the lower ocean of dissociated gases, which must thus be similarly waved. In other words, the surfaces of both oceans must be curved, and the photosphere must rest conformably upon the curvature of the non-burning gases.

161. But will their parallelism be perfect? I think not, and will endeavour to state why. I have shown that from the known laws of dissociation and combustion, the amount of combustion producing the photosphere must be limited by the maximum possibility of radiation, and that this radiation is limited by the obstructing envelope of combustion-products forming the next atmospheric layer outside the photosphere. The absorbing envelope is of course distributed in accordance with the law of atmospheric densities. Thus its density and powers of limiting the radiation will be greatest at the bottom of the deepest valleys of the photosphere, and the least on the summits of its highest mountains. Now these ground-swell billows, to which I attribute the faculæ, are huge mountain chains, and the hollows between them are profound valleys; the difference of barometric pressure between the summits of these mountains and the bottoms of the valleys must be very great. Our Mont Blanc is but a pimple compared with these, and yet at the summit of Mont Blanc the density of the atmosphere is but little more than half of that on the plains of Lombardy below. Thus the radiation from the summit of the faculæ must be subject to less obstruction than that from the lower levels of the photosphere, and therefore the combustion of the dissociated gases will there be capable of penetrating to a greater depth, or, in other words, the photosphere will be the thickest and consequently the most luminous on the ridges of these great waves. This, combined with the diminished absorption of the thinner envelope, fully explains the recorded appearances of the faculæ.

162. In a "Notice of a Zone of Spots on the Sun," published in *The Proceedings of the Royal Society*, March 2, 1866, Professor Phillips makes the following summary of his observa-

tions of the gradations of solar splendour; "Looking directly on the central regions, the light is found to be much the brightest on the apparent summits of the undulations of the photosphere ('rice grains'); and looking to the limb, it is the faculæ which are the brightest parts. *In each case it is the outermost parts of the photosphere which are the brightest, and the innermost parts which are the darkest. The depth of shade appears to be in direct proportion to the depth below the outer surface of the photosphere.*" That portion of the above which I have put in italics needs no comment, as its bearing upon my explanations is so direct and obvious. That when "looking directly on the central regions," the light is found to be the brightest on the summits of the flame-tongues and ripples, and that such minor variegation should not appear when "looking to the limb," is precisely what should be seen if I am right; for it is only in the "central regions" that we are looking down the summits of these flame projections, and thus seeing them projected as thickenings of the photosphere. When "looking to the limb," they must be seen in profile, with a thickness due only to their width, which in these flame-tongues and short waves, or dashing ripples of flame, must be but small; while the great ground-swell waves of the faculæ, seen in profile on the limb, should there by their great breadth present the greatest depth of flame, as measured by a plumb line coincident with the visual ray.

CHAPTER XI.

THE SOLAR PROMINENCES.

MECHANICAL AGITATION NOT SUFFICIENT TO ACCOUNT FOR THE SOLAR PROMINENCES—THE DOWNRUSH INTO THE SPOT CAVITIES MUST PRODUCE A CORRESPONDING UPRUSH IN OTHER PLACES—CONSEQUENCES OF THIS UPRUSH OF DISSOCIATED GASES—EJECTION OF METALLIC VAPOURS INTO AND BEYOND THE PHOTOSPHERE—CONSEQUENCES OF THE EJECTION OF METALLIC VAPOURS—FORMATION OF METALLIC HAILSTONES—ORDER OF PRECIPITATION OF EJECTED METALS—SPECTROSCOPIC EVIDENCE OF EJECTED METALS—THE BRILLIANCY OF THE SOLAR PROMINENCES COMPARED WITH THAT OF THE PHOTOSPHERE—VARYING MEAN LUMINOSITY OF THE PHOTOSPHERE—EXPLANATION OF THE COMPARATIVE DINGINESS OF THE PROMINENCES—THE COLOUR OF THE PROMINENCES.

163. No theory of the constitution of the sun can be sound which does not satisfactorily account for these marvellous eruptions, the solar prominences. Mere mechanical agitation, however violent, does not afford anything like an adequate explanation of such isolated columnar masses of flame. Waves and vortices, and the ordinary inequalities of the surface of the photosphere, may be thus explained, but something more is required to account for the violent outbursts which rend the photosphere, and eject it upwards in pillars of flame reaching to an elevation of 100,000 miles. Do my hypotheses assist us here? I think they do.

164. In the course of Chapter VI. I have stated hypothetically that the matter of the photosphere and the vapours of the chromosphere must be carried down the spot vortices into the lower and hotter regions of dissociation; but I have cut

out most of the arguments by which I sought to support this, as they have since been superseded by the direct and beautiful researches of Mr. Lockyer, who has not only proved the existence of such a downrush, but has actually measured its velocity by the altered refrangibility of the thickened hydrogen line. Now such a downrush cannot occur in one place without a corresponding uprush in another. If the uprush were that of an ordinary fluid, it would merely produce a rounded mound, like the seething uprush of waters that is seen beyond the foot of a cascade. But what is the nature of the uprushing material in this case? It must be an enormous volley—millions of cubic miles of a furiously explosive mixture, consisting chiefly of the elements of water. This will be poured through the stratum of flame of the photosphere into the cooler regions beyond, and raised above the denser portion of the vaporous jacket that limits the normal radiation.

165. What must be the consequence of this? It must be a *continuous* explosion, of such mighty force and magnitude that beggars the imagination in its efforts to picture its action and results. We know the deafening report which accompanies the explosion of a soap bubble when filled with these gases, accurately mixed in the proportions to form water; what would be the crash if the cupola of St. Paul's were filled in like manner and exploded? How far would the ball and cross and the masonry of its vault be projected? What if

> "The cloud-capped towers, the gorgeous palaces,
> The solemn temples, the great globe itself,
> Yea, all which it inherit" were
> "Melted into air, into thin air,"

of such explosive quality that the report from firing a bubbleful leaves a deaf humming in the ears, and these were all blasted in one hideous crash? What if a hundred of such worlds, all charged with the material of this horrid ruin, were fired successively in one long bellowing train, combining their propulsive efforts like the contiguous grains of powder in a gun!

What would be the limits of the projectile power of such a combination?

These are the startling questions which now flash up before our mental vision like monstrous phantoms that overawe the imagination, and confound the intellect in our efforts to grasp their magnitude. But yet the solar explosions must commonly occur on this stupendous scale, for the dissociated gases must be upheaved from the bowels of the sun, in exchange for the downrushing contents of the great spot cavities. The quantity thus upheaved *must* be proportionate to the contents of the spot cavities, and this *must* explode when it has dashed through the flames of the photosphere, and beyond the vaporous envelope which bound its energies in previous restraint. As an example of the quantity of explosive material that may thus be ejected, I may refer to a great spot observed by Captain Davis on the 30th August, 1839, measuring 186,000 miles in its greatest length, and having a *surface* area of twenty-five thousand millions of square miles. Worlds of the size of ours might be poured by hundreds into such a cavity, like peas in a basin.

166. I have hitherto spoken only of the elements of water concerned in this cosmic cannonade. These gases, however, are by no means all. The spectroscope has revealed the vapours of nickel, cobalt, copper, iron, chromium, manganese, titanium, zinc, cadmium, aluminium, magnesium, barium, strontium, calcium, and sodium, all of which are combustible, and capable of burning on the outer stratum of the photosphere by combination with the free atmospheric oxygen. With a few exceptions, such as zinc, cadmium, and sodium, the products of combustion of these metals would become solid particles shortly after they emerged from the photosphere, and a considerable proportion of the iron, nickel, cobalt, and copper, if rapidly projected through the photosphere, as they must be by these upheavals and explosions, might pass through this fiery ordeal without complete oxidation.

167. What will be the result of this sudden belching forth of metallic vapours from the fierce heat below, which kept them purely gaseous, into the regions beyond the photosphere where the temperature is below their melting points?

It is obvious that under these circumstances there must occur, according to the temperature, and to the boiling and melting points of the different gaseous metals thus projected outwards, a series of precipitations analogous to those which occur to the gaseous water of our own atmosphere. There must be formed vesicular vapour, rain, snow and hail, resulting from the different degrees and circumstances of such condensation. The philosophy of terrestrial hail-storms is still very obscure; we know the circumstances most favourable to the formation of hail-stones, but we know very little of *how* they are formed. We know that sudden and violent atmospheric disturbance, accompanied with fierce electrical discharges, are the most prominent conditions of their formation. Now these conditions must exist on a hugely exaggerated scale in the solar outbursts which bring the metallic vapours into the region of condensation; and therefore I think we are justified in supposing that while a large proportion of the metallic vapours may be precipitated in a form analogous to snow, another and more important proportion will, in the course of their condensation, be aggregated into concretions analogous to our hail-stones.

168. Under circumstances strictly analogous, the vapours of metals would have a greater tendency to form such concretions than would the vapour of water, on account of the much greater suddenness with which the solidification of the metallic vapours must occur. The vapour of water gives out a very large amount of restored (or latent) heat in passing from the gaseous to the liquid state, then another large amount in solidifying, thus affording time for the development of the crystalline structure of snow flakes. The cooling of a metallic vapour with its low specific heat and little converted heat,

would merely effect a sudden collapse into the liquid, and thence to the solid form, just such as would produce metallic hail rather than metallic snow.

169. A glance at the list of metals existing in the sun is suggestive of many curious speculations in connection with this part of the subject. When the great volley of their vapours are poured forth into outer space having a gradually diminishing temperature, a natural selective order of condensation must of necessity occur. Those with the highest melting points, such as iron, titanium, chromium, manganese, nickel, cobalt, and aluminium, would be the first to precipitate. These would probably be partly in the metallic state and partially oxidized. I say only *partially* oxidized, because although these vapours are all readily combustible in the presence of free oxygen, they would under the circumstances supposed be opposed in their struggle for oxygen by the hydrogen, the sodium, and the other metals that would seize it with so much greater avidity.

170. So long as these metals, or their oxides, retain the gaseous condition, and are not subject to any very excessive pressure, they may be detected either by their bright bands or their absorption lines in the spectrum, but immediately they assume the form of discrete solid or liquid particles, the spectroscopist loses the clue to their separate identity, for then they produce a continuous spectrum, whether they emit the light of their own incandescence, or that which they may borrow and reflect. My explanations of the origin of the solar energies require that we should find the bulk of the solar prominences composed of hydrogen, partly heated to incandescence previous to flaming, partly flaming, and partly united to oxygen and existing as incandescent aqueous vapour; that sodium vapour should accompany it in a similar state of incandescence, and that there should probably be also found at the lower portions of the prominence some vapours of the other more volatile metals. I say *probably* for these, because the step between

their volatility and that of sodium is a very wide one, and so also is that of their spectroscopic demonstrability. This latter property must not be overlooked in reference to this and similar questions. The non-existence of the bands of particular substances in the spectroscope does not necessarily prove their absence. We find sodium almost everywhere, not because it is more ubiquitous than several other substances, but because the brilliancy of its spectroscopic indications make it more readily evident.

171. Before leaving this part of the subject I may say a few words on the brilliancy and colour of the prominences. If these are, as now generally admitted, portions of the photosphere ejected into the chromosphere, why should they be so much less brilliant than the photospheric matter of the limb from which they are ejected? Why should they be invisible except when the sun is darkened by the veiling moon, or when the solar light is hidden or diluted by artificial devices? I am not aware that any attempt has hitherto been made to explain this, but its explanation follows as a matter of course, if my views of the constitution of the photosphere be accepted. When we look at the limb of the sun, the line of vision penetrates an enormously greater thickness of flaming photospheric matter than when we look perpendicularly on the centre of the disc, but at the same time our vision must penetrate a correspondingly increased depth of chromosphere and its obstructing vapours. We know the immense difference between the apparent brilliancy of the sun, when seen at its summer midday height, and when viewed on the horizon across the vapours, etc., of the lower regions of our terrestrial atmosphere. If a few miles of our atmosphere, with its small proportion of aqueous vapour and dust-haze, can make all this difference, what must be the obstructive effect of all that enormous depth of aqueous vapour and metal smoke through which the rays from the edge of the solar limb must pass in glancing horizontally across his rotundity on their way to us? If no

compensating action such as the increased depth of flame existed, the sun would merely present to us a small dazzling disc, with gradually fading brilliancy from its centre outwards; this would be surrounded by a waning nebulosity, darkening away to gradual extinction. We should probably see nothing of the outer edge of the photosphere, but in its place only the vapours illuminated by the absorption of its radiations.

172. A diminution of mean luminosity proceeding from the centre of the sun towards his limb has been observed, and its extreme limits are estimated by Sir W. Herschel as 1000 to 469. But if the difference between a perpendicular and oblique passage of rays through the little bit of atmosphere between us and our visible horizon, can make all the difference between the blinding dazzle of the tropical midday sun and the dull copper-coloured orb seen on the tropical horizon, the obscuration due to a corresponding difference of obstruction of the radiations through the sun's own atmosphere must far exceed the ratio of 1000 to 469. The increased luminosity due to the accumulated concurrent radiations from a proportionally greater depth of photosphere are required to explain this comparatively small degree of difference.

173. The prominences rising from the limb of the sun only radiate from the thickness of their diameter, probably much less than this, as their explosion is not instantaneous but evidently progressive, as should be theoretically anticipated from its necessary concurrence with the rate of radiation. They must be to some extent hollow flames, or mixtures of flame and incandescent gas and vapours. This theoretical view of their constitution is confirmed by the spectroscope. They will thus present a very much thinner or shallower body of flame, or photospheric matter, than the bending edge of the photosphere which forms the limb itself; and their luminosity should be proportionably less,—quite as much less as it is actually found to be.

174. The colour of the prominences is thus described by

Mr. Pope Hennessy:*—"It seemed to be a tower of rose-coloured clouds. The colour was most beautiful,—more beautiful than any rose-colour I ever saw; indeed, I know of no natural object or colour to which it can be, with justice, compared. Though one has to describe it as rose-coloured, yet in truth it was very different from any colour or tint I ever saw before." This colour may be partly a result of absorption, as the vapour of water absorbs chiefly the blue rays, and much of it may be due to the actual colour of incandescent solid particles constituting the metallic snow flakes and hailstones, whose formation I have already described. The further evidence of the existence of such ejected solids will form the subject of the next chapter.

* "An account of Observations of the Total Eclipse of the Sun, made August 18th, 1868, along the coast of Borneo." By His Excellency, J. Pope Hennessy, Governor of Labuan. *Proceedings of the Royal Society*, vol. xvii., page 85.

CHAPTER XII.

THE CORONA AND ZODIACAL LIGHT.

WHAT BECOMES OF THE SOLID MATTER OF THE PROMINENCES?—MY EXPLANATION OF THE "CORONA"—GUILLEMIN'S DESCRIPTION OF THE CORONA—MR. WARREN DE LA RUE'S DESCRIPTION OF THE CORONA—MR. POPE HENNESSY'S DESCRIPTION OF THE CORONA—THESE DESCRIPTIONS CONTRADICT ATMOSPHERIC HYPOTHESES—THESE DESCRIPTIONS SUPPORT MY HYPOTHESIS—THE SPECTRUM OF THE CORONA—ELECTRICAL DISCHARGES ANALOGOUS TO THOSE OF THE AURORA BOREALIS MUST OCCUR IN THE CORONA—THE LIMITS OF THE CORONA—THE ORIGIN OF THE ZODIACAL LIGHT—SIR JOHN HERSCHEL'S DESCRIPTION OF THE ZODIACAL LIGHT—THE FORM AND POSITION OF THE ZODIACAL LIGHT CORRESPONDS WITH MY EXPLANATION—VARIATIONS OF THE ZODIACAL LIGHT—THE DENSITY AND OPACITY OF THE ZODIACAL LIGHT—THE COLOUR OF THE ZODIACAL LIGHT—THE SCINTILLATIONS OF THE ZODIACAL LIGHT—PERIODICAL FLUCTUATIONS OF THE ZODIACAL LIGHT.

175. In the last chapter I have stated my reasons for concluding that the solar prominences contain not only ejections of gaseous matter, but, in addition to these, abundant vollies of solid metallic matter, both in the reguline and oxidized state. This conclusion opens another very interesting question, viz., What must become of the solid particles thus involved in these furious explosions? To answer this question we must again consider the magnitude of the forces at work. Mr. Lockyer says, "In one instance I saw a prominence 27,000 miles high change enormously in the space of ten minutes; and lately I have seen prominences much higher born and die in an hour."*

* *Macmillan's Magazine*, August, 1869.

The prominence observed during the total eclipse of 1842, named the "Turkish cimeter" by Mr. Dawes, and "the boomerang" by the Astronomer Royal, was 70,000 miles high supposing that it originated precisely at the extreme edge of the sun's disc. If it started from any other part it must have been much higher.

176. It is evident that the projectile force that can eject *gaseous* matter through a resisting medium to a distance of 70,000 miles must be capable of throwing the *solid* particles to a vastly greater distance; and these protuberances, of greater or less magnitude, being ordinary solar phenomena, the sun must be continually throwing out particles of metals and metallic oxides in all directions, if my theory of his constitution be sound. Have we any evidence of the outpouring streams of such material, and of the raining back of those portions which solar gravitation has reclaimed? I think I may safely answer this question in the affirmative, and assert that the "*corona,*" which is visible during the few minutes of total eclipse of the sun, consists of the solid particles thus in course of ejection and return. This corona has been attributed to a great extension of the solar atmosphere; but the recent researches of Messrs. Frankland and Lockyer have, I think, satisfactorily shown that the density of the solar atmosphere at, or about, the surface of the photosphere is little, if any, greater than that of our own at the sea level.

177. I have never had the privilege of witnessing a total eclipse, and therefore can only refer to published descriptions and drawings to learn whether the appearances correspond with the explanation I offer. M. Guillemin thus describes the corona. "Some minutes before and after, but especially during the totality, a luminous appearance in the form of a halo surrounds the sun, and throws in every direction *rays of light separated by dark spaces.* In many total eclipses, independently of the regular corona, *other light portions, the rays of which have directions more or less eccentric,* have been remarked *irregularly situated on its contour.* Plate 12 shows in detail the

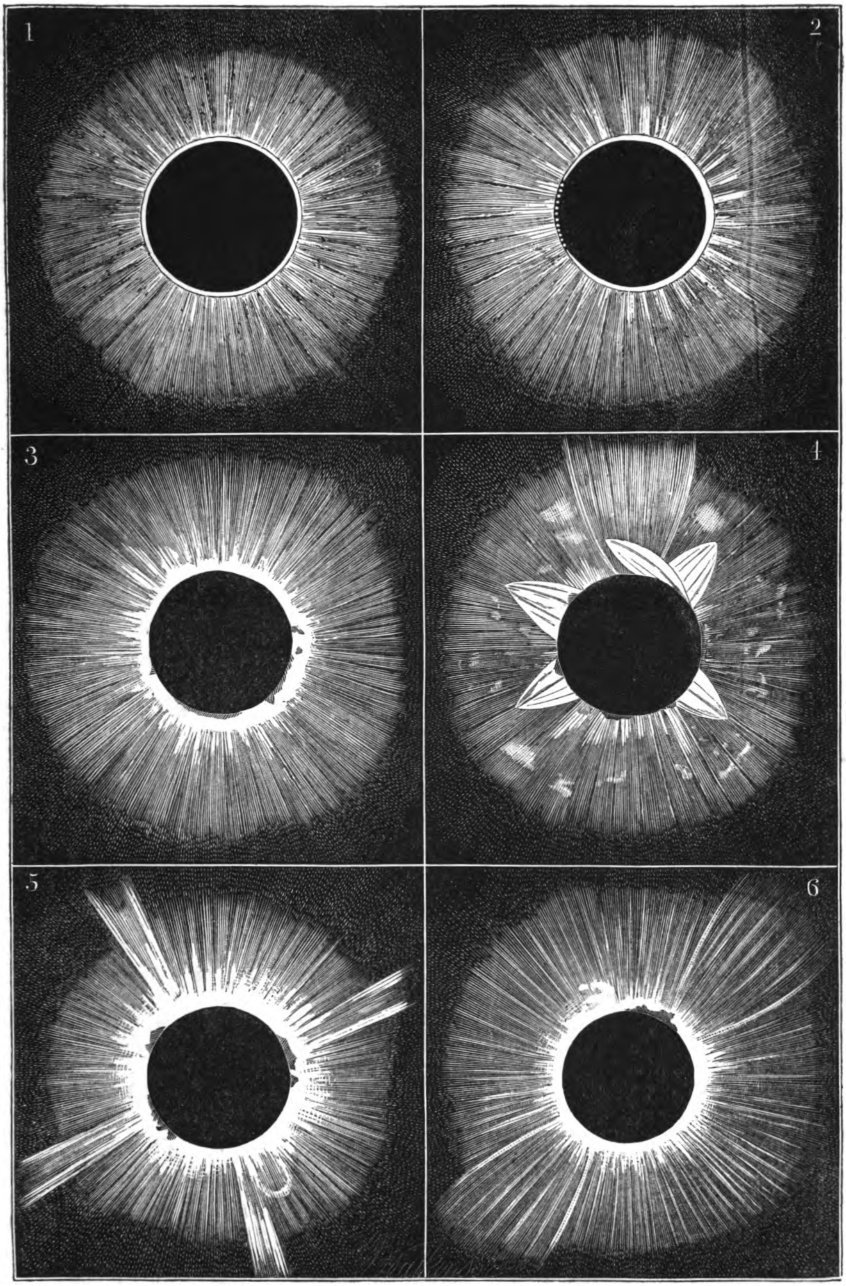
coronas of several total eclipses. The colour of the corona which immediately surrounds the dark disc is sometimes of a pearly or silvery white,—sometimes yellowish, and even red. The explanation generally given of this corona is, that it indicates the existence of a solar atmosphere enveloping the radiant body to an enormous distance."* I have copied the plate which is here referred to, in which fig. 1 represents an "annular eclipse; fig. 2, "Annular Eclipse of the 13th May;" fig. 3, "Total Eclipse of 28th July, 1851 (Dawes);" fig. 4, "Eclipse of 1858 (Lias);" fig. 5, "Total Eclipse of 18th July, 1860, (Feilitzsch);" fig. 6, "Total Eclipse of 8th July, 1842."

178. In the Bakerian Lecture "On the Total Solar Eclipse of July, 1860, observed at Rivabellosa," Mr. Warren de la Rue says, "No attempt was made to obtain accurate observations of the corona, but nevertheless a few seconds were devoted to this phenomenon. Even several minutes before totality the whole contour of the moon could be distinctly seen; when totality had commenced, the moon's disc appeared of a deep brown in the centre of the corona, which was extremely bright near the moon's limb and appeared of a silvery white, softening off *with a very irregular outline and sending forth some long streams.* It extended generally to about from 0·7 to 0·8 of the moon's diameter beyond the periphery."† "*The long streams*" observed by Mr. De la Rue are apparently the same as are depicted by M. Feilitzsch in his representation of this same eclipse. Figure 5.

179. In "An Account of Observations of the Total Eclipse of the Sun, made August 18, 1868, along the coast of Borneo." By His Excellency J. Pope Hennessy, Governor of Labuan, published in the *Proceedings of the Royal Society*, vol. 17, p. 85, the following description is given of the corona. "Suddenly there burst forth a luminous ring round the moon. This

* *The Heavens*, by Guillemin. Edited by Lockyer, pp. 176, 177.

† *Proceedings of the Royal Society*, vol. xii., p. 61.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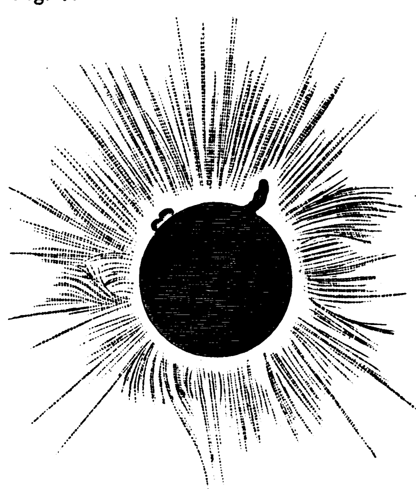
ring was composed of a *multitude of rays quite irregular in length and in direction;* from the upper and lower parts they extended in *bands* to a distance of more than twice the diameter of the sun. *Other bands appeared to fall towards one side;* but there was no regularity, *for bands near them fell away apparently* towards the other side. When I called attention to this, Lieut. Ray said, 'Yes, I see them; they *are like horses' tails;*' and they certainly resembled *masses of luminous hair in complete disorder.*" Figure 7 is a copy of the sketch which accompanies this paper. The "horses' tails" are plainly shown.

Fig. 7.



180. The italics of the above quotations are my own. I have marked them to indicate those features which are utterly inexplicable upon the hypothesis which ascribes the corona to an extension of the solar atmosphere, or "to vapours of excessive tenuity, existing at an immense height in our own atmosphere."* Either of these explanations absolutely demand a symmetrical, homogeneously diffused and regularly graduated halo of light, and are flatly contradicted by "rays of light,

* *Familiar Lectures*, by Sir John Herschel, p. 87.

separated by dark spaces;" or "other light portions, the rays of which are more or less eccentric and irregularly situated on its contour;" or the "irregular outline and sending off some long streams;" or the "multitude of rays, quite irregular in length and direction;" or those "other bands" which "appeared to fall towards one side;" or the resemblance to "horses' tails," or "masses of luminous hair in complete disorder."

181. On the other hand, these extraordinary irregularities stand forth most strikingly in support of my hypothesis. The irregular distribution of the prominences demand irregularity in the volleys they pour forth, and each of the *bands* or "horses' tails" is exactly what I require to represent a charge of solid fragments projected by the bursting of one of these mighty shrapnels. The eccentricity, the irregular length and direction, and the appearing to fall on one side, is exactly what must occur when such prominences as the "boomerang," the "goat's horn," the "dog's tooth," etc., shoot forth their vollies in a tangent to their curved contour. The long radial streams extending beyond the rest, and of excessive brilliancy, are obviously explained as the stream of missiles from prominences of extraordinary magnitude and projectile power. The cloudy patches on fig. 4 are also easily explained as agglomerations of missiles projected by the short and sudden bursting of the upheaval from a symmetrical spot of great depth but small area. The upheaval from such a cavity would approach to the form of a spherical mound or huge bubble bursting violently and collapsing rapidly; while the large irregular spots with several umbræ would upheave a succession of such mounds, resulting in a protracted rocket-like explosion, projecting a long stream of missiles such as are most strikingly shown in figures 5 and 6.

182. The results of spectroscopic examinations of the corona also afford the strongest possible confirmations of my hypotheses. Such a stream of incandescent *solids* should ex-

hibit a continuous spectrum, the feebleness of their light should of course produce but a feeble spectrum of this kind. The spectrum of the corona is described as "a feeble continuous spectrum." This was all the information the spectrum afforded until recently, but the last total solar eclipse observed in America in 1869, has revealed an additional spectroscopic fact which, according to Mr. Lockyer, "is *bizarre* and puzzling to the last degree."* This puzzling revelation is, that *besides* the feeble continuous spectrum, there are three bright lines coinciding with those which have been previously observed as peculiar to our aurora borealis. This fact, so far from being *bizarre* or puzzling to me, is precisely what I wanted to satisfy the imperative demands of my explanation of the corona.

183. We know that when steam issues with great rapidity through an orifice of either metal or wood, as in the Armstrong's hydro-electric machine, or from an ordinary high-pressure boiler, the friction between the steam and the solid produces strong electrical excitation and consequent electrical discharges; and that the presence of potash or soda mingled with the steam greatly increases the amount of electrical excitation. The old lecture-table experiment of blowing air from a bellows upon the cap of the gold-leaf electrometer, illustrates the same kind of excitation. Now the furious rushing of these metallic particles through the air and aqueous vapour intermingled with soda, etc., such as must surround the sun if my views are correct, must produce electrical discharges; and the great rapidity of projection must compensate for the rarity of the medium through which the projectiles are flying. If then the aurora borealis be, as may now be considered demonstrated, an electrical discharge taking place in the upper and highly rarefied region of the earth's atmosphere, its spectrum, whatever that may be, ought to be repeated by the corona. The spectra of electrical discharges are known to vary with

* *Nature*, vol. i., p. 15.

the media in which they take place. Now the aurora occurs at an elevation where the special atmospheric accumulations of the earth are thinning towards emergence into the general medium of space, and the corona occupies a corresponding relative position to the atmosphere of the sun and the same universal medium.

184. It is quite obvious, both from these figures and the recorded descriptions of the corona, that its limits do not represent the end of the journey of the bulk of these missiles. In fig. 4 there are five curves which may represent vollies of oblique projectiles that have completed their utmost excursion from the sun within the limits of the corona, and are returning in something like the ordinary parabolic curve of terrestrial projectiles; but the other radial straight lines, if they represent the path of bodies ejected from the sun, tell us very plainly that they only indicate the start for a much longer journey; that we must look much further for the curvilinear path that indicates the commencement of return. The limits of the visible corona are due to radial diffusion and loss of luminosity.

185. The zodiacal light presents exactly the phenomena required to satisfy these theoretical requirements. Here is a lenticular zone of nebulous matter, having just the form and position which a dense cluster of solid particles projected outwards from the spot-regions on either side of the solar equator should attain if those particles continued their journey far beyond the visible limits of the corona, and then, at varying distances, terminated their radial excursion in the curvilinear resultant of the two forces of explosive projection and solar gravitation.

186. It is thus described by Sir John Herschel; "The zodiacal light, as its name imports, invariably appears at the zodiac, or, to speak more precisely, *in the plane of the sun's equator* which is 7° inclined to the zodiac, and which plane, seen from the sun, intersects the ecliptic in longitude 78°

and 258°, or so much in advance of the equinoctial points. In consequence it is seen to the best advantage at, or a little after, the equinoxes, after sunset in the spring, and before sunrise at the autumnal equinox, not only because the direction of its apparent axis lies at those times more nearly perpendicular to the horizon, but also because at those epochs we are approaching the situation when it is seen most completely in section. At the vernal equinox the appearance of the zodiacal light is that of a pretty broad pyramidal, or rather *lenticular*, body of light which begins to be visible as soon as the twilight decays. It is very bright at its lower or broader part near the horizon, and (if there be broken clouds about) often appears like the glow of a distant conflagration, or of the rising moon, only less red. At higher altitudes its light fades gradually, and is seldom traceable much beyond the Pleiades, which it usually, however, attains, and involves; and its axis at the vernal equinox is always inclined (to the northward of the equator) at an angle of between 60° and 70° to the horizon; where also it is broadest, occupying in fact an angular breadth of somewhere about 10° or 12° in ordinary clear weather."

187. This pyramidal or lenticular form, with its axis coinciding with the solar equator, is exactly the form and position which should be assumed by the aggregation of missiles, projected mainly from the two spot zones, situated about equidistant on either side of the solar equator, and nearly parallel to it. There are, however, some small discrepancies between the position assigned to the zodiacal light by different observers. M. Guillemin says, "It is to be remarked that the direction of the axis of the cone, or of the pyramid, prolonged below the horizon, *always passes through the sun.* It was believed at first that this direction precisely coincided with the solar equator, but it seems more certain that it coincides with the plane of the earth's orbit, or the ecliptic." In a note is added that, "The recent observations of Mr. Heis, of Munster, and Mr. Jones, at Japan, made simultaneously, show, however, the axis

of the luminous cone as forming an angle with the latter plane.*

188. These small differences between observations made at different times, and coincidences between observations made simultaneously, suggest an explanation similar to that of the old story of the chameleon; for it is certain that the zodiacal light varies very greatly from time to time in dimensions and brilliancy, and if my explanation of its origin is correct, the direction of its axis must vary somewhat with the changes in the general positions of the spots and the consequent variations of the resultant of their projectile forces.

189. As the outer boundary of the zodiacal light should represent the outer limits of *ordinary* abundant projection, we may expect that somewhere within this distance, *i. e.*, nearer to the sun, we should find something like the *mean* distance of ordinary projection. There, of course, the projectiles will be the most abundant and the thickest. In strict accordance with this, it is found that proceeding downwards towards the horizon the zodiacal light becomes not only brighter and *broader*, but obviously *more dense*, for Mairan, "who occupied himself with this phenomenon in the days most favourable for observation," found that "It was only towards the apex that he could discern the small stars in the region on which the light was projected."† This indicates a degree of opacity just such as should be exhibited by such a group of particles, where they are most closely packed, and differs very materially from the transparency of cometary matter to which the zodiacal light has been compared.

190. It is also an interesting fact that so many observers, such as Mairan, Derham, Arago, Chaçornac, etc., describe the "yellowish red" colour of the zodiacal light in nearly the same terms as the eclipse observers describe the outer region of the

* Guillemin, *The Heavens*, p. 89.

† *Ibid.*, p. 86.

corona. As this "yellowish red colour" is observed only in the portions of the zodiacal light which are nearest to the sun, and in the portions of the corona which are furthest from it, and as this outer part of the corona seems to be brighter, more yellow, and less red than the inner part of the zodiacal light, while the part of the corona nearest of all to the sun is "pearly white," or "silvery white," it would appear as if we have thus presented a visible manifestation of the gradual cooling of these bodies as they recede from the sun. If so, their luminosity must result from a combination of reflected sunlight and of light directly radiated by their own incandescence. In either case the spectrum of the zodiacal light, like that of the corona, should be feebly continuous. I have not met with any record of a spectroscopic examination of the zodiacal light.

191. "Cassini and Mairan have observed in the luminous cone momentary sparklings, which they explain by the rapid movements of its particles, alternately presenting faces of unequal size; nearly in the same way as one sees the grains of dust sparkling in the rays of the sun, when they penetrate into the interior of a dark room."* Crystals capable of producing such sparklings, and masses of irregular shape presenting varying facets, would probably exist among the particles of metallic oxides, etc., which I suppose to constitute the zodiacal light, but I can hardly conceive that their minute scintillations would be visible at so great a distance. Some such scintillations may, however, be fairly expected as a visible result of the collisions that must of necessity occur between the ascending and decending fragments; especially where some of the larger and denser projectiles, of which I shall presently speak, are shooting onward with great velocities. Some of these collisions should produce flashes of considerable intensity.

192. I shall presently explain that I regard the zodiacal light as mainly consisting of the lighter portions, the smaller

* Guillemin, *The Heavens*, p. 90.

fragments, and the more completely oxidized material of the solar projectiles; those which have been ejected with a great, but not with the maximum projectile power, and which, *for the most part,* are ultimately reclaimed by solar gravitation. According to this view, the total quantity of matter thus suspended about the sun must vary according to the activity of the sun's projectile energies, and this variation should be manifested by corresponding fluctuations in the brightness and magnitude of the zodiacal light, which should present the greatest magnitude and brilliancy at those periods when the sun spots are most numerous, and be the least visible in the opposite periods of solar calm. This expectation is fully verified by actual observation. It has been found that the periods of greatest spot development correspond with those of the greatest development of the zodiacal light. In reference to this I should explain that I regard the outline of this luminosity as rudely representing the resultant of the outlines of the orbits which must be attained by some of the projected bodies; orbits having of course a certain degree of permanency, which, however, would be limited by the resistance of the general medium; this resistance varying inversely with the magnitude of such gravitating fragments. Their orbits should be ellipses of almost every conceivable degree of eccentricity, from a straight line returning upon itself, due to absolutely vertical projection, to a circular orbit, produced by the tangental projection of such curving prominences as the "ram's horn," etc. The outline of the zodiacal light would be formed by the termination or aphelion portion of these excursions, or of such a number of them as by their combination should be sufficient to produce a visible result.

CHAPTER XIII.

THE ORIGIN OF METEORITES.

DO THE SOLAR PROJECTILES PASS THE BOUNDARIES OF THE ZODIACAL LIGHT —THE FORCE OF PROJECTION OF THE SOLAR PROMINENCES—SOLAR PROJECTILES MAY REACH THE EARTH'S ORBIT—SIR JOHN HERSCHEL'S DESCRIPTION OF A GREAT SOLAR ERUPTION—PROJECTILE POWER OF SUCH AN ERUPTION—WHICH OF THE SOLAR CONSTITUENTS WOULD BE PROJECTED THE FARTHEST—THE COMPOSITION OF METEORITES—THE OCCASIONAL CONSTITUENTS OF METEORITES—THE OCCLUDED HYDROGEN OF METEORITES — THE LARGE QUANTITY OF HYDROGEN FOUND IN METEORIC IRON IS CONFIRMATORY OF MY EXPLANATION OF METEORITES —CARBON IN SOME STATES OF COMBUSTION CANNOT BE DETECTED BY THE SPECTROSCOPE—SULPHUR MAY EVADE THE SPECTROSCOPIST—NEGATIVE EVIDENCE OF THE SPECTROSCOPE RESPECTING SILICON IS NOT RELIABLE—CONCLUSIONS OF DR. ROSCOE AND DR. WATTS RESPECTING THE SILICON IN THE BESSEMER FLAME—SILICON IS BURNING IN THE BESSEMER FLAME—THE NEGATIVE EVIDENCE OF THE SPECTROSCOPE DOES NOT DISPROVE THE EXISTENCE OF THE METALLOIDS IN THE SUN—MR. SORBY'S MICROSCOPIC RESEARCHES ON THE STRUCTURE OF METEORITES— THEORETICAL DISTRIBUTION OF METEORITES—THE OBSERVED DISTRIBUTION OF METEORITES.

193. I HAVE already stated reasons for concluding that the limits of the corona are by no means the limits of the projectile energy of the solar outbursts, and that the radial vollies of solid matter continue to the visible limits of the zodiacal light. Is this the outermost range of such projection, or may there yet be some wandering fragments flying out farther still, assuming the dignity of planetary independence, and rolling through space in stable orbits, thus forming a permanent outer fringe to the zodiacal light?

194. Mr. Lockyer tells us that he has watched prominences about 30,000 miles high, whose birth and death have been completed in an hour. This indicates an average velocity of 30,000 miles an hour, but what is the initial velocity? If the prominences which he observed had consisted of solid projectiles, there would be no difficulty in calculating this according to the known laws of acceleration of falling bodies; but here we have the case of gaseous matter projected through other resisting gaseous matter. If a cannon-ball were projected perpendicularly we could calculate its initial velocity if we thus knew the height attained, and the time required to reach it; but if we knew nothing more than the length and duration of the flash of the gun, we could form but a very rough estimate of the distance to which the ball might be thrown. In the case before us the flash of the gun has been seen and measured, and we can make a rough estimate of the charge, but the problem of determining the range of the shot is by no means an easy one.

I have made some attempts to calculate the projectile force of these tremendous outbursts, but am not satisfied with the data upon which any of them are founded, and therefore put them aside at present, as I do believe that there is much truth veiled in the paradox that "there is nothing so deceptive as facts, excepting figures," for if hypotheses are stated as facts, and loose estimates are put into the shape of precise figures, we are apt to form definite ideas without a justifiable basis, and are led to rely upon them as upon exact knowledge, instead of leaning upon them with the caution due to fragile suppositions.

195. At present, therefore, I prefer to speak very generally, and merely to venture an opinion that the magnitude of the charge producing these explosions, if compared with that of our human artillery, justifies us in assuming that the solid metallic hailstones, precipitated under circumstances most favourable for acquiring the highest projectile velocity, may be thrown far beyond the earth's orbit. We must not forget

that the prominences seen only during the momentary glimpses of a total eclipse, or caught in the slit of the spectroscope during the very recent period of such observations, are but the flashes of the ordinary any-day solar artillery; and that we have evidence of occasional outbursts of immensely greater magnitude than these. I refer, of course, to those which shake the needles of our magnetic observatories, and announce their presence by auroral flashes, and by the messages they send along our telegraph wires.

196. Sir John Herschel thus describes one of these, "There occurred on the 1st September, 1859, an appearance on the sun which may be considered an epoch, if not in the sun's history, at least in our knowledge of it. On that day great spots were exhibited; and two observers, far apart and unknown to each other, were viewing them with powerful telescopes; when suddenly, at the same moment of time, both saw a strikingly brilliant luminous appearance, like a cloud of light far brighter than the general surface of the sun, break out in the immediate neighbourhood of one of the spots, and sweep *across* and *beside* it. It occupied about five minutes in its passage, and in that time travelled over a space on the sun's surface which could not be estimated at less than 35,000 miles. A magnetic storm was in progress at the time. From the 28th of August to the 4th of September many indications showed the earth to have been in a perfect convulsion of electro-magnetism. When one of the observers I have mentioned had registered his observation, he bethought himself of sending to Kew, where there are self-registering magnetic instruments always at work, recording by photography at every instant of the twenty-four hours the positions of three magnetic needles differently arranged. On examining the record for that day, it was found that at the very moment of time (as if the influence had arrived with the light) all three had made a strongly marked jerk from their former positions. By degrees accounts began to pour in of great auroras seen on the nights of those days, not only in these

latitudes, but at Rome; in the West Indies; on the tropics within 18° of the Equator (where they hardly ever appear), nay, what is still more striking, in South America and in Australia; where at Melbourne, on the night of the 2nd September, the greatest aurora ever seen there made its appearance. These auroras were accompanied with unusually great electro-magnetic disturbances in every part of the world. In many places the telegraph wires struck work. They had too many private messages of their own to convey. At Washington and Philadelphia, in America, the telegraph signalmen received severe electric shocks. At a station in Norway the telegraphic apparatus was set fire to; and at Boston in North America a flame of fire followed the pen of Bain's electric telegraph, which, as my hearers perhaps know, writes down the message upon chemically prepared paper."*

197. As all these collateral phenomena so vastly surpassed in magnitude those which accompanied the formation of such a prominence as the "boomerang," measuring 70,000 miles in height, we may safely infer that the outbursts which produced them,—one of which was seen as a great flash even upon the dazzling face of the sun itself,—must have produced prominences which, had they been seen in profile like the boomerang, would have been many times greater than this, and must have projected their contents with proportionally greater energy.

198. I have stated that those metallic hailstones which are precipitated under circumstances most favourable for acquiring the highest projectile velocity, would probably pass beyond the orbit of the earth. Now, which are these? Obviously those which would be the first to solidify, and thus obtain the highest initial velocity, and those which would best retain their metallic condition and thus have the greatest density. Which will best fulfil these conditions? Looking over the list of metals in the sun (sect. 166), the answer to this question is easily supplied.

* *Familiar Lectures*, p. 79.

They are iron, nickel, cobalt, chromium, manganese, titanium, and aluminium. Copper comes next, but its melting point is comparatively low. Do hailstones of such composition fall upon the earth as they occasionally should if projected by the sun as far as I suppose?

199. There is no difficulty in answering this question. The chemical composition and mechanical structure of meteorites, afford most direct and decided confirmation of this view of their origin. Their chief constituent is iron, which the spectroscope finds to be so abundant in the solar envelopes. This is not only the most abundant metal, but it is also an invariable constituent of meteorites. The most frequent and characteristic metallic constituents next to iron are just those which have the properties best adapting them to solidify when the highest projectile velocity would be attained, such as nickel, cobalt, chromium, manganese and aluminium.*

200. In concretions formed in the manner supposed, we should expect to find that the iron, and other metals above named, would be partly in the metallic state and partly oxidized. This is exactly the case in aerolites. We should also expect to find as irregular constituents, some of the other solar elements that would of necessity be sometimes caught up and entangled with the chief ingredients while in the act of precipitation and solidification. They are found accordingly, and with just the sort of irregularity that might be expected.

201. The most interesting of these is the occluded hydrogen discovered by Professor Graham as a constituent of meteoric iron. Having proved that iron possesses so remarkably the

* Titanium does not appear to have been detected. My own experience in the analysis of compounds containing large proportions of iron and silicon with small traces of titanium, leads me to conclude that, unless titanium has been specially sought for by a chemist who has had some special experience in its determination, it would, if present in small quantities, most probably be thrown down, partly with the iron and partly with the silicon, and thus be altogether overlooked.

property of condensing within itself a considerable quantity of the gases with which it was surrounded during the period of its solidification, he proposed to learn something of the previous history of meteoric iron, by submitting it to the same examination as that to which he had subjected the other samples of iron prepared in furnaces of human construction. The specimen he examined was a piece of the meteoric iron of Lenarto, which the analysis of Werle had shown to be composed of

Iron . .	90·883
Nickel .	8·450
Cobalt .	0·665
Copper .	0·002

Professor Graham found that when a piece of this iron having a bulk of 5·78 cubic centimetres, was heated to redness in a vacuum, "gas came off rather freely, namely

In 35 minutes	5·38	cubic centimetres.
„ 100 „	9·52	„
„ 20 „	1·63	„
In 2 hours 35 minutes . .	16·53	„

"The first portion of the gas collected had a slight odour, but much less than that of the natural gases occluded by ordinary malleable iron. The gas burned like hydrogen. It did not contain a trace of carbonic acid, nor any hydro-carbon vapour absorbable by fuming sulphuric acid. The second portion of gas collected, consisting of 9·52 cubic centimetres, gave by analysis—

Hydrogen . .	8·26	cubic centim^s^.	=	85·68	per cent.
Carbonic Oxide	0·43	„	„	4·46	„
Nitrogen . .	0·95	„	„	9·86	„
	9·64			100·00	

"The Lenarto iron appears, therefore, to yield 2·85 times its volume of gas, of which 86 per cent. nearly is hydrogen. The proportion of carbonic-oxide is so low as 4½ per cent.

"The gas occluded by iron from a carbonaceous fire is very different; the prevailing gas then being carbonic-oxide. For comparison, a quantity of clean horse-shoe nails was submitted to a similar distillation. The gas collected from 23·5 grammes of metal (3·01 cubic centimetres) was—

In 150	minutes	5·40	cubic centimetres.
„ 120	„	2·58	„
In 4 hours 30	„	7·98	

"The metal has given 2·66 times its volume of gas. The first portion collected appeared to contain of hydrogen 35 per cent., of carbonic oxide 50·3, of carbonic acid 7·7, and of nitrogen 7 per cent. The latter portion collected gave more carbonic oxide (58 per cent.), with less hydrogen (21 per cent.), no carbonic acid, the remainder nitrogen. The predominance of carbonic oxide in its occluded gases appears to attest the telluric origin of iron.

"Hydrogen has been recognized in the spectrum analysis of the light of the fixed stars, by Messrs. Huggins and Miller. The same gas constitutes, according to the wide researches of Father Secchi, the principal elements of a numerous class of stars, of which *a* Lyræ is the type. The iron of Lenarto has no doubt come from *such an atmosphere, in which hydrogen greatly prevailed*. This meteorite may be looked upon as holding imprisoned within it, and bearing to us, the hydrogen of the stars.

"It has been found difficult to impregnate malleable iron with more than an equal volume of hydrogen, under the pressure of our atmosphere. Now the meteoric iron (this Lenarto iron is remarkably pure and malleable) gave up about three times that amount, without being fully exhausted. The inference is that *the meteorite has been extruded from a dense atmosphere of hydrogen gas,* for which we must look beyond the light cometary matter floating about within the limits of our solar system."*

202. I have thus transcribed the greater part of this interesting paper, as it has so important a bearing upon this part of my subject. The italics are my own. After what I have already stated, I think we need not go beyond "the limits of our solar system" to find the "dense atmosphere of hydrogen gas," or the "atmosphere in which hydrogen greatly prevailed," in order to account for the composition and quantity of gas thus occluded. This iron coming, as I suppose, from the great depths of the strata of dissociated water, etc., and having been subject during the period of its solidification to the tremendous pressure of the explosion that hurled it forth, must be expected to contain not only a greater *proportion* of hydrogen gas than ordinary iron, but a larger absolute quantity than it could take up under ordinary pressures. My hypothesis *demands* exactly what Professor Graham has found.

203. Among the substances occasionally found in meteorites are carbon, sulphur, and silicon, neither of which are included in the list of elements that have been found in the sun by means of spectrum analysis. We are, however, by no means justified in concluding therefore that they do not exist there, for these and most of the other non-metallic elements are just the substances concerning which the spectroscope gives very ambiguous information. The carbon spectrum is very variable. The combustion of carbonic oxide gives a continuous spectrum, by which we can identify nothing, as it supplies us with no information as to which of the solar lines are due to the absorption of its combustion products.

Now Deville has shown that when carbonic acid is dissociated by heat it is split up into carbonic oxide and oxygen; therefore if my views are correct, and carbonic acid is a part of the general atmospheric medium, as it is of our atmosphere,

* *Proceedings of the Royal Society*, May 16, 1867.

the carbonic oxide would only contribute to the general continuous spectrum of the sun, and thus its carbon could not be identified by the spectrum. Some compounds of carbon, such as cyanogen and hydro-carbons, do give special carbon lines that may be recognized, but these organic compounds are not likely to be burning in the sun.

204. Sulphur is also very strongly addicted to the production of a continuous spectrum, and like carbon and nitrogen gives varying spectra. Dr. Roscoe thus summarizes its behaviour: "When sulphur burns in the air, or when carbon disulphide burns in nitric oxide, a continuous spectrum is observed. If a little sulphur be introduced into a narrow Geissler's tube, and the air withdrawn, a band spectrum of the first order is seen, upon warming the tube and passing the spark through. On continuing to heat the tube these bands change to bright lines."* As the conditions under which sulphur would probably burn in the sun are quite unlike those of the narrow Geissler's tube, it would, if there, most probably produce a continuous spectrum, and could not be detected by the spectroscope.

205. As regards silicon, which in combination with oxygen and the metals is an abundant and important constituent of some meteoric stones, I need only refer to the following facts to prove that the negative evidence of the spectroscope is, in respect to this element, not at all worthy to be taken into account.

206. Dr. Roscoe and Dr. Watts have devoted a considerable amount of time, and care, and skill, to the examination of the spectrum of the Bessemer flame, and have published the results of their researches; but neither of them have detected any spectrum indications of silicon. Dr. Roscoe's experiments were made at the works of Sir John Brown and Co., Sheffield, and he concludes his statement of the results with the following

* *Spectrum Analysis.* Six Lectures, by Henry E. Roscoe, B.A., Ph.D., F.R.S., p. 133.

remarks. "Those who are practically engaged in working this process would like spectrum analysis to do a great deal more; they would like to be told whether there is any sulphur, phosphorus, or silicon in their steel: questions which unfortunately, at present, spectrum analysis cannot answer, for this very good reason, that these substances do not appear at all as gases in the flame, but that they either remain unvolatilized in the molten metal, or swim on its surface in the slag of the ore; and consequently the lines of these bodies are not seen in the spectrum of the flame."*

207. I have no doubt that Dr. Roscoe is perfectly right in his statement that neither of these elements can be detected by the spectroscope under the conditions existing in the Bessemer flame, but his explanation of the fact is certainly erroneous as regards silicon, and probably so in reference to sulphur and phosphorus. I have analyzed all the brands of pig iron used in the Bessemer converters of Sir John Brown and Co., and find that the average proportion of silicon contained in them is a small fraction below 3 per cent. Assuming that one-third of this is oxidized in the cupola, there remains 2 per cent. in the converter charge. Now as each of the converters on which Dr. Roscoe made his experiments receives a charge of above 5 tons, it must contain 2 cwt. of combustible silicon, and as the whole of this is oxidized during the blow, Dr. Roscoe repeatedly witnessed and examined the combustion of 2 cwt. of silicon in about twenty minutes without detecting any trace of it in the flame.

It is true that a portion of this silicon which is oxidized during the passage of air through the molten metal does combine with the iron, and forms a cinder which floats on the top of the steel, but another and a very considerable portion of the silicon is burning simultaneously with carbon, and contributing its share towards the production of the magnificent flame which

* *Spectrum Analysis*, p. 110.

roars out of the mouth of the converter. The reason why the silicon cannot be there detected is that it contributes to the continuous spectrum, which is really the main feature revealed by the spectroscope when directed to the Bessemer flame. Broadly speaking, the Bessemer flame gives a continuous spectrum.

208. From all I have been able to learn I conclude that, in the present state of our knowledge, the spectroscope does not afford any reliable information respecting the existence or non-existence of the *metalloids* in the sun. They may all be there though the spectroscope should not detect one of them.

209. Mr. Sorby's microscopic investigations of the structure of meteorites afford decided confirmations of my view of their origin. In a paper published in the *Proceedings of the Royal Society*, June 16th, 1864, he infers from their microscopic structure that they must have originated as nearly as possible in the manner I have described in comparing the aggregation of the less volatile matter of the solar ejections to the formation of *hailstones*. I believe that he has subsequently published some further hypotheses respecting their origin, somewhat similar to those above stated, and I propose to avail myself of the privilege of his friendship in order to learn directly from him the particulars of his interesting researches, and to examine some of his microscopic sections of meteorites; but I have not had leisure and opportunity of doing so in time for the publication of this volume, and must therefore postpone this subject, and include it in the supplementary Essay, in which I shall more fully discuss several other subjects collateral to the present work.

210. If these meteoric bodies are thus projected beyond the usual limits of the zodiacal light, they should form a ring or rings outside the limits of that lenticular aggregation, with planes nearly corresponding to the spot-zones on either side of the solar equator. The strictly theoretical result would be the formation of two such principal elliptical rings,—one cor-

responding to an extension of the northern, and the other to the southern zone of spots. Besides these, there should be intermediate and outlying zones of irregularly and more thinly scattered projectiles, from the irregular spots of the equatorial and outlying regions.

211. I need scarcely remind the reader that these theoretical expectations are but an expression of the results of recent observations and generally received conclusions respecting the origin of the August and November star-showers, and the other minor and more or less regular meteoric displays. The August and November showers have been assigned to two principal rings, and the minor phenomena to other less densely packed and less definite rings or zones corresponding to fifty-six radiant points of shooting-stars, which have already been determined.

I think, therefore, that I may conclude that all the knowledge we possess respecting the chemical composition, mechanical structure, and cosmical distribution of meteorites accords as closely as the nature of the subject and the present gaps in our knowledge permits, with the idea that they are some of the denser hailstones precipitated from the upheaved dissociated inner vapours of the sun, and flung outwards into the planetary regions by the furious explosions to which such dissociated gases must of necessity be subject when they are whirled upwards by the eccentric rotation of the solar nucleus.

CHAPTER XIV.

THE ASTEROIDS.

OCCASIONAL SOLAR OUTBURSTS FAR EXCEED THE MAGNITUDE OF THE ORDINARY PERIODICAL MAXIMA—HISTORICAL RECORDS OF DARKENING OF THE SUN—THE CONTINUOUS GRADATION FROM THE CORONA TO THE ASTEROIDS—THE MAGNITUDE OF THE ASTEROIDS PRESENTS NO DIFFICULTY—THE FINAL ORBITS OF THE ASTEROID EJECTIONS—VARIATIONS OF LUMINOSITY OF THE ASTEROIDS—THE HYPOTHESIS OF A SHATTERED PLANET WILL NOT BEAR EXAMINATION—VOLCANOES AND EARTHQUAKES ARE PURELY SUPERFICIAL PHENOMENA—MY HYPOTHESES COMPARED WITH THAT OF A SHATTERED PLANET—THE DANGERS OF DEPARTURE FROM STRICT INDUCTION—MY HYPOTHESES ALL REST DIRECTLY UPON DEMONSTRATED PHYSICAL LAWS.

212. I REGRET exceedingly that I am unable, for the reasons stated in section 194, to calculate the maximum projectile force of the great solar eruptions producing the prominences, as I do not see any sufficient reason for limiting the range of their energies either to the ordinary limits of the zodiacal light (which we are told "sometimes extends beyond the earth's orbit, and sometimes lies within it,")* or to that fringe of meteoric masses beyond which formed the subject of the last chapter. These may be the results of the ordinary and the common periodical maxima of solar eruptions; but such eruptions are not all that have to be taken into account. It is evident from the phenomena observed on 1st September, 1859, (described in sect. 196), that occasional isolated outbursts take place, which enormously exceed the ordinary periodical maxima.

* Guillemin, *The Heavens*, p. 90.

213. But even the extraordinary magnitude of this solar convulsion may not by any means represent the limit of explosive violence which the sun is capable of occasionally exerting. We have fair reason to suppose that, during the countless ages, —the bye-gone millions of millions of years of solar combustion,—there have been occasional extraordinary outbursts still grander than that which was so well observed and recorded on Sept. 1, 1859. This, in spite of its magnitude, would probably have passed unnoticed had it happened but twenty years earlier, when we had no electric telegraphs to disturb,—no magnetic observatories to record photographically such disturbance, and few vigilant solar observers to witness the flash. This, it is true, is the greatest solar commotion that has occurred during the epoch of magnetic observatories and electric telegraphs; but what a microscopic point of time is this compared with the age of the sun! We have historical records of what must have been, if the accounts are at all reliable, vastly greater solar outbursts than even this. "Thus in the annals of the year A.D. 536, the sun is said to have suffered a great diminution of light, which continued fourteen months. From October A.D. 626 to the following June a defalcation of light to the extent of one half is recorded; and in A.D. 1547, during three days the sun is said to have been so darkened that stars were seen in the day-time."* These accounts are very marvellous; but the fact that the dates correspond with the calculated periods of great spot maxima strongly confirms the records which, with some allowance for the historical exaggeration of the period, may be accepted as evidence of the occurrence of spots of sufficient magnitude to produce effects plainly visible without telescopic aid, and far exceeding any of the spot-obscurations that have been observed during the telescopic period of astronomical history.

214. It is but a step from the meteoric zone, crossing the

* Herschel's *Outlines of Astronomy*, art. 394 A.

orbit of the earth to the zone of "pocket planets," beyond the orbit of Mars; a step that for a projectile is practically shortened by the continuous diminution of the reclaiming force of solar gravitation. It is but a like gradation from meteoric dust to meteoric grains, then to the meteoric pebbles, nodules, and masses weighing hundreds of pounds, and even tons, that have fallen upon the earth, to the smallest of the asteroids, and from them to Pallas, the giant of the series, whose bulk is 2177 times less than that of our earth. It is perfectly consistent with the primary principles of the hypothesis I have already ventured, that the larger masses should be projected to the greatest distance; and also that the more tremendous and profound the whirling tempest in the sun, the greater must be the dimensions of solid masses that would be torn out of the bowels of his vaporous depths.

215. According to my description of the structure of the sun there must, in the ocean of dissociated gases, be a lower depth, composed mainly or entirely of the vapours of the heavier metals; and should volumes of this metallic gas, commensurate with the magnitude of an extraordinary spot cavity, be forced upwards, involved in the subsequent explosion, and consolidated at an early stage of its outward flight, there is nothing extravagant in the supposition that this consolidation of a volume of metallic vapour, many times greater than the earth, should produce single masses varying from a few miles in diameter to $\frac{1}{2177}$th of the earth's bulk. I venture to affirm that the *dimensions* of the asteroids do not stand at all in the way of this audacious hypothesis; the primary difficulty which makes me hesitate is that of the projectile velocity. The largest and heaviest masses would, of course, acquire the same initial velocity as the smallest and lightest particles, precipitated in corresponding positions; and these largest and most dense would travel the furthest, as they would encounter the least relative resistance from the gaseous matter through which they must pass at the commencement of their journey; but whether the

utmost possibilities of explosive energy (for such we have to consider) are sufficient to project a body from the surface of the sun to the region between Mars and Jupiter, is a question to which I can only give a suppositious answer in the affirmative.

216. The position and character of the orbits of these bodies accord generally with this hypothesis; the average plane of their orbits is not very far removed from that of the solar equator; the respective planes vary considerably, as might be expected, if they are results of *extraordinary* solar disturbances. The form of their orbits is also very variable, many of them being much longer ellipses than those of the planets. I should have expected a still greater elongation and eccentricity in some of them, and such orbits may have existed; but an asteroid with an orbit of cometary eccentricity that would, at each revolution, cross the paths of Mercury, Venus, the earth, and Mars, in nearly the same plane, and dive through the thickly scattered zodiacal cluster, both in going to the sun and returning from it, would be subject to disturbances which would continue until one of two things occurred. Its tangental force might become so far neutralized, and its orbit so much elongated, that finally its perihelion distance should not exceed the solar radius, when it would finish its course by returning to the sun. On the other hand, its tangental velocity might be increased by heavy pulls from Jupiter, when slowly turning its aphelion path, and be similarly influenced by friendly jerks in crossing the orbits of the inferior planets; and thus its orbit might be widened, until it ceased periodically to cross the path of any of the planets, by establishing itself in an orbit constantly intermediate between any two. Having once settled into such a path, it would remain there with comparative stability and permanency. If I am right in this view of the dynamical history of these older ejections, all the long elliptical paths of zodiacal particles, meteorites, or asteroids, would thus in the course of ages become eliminated, and the remaining orbits would be of planetary rather than cometary proportions.

The recent ejections may have any form of orbit within the boundaries of the conic sections; but as their age must be but a very small fraction of the age of the solar system, they will constitute but a correspondingly small proportion of the outer and greater masses now under consideration. With those lighter particles forming the zodiacal light the case will be different, as the shorter the excursion of the projectile, the more subject must it be to reclamation by a body of such great dimensions as the sun. Hence the variability of the juvenile corona and youthful zodaical light as contrasted with the stability of these middle-aged asteroids.

217. M. Guillemin describing Juno says, "Its colour is reddish, and its light is subject to sudden variations, which are not less remarkable than the rapidity with which they are accomplished. This phenomenon is not peculiar to Juno; it is observed in Vesta, which sometimes becomes very bright, in Ceres, and in many other of the minor planets. Several hypotheses have been suggested to explain this fact. Some suppose that the different faces of these small bodies do not reflect the solar light with the same intensity; that some are formed of crystalline facets, or even have a light of their own. Others believe that the small planets are irregularly formed, presenting to us consequently sometimes very extensive, and at others very limited surfaces. Whichever hypothesis we admit, both take for granted a rotation."* This is exactly the appearance which should be presented by the rotation of irregular shaped subangular masses, similar to the great meteorites that have struck the earth. The "yellow," "reddish yellow," and "reddish" colour of these asteroids is also interesting, as it corresponds to the light which would be reflected from an external coating of oxide of iron.

218. The theory which ascribes the asteroids to the fragments of a broken planet occupying the space which, according

* *The Heavens*, p. 217.

to Bodes' law, constitutes a planetary vacancy, is now becoming generally abandoned, on account of failure of that law in reference to the planets Neptune and Mercury, and the non-fulfilment of theoretical requirements by the orbits of Vesta and some of the more recently discovered asteroids.

There is another and, as it appears to me, far more serious objection, which appears to have passed unnoticed. I maintain that the notion of a *single* shattered planet is nothing more nor less than a physical absurdity,—a supposed effect for which we can assign no cause of even the remotest approximate sufficiency. The inventors and supporters of this "catastrophe" hypothesis seem to base it on the supposed possibility of a planet exploding by the action of forces analogous to those which produce volcanic eruptions and earthquakes. At the first glance there is an aspect of plausibility about this, but it will not bear examination.

219. To such minute creatures as ourselves volcanic and earthquake phenomena, when viewed from terrestrial distances, appear as manifestations of stupendous and awful forces. The earth appears *to us* to be "shaken to its foundations:" but what is a volcano viewed cosmically? The largest craters of this world are but little pimples on the earth's epidermis. The gaping chasms which have horrified mankind, when earth-waves have shattered their frail tenements; the greatest dykes, faults, and dislocations of strata which geological investigation has disclosed, are but cracks in the mere cuticle of our globe. It has been truly said that all our knowledge of actual geological structure is limited to the revelations presented by mere scratchings of the surface of the earth. So with the volcanic forces; they are utterly superficial. There is not a tittle of evidence to show that they are connected with any sort of deeply seated explosive energy.

220. The volcanos of the moon are, proportionally, a vast deal larger than those of the earth; they are not merely pimples, but boils; but then we must remember how these and

all other craters are formed. They are not thrust up at once by a single outburst of expansive energy, but are the growths of ages of gradual contributions of the comparatively molecular fragments that are from time to time ejected, and which either fall or flow upon their sides, and thus gradually build them up. Superficial forces producing mere superficial irregularities are all that volcanic action indicates. To attribute an internal explosive capacity to a solid planetary mass such as should shatter it into fragments, is to depart altogether from the sober paths of inductive philosophy and wander into the dream-land of inventive fiction. I have already admitted that my own is an audacious hypothesis, but its audacity is purely *quantitative*. I have invented no new kind of force, but have merely assumed that a force which must exist, which we see in operation, which must operate exactly in the required direction, and whose magnitude is so gigantic that the human mind is utterly overwhelmed when it endeavours to picture its operation, is sufficiently great to project the sort of material which it must project in that direction, to the distance at which we find it.

221. Bacon has taught us that the business of science is to observe and collate facts, to generalize these, and induce the laws of their causation; and then, *after we have demonstrated these laws*, to use them as the intellectual machinery of deduction by means of which new and unknown facts may be revealed, and obscure phenomena explained. From the moment that philosophers adopted this course science started in a new career of progress; so long as they have adhered to it they have assisted its progress; when they have departed from it they have failed to do so. The grandest truth of all physical science,—the universality of gravitation,—was demonstrated upon the basis of the terrestrial experimentally verified phenomena of falling bodies. Newton *invented* nothing in his theory of gravitation; he merely assumed that a force demonstrated upon the earth extended consistently throughout the universe in accordance with the necessary geometrical laws of radia dif-

fusion. He asked for nothing anywhere throughout the whole immensity of the universe that was not demonstrable here on earth. He adhered strictly to the severest rules of inductive investigation, and the result was the grandest triumph the human intellect has ever achieved. The case was quite different in his theory of light. There *he invented corpuscules* without any direct evidence of their existence; he endowed these corpuscules with a wondrous power of motion, without showing any known force competent to produce such motion; he assumed gratuitously that they had "fits" of easy transmission and reflection, and thus built up a whole fabric of hypothetical fictions upon which the same powers of intellect were exercised even more laboriously than upon his theory of universal gravitation. But how different has been the fate of the corpuscular theory of light from that of the theory of universal gravitation!

222. I have endeavoured to keep the moral of this and other similar examples constantly before me during the course of these speculations, the boldness of which I freely admit, but which I contend are all warranted on the principle of assuming that the whole universe is framed upon one uniform plan;—that all the great physical forces of the universe are operating here upon our little world and around our little selves;—that the laws we discover by studying the operation of these forces, even on the smallest scale, apply to the most gigantic cosmical operations of nature. Without deviating a hair's breadth from the strictest application of these principles, as I am able to understand them, I have reached the conclusion that asteroids *must* be formed by the solar eruptions, and *must* be ejected to vast distances; the only questionable assumption is, that the *quantity* of propulsive force *may* have been occasionally sufficient to carry them to so great a distance as the wide space between the orbits of Mars and Jupiter.

CHAPTER XV.

METEOROLOGY OF THE MOON AND THE INFERIOR PLANETS.

MY PRIMARY HYPOTHESIS OF AN UNIVERSAL ATMOSPHERE SHOULD BE SEVERELY TESTED—THE OBSERVED ATMOSPHERES OF THE PLANETS, ETC., TO BE COMPARED WITH THEIR CALCULATED ATMOSPHERES—THE THEORETICAL ATMOSPHERE OF THE MOON—THE MEAN LEVEL OF THE MOON—INFERENCES DERIVED FROM OCCULTATIONS—SUPPOSED ATMOSPHERE ON THE LOWER LEVELS OF THE MOON—DOUBTS OF ASTRONOMERS RESPECTING A LUNAR ATMOSPHERE—STATE OF THE AQUEOUS MATTER OF THE LUNAR ATMOSPHERE—IS THERE EVIDENCE OF A DEPOSIT OF HOAR-FROST ON THE DARK SIDE OF THE MOON?—THE THEORETICAL ATMOSPHERE OF MERCURY—THE HYDROLOGY OF MERCURY—METEORITES AND POCKET SATELLITES OF MERCURY—OBSERVATION CONFIRMS THEORETICAL ESTIMATE OF THE ATMOSPHERE OF MERCURY—BEER AND MÄDLER'S CONCLUSIONS RESPECTING THE ATMOSPHERE OF MERCURY—THE "NEBULOUS RING" AND "EQUATORIAL ZONE" OF MERCURY—POSSIBLE IDENTITY OF THE NEBULOUS RING AND EQUATORIAL ZONE—THE THEORETICAL ATMOSPHERE OF VENUS—THE HYDROLOGY OF VENUS—THE THEORETICAL AND THE OBSERVED ATMOSPHERE OF VENUS—SCHRÖTER AND GUILLEMIN'S DESCRIPTIONS OF THE ATMOSPHERE OF VENUS—THE MISTY OBSCURATIONS OF THE ATMOSPHERE OF VENUS ARE NOT DIMINISHED BY INCREASE OF TELESCOPIC POWER—THE DISADVANTAGES OF ENGLISH OBSERVERS—A TERRESTRIAL ILLUSTRATION OF ATMOSPHERIC OBSTRUCTIONS TO DISTINCT VISION—THE EFFECT OF SLIGHT ATMOSPHERIC VARIATIONS AND OF THE IMPERFECT TRANSPARENCY OF TELESCOPE LENSES—THE VALUE OF THE OBSERVATIONS OF THE ITALIAN ASTRONOMERS.

223. As every argument of the preceding chapters is built upon the primary assumption of the existence of an universal atmosphere of similar composition to that around our earth, and that it is accumulated around every gravitating body of the universe in a quantity proportionate to its gravitating power; and as the constitution which I have ascribed to the sun, and

all the forces and phenomena I have endeavoured to explain, are merely the necessary consequences of this universal extension of our atmosphere, it is of the utmost importance that the primary assumption should be subject to every possible form of collateral scrutiny. When Torricelli ascribed the rising of the mercury in the vacuum-tube to the weight of the atmosphere, Pascal suggested his celebrated *experimentum crucis* of carrying the tube to places where the atmosphere must, according to the hypothesis, vary in weight;—up the sides of mountains where it must be lighter, and down to the sea-level or below, where it must be heavier; and then, by comparing the height of the mercury with the calculated density, to verify or refute the hypothesis.

224. A strictly analogous "experimentum crucis" for the trial of my fundamental hypothesis is obtainable from the varying masses of the members of our solar system. Near enough to us for detailed scrutiny is one of the smallest members of the system—our own satellite, and within the reach of our telescopes—near enough to enable us to learn something about its atmosphere, is Jupiter, the largest of the attendants on the sun, whose mass is 27,100 times greater than that of the moon. Between these extremes we have six planets presenting variations of mass; and although we cannot apply direct barometrical measurements to determine the actual density of their atmospheres, we can form some approximate idea of their relative actual densities. Their theoretically required densities can be calculated with some degree of accuracy. Now, if the recorded observations of astronomers relative to the quantity of atmospheric matter corresponds, as nearly as the approximate nature of such observations permits, with my calculated atmospheric requirements, such accordance will afford a most important verification of the fundamental hypothesis; for if the quantity of atmosphere belonging to each body were merely an individual speciality, such as it is commonly regarded, the laws of probability would demand some very serious discrepancies among

these eight bodies, including so wide a range of variation of theoretical atmosphere as that between Jupiter and the moon.

I will therefore proceed to calculate their theoretical atmospheres, and compare the results of such calculations with the observations that have been made and recorded on the actual atmosphere of each, commencing with the moon as the nearest body to the earth, and the one respecting which we have the most minute and reliable observations.

225. The mass of the moon is $\frac{1}{80}$ of that of the earth. By the rule already given (sect. 28) this number must be multiplied by its own square root, and the product will express the total atmosphere of the moon:—the earth's total atmosphere being taken as unity. We thus get $\frac{1}{80}\sqrt{\frac{1}{80}}=\frac{1}{715{\cdot}5416}$, or, discarding fractions, the total atmosphere of the moon should be $\frac{1}{715}$ of that of the earth. But the moon being smaller than the earth, this atmosphere will be concentrated upon a smaller surface, and the extent of this concentration will be shown by dividing the number expressing the total atmosphere by the square of the moon's diameter (the earth's diameter being unity). The quotient will express the atmospheric pressure or density at the lunar surface, that upon the surface of the earth being still our unit. Now the diameter of the moon thus expressed is 0·264, the square of which is 0·0697. The pressure will therefore be $\frac{\frac{1}{715}}{0{\cdot}0697}=\frac{1}{49{\cdot}8355}$ or, in round numbers, $\frac{1}{50}$ of that upon the earth's surface. Thus the barometer should stand at six-tenths of an inch instead of 30 inches, and the pressure per square-inch should be three-tenths of a pound, instead of 15 lbs. It would be about equivalent to the vacuum obtainable in the receiver of a common old-fashioned air-pump in ordinary working order.

226. In the determination of the pressure of the atmosphere upon the earth we take our barometers to the sea level, *i. e.*, the lowest portion of the earth's general surface. We must in like manner take our imaginary barometers to the lowest general surface of the moon, to obtain this calculated column

of mercury of six-tenths of an inch. From the excessive irregularity of the lunar surface it would be very difficult to define clearly where this lowest *general* surface is. It must be at the bottom of the average craters and valleys of the lunar surface, probably about the mean level of the *Oceanus Procellarum,* and the other dark regions called the "Maria" by selenographists. It would be as unfair to take the lowest *exceptional* depressions of the lunar surface as the atmospheric base, as to take our terrestrial sea-level from the Caspian. Equally incorrcct would it be to take the summit-level of the lunar mountains, which, in proportion to the general volume of the moon, are so much greater than those of our earth. At the summits of these the rare atmosphere must be further thinned out to such excessive tenuity that the difference between its density and that of the general atmospheric medium would be too small to effect any measurable refraction.

227. All who are acquainted with the results of telescopic observations of the moon will at once perceive how closely this accords with the descriptions of observers. The observations of occultations are generally stated to indicate no atmosphere, though some doubts as to the absolute truth of this have been expressed. Now the edge of the lunar disc, by which the boundary of the moon's diameter is measured, and which determines the beginning and end of the occultation, is formed by the summits of the lunar peaks and ranges, levelled by the effect of perspective, for we look upon them horizontally, as though from their own summit level. The only portion of the lunar atmosphere remaining for the purposes of horizontal refraction, visible from the earth, must be that portion of the 0·6 inch atmosphere which rises above these lofty summits.

It is stated in astronomical treatises that a lunar atmosphere having a density equal to only $\frac{1}{2000}$ of that of our own, would be indicated by the acceleration of the observed period of an occultation. This calculation rests upon the assumption of an atmosphere suddenly and sharply terminating in an absolute vacuum. If instead of this the lunar atmosphere is, as I maintain, only a graduated condensation of the general atmospheric medium, this calculation will require very material modification. I am at present unable to see any data upon which the density of the universal atmosphere can be calculated, though I think it quite possible that it may hereafter be estimated, and suspect that it will be found of greater density in the interplanetary spaces, *i. e.*, within the limits of the solar system, than I would at present venture to state, and that it will there be subject to the same laws of motion as the *solid* planetary matter

228. In reference to the thin atmosphere which, if I am right, we ought to find within the craters, and on the *maria* or plains of the moon, I will quote the following from M. Guillemin's work on *The Heavens*, to which I have been already indebted for so many quotations. He says (p. 155 of Mr. Lockyer's English translation), "Is it possible that there may be an atmosphere confined to the bottom of the lowest plains and the deepest craters? Nothing renders probable or contradicts this hypothesis. But at all events no cloud ever disturbs the purity of its sky; for clouds even of slight dimensions would be easily perceived from the earth, and no convincing observations of any are recorded." To this the Rev. T. W. Webb adds the following note, "After all fair deductions on the score of imperfection of observation or precipitancy of inference, there are still residuary phenomena such as, for instance, the extraordinary profusion of brilliant points which, on rare occasions, diversify the *Mare Crisium*, so difficult of interpretation, that we may judge it wisest to avoid too positive an opinion."

229. I could go on multiplying quotations to show that there exists in the minds of astronomers just that amount of doubting and difference of opinion that should be caused by such an amount of atmosphere as I have calculated, supposing it to be distributed in the manner I have described. There is

far too little to produce anything like the effects that would be looked for according to the analogy of our own atmosphere, and yet just too much to allow them positively to assert unhesitatingly that there is no atmosphere. Regarding as I do with the profoundest veneration those sublime records of divine truth which the highest of the high priests of nature, our great astronomers, have bequeathed to mankind as material for his moral and intellectual elevation, I cling with reverence even to the doubts and hesitations of the purified intellects of these great men, and bring them forth as evidence.

230. It is obvious that such an atmosphere as I have calculated could not produce any cloud phenomena at all comparable with those of our terrestrial atmosphere; yet remembering what I have argued respecting the aqueous element of the general atmosphere, there should be a light and *sudden* condensation, not of water, but of minute ice crystals, or hoar frost when the fierce glare of the sunlight suddenly deserts the moon. May it be this which is sometimes visible in the shady depths of the *Mare Crisium*, which is situated in the best possible position for seeing it, if it be there in the filmy quantity demanded. This depression being close upon the western edge of the moon, we should see such films in profile, and thus they would present a density corresponding to their horizontal width, and should appear as we see the bands of our cirro-stratus clouds when near the horizon, presenting an appearance of density so much greater than when they are spread over our heads.

231. According to this the dark side of the moon should be covered, during its darkness, with a thin film of hoar-frost. This suggests the question, Whether the actual "earth-light" of the moon is greater than it should be theoretically, taking into consideration the quantity of light the earth is capable of reflecting, as compared with that of the sun, and assuming that no change occurs in the reflecting power of the lunar surface. It should be so if the dark side of the moon is thus whitened.

232. I am sorely tempted to discuss some of the recent speculations respecting the structure and form of the moon, such as those of Professor Frankland and Hansen, but must refrain from doing so, for if I had yielded to all the digressive seductions which my subject has presented, this little Essay would have become so enlarged, that with my small modicum of literary leisure its publication must have been almost hopelessly deferred.

233. I will now proceed to the planets. Commencing from the sun, and passing by the hypothetical planet Vulcan, we come to Mercury, whose mass is to that of the earth as 0·0769 to 1, and whose diameter is 0·389 to 1. As $\sqrt{0·0769} = 0·2773$, the total atmosphere of Mercury, omitting minute fractions, should be $0·0769 \times 0·2773 = 0·02132457 =$ about $\frac{1}{47}$, and the atmospheric pressure on the surface of Mercury $\frac{0·02132457}{0·389^2} = 0·1409 =$ about $\frac{1}{7}$. Thus on the plains of Mercury the barometer should stand at about $4\frac{1}{4}$ inches, and the pressure of the atmosphere should be about 2 lb. 2 ozs. per square inch. The boiling point of water under such a pressure would be about 128° Fahr.

This planet should therefore be surrounded by a rare but decidedly appreciable atmosphere. As Mercury receives from the sun about seven times as much heat as the earth obtains, the mean temperature of its equatorial regions should far exceed that at which water boils upon its surface, and the summer temperature even to its polar regions would be above the boiling point. As the inclination of the axis of Mercury must produce such great variations of season, and the distance between these extremes of temperature must be further increased by the great eccentricity of the orbit of the planet, the winter temperature may fall below the boiling point, and condensation may thus occur on those latitudes where the daily rotation brings no sunlight.

234. I must not pass over the theoretical meterology of this planet without alluding to one peculiarity. It must sometimes

be immersed in the matter which forms the zodiacal light, and if my views are correct, it must be more subject than any other planet to the fall of meteorites upon its surface; thus a considerable number of these bodies of all dimensions should from time to time be diverted from their eccentric orbits about the sun, and constrained by the gravitation of this planet to travel around him in orbits of greater or less permanency, forming a series of pocket satellites.

235. Let us now see how far the above stated theoretical requirements are verified by observation. The proximity of Mercury to the sun renders observation of the details of his surface very difficult, and therefore we have but little positive knowledge to appeal to. Dark bands have been observed on his disc by Schröter, but of the nature of these little or nothing is known. No markings which, like those on Mars and Venus, can be assigned to land and water, have been discovered. So far as this negative evidence goes it is confirmatory, but I do not consider it sufficiently reliable to lean upon it with any degree of confidence. The remarkable irregularities of the terminator, and more especially the curious truncation of the Southern Horn, appear to me to afford much stronger confirmation of the absence of any ocean than do the negative evidence derivable from direct examination of its surface. If I understand rightly that the boundary edge between light and shade is *always* more or less rough and jagged, there can be no considerable body of water on the surface of the planet, for the terminator in passing over such a surface should present a *perfectly* sharp unbroken line. It would be so positive and definite that the limits of any considerable ocean or sea might be measured with tolerable accuracy, and its form delineated by the expansion and contraction of its boundaries as the line of the terminator passed across it. Such keen observers as Schröter, Beer, and Mädler, should have detected such definite lines in their eager search for some recurring pecularity by which to measure the rotation of this planet.

236. The existence of some amount of atmosphere is indicated by the twilight shading of the terminator. I am not aware that any estimate of the density of this atmosphere has been attempted, the difficulties of observation and the very small datum afforded by this mere shaded line, being an insufficient basis for numerical calculation. Beer and Mädler, referring to their observations of this line, say "Hence we may conclude that Mercury has a *pretty sensible atmosphere.*" This is as near an approximation of my theoretical atmosphere of 4¼ inches barometric pressure as may be expected under the difficulties of observation which this planet presents.

237. In reference to what I have said about the occasional immersion of Mercury in the nebulous matter of the zodiacal light, and its gathering some of that matter in an orbital arrangement about itself, the following is very interesting. "According to many astronomers, Mercury presented the following aspect in its transit of 1799 across the solar disc. Instead of a black round spot, perfectly clear and well-defined, there was seen all round the disc of the planet a circular band, less luminous than the rest of the surface of the sun, forming a sort of nebulous ring. It was thence inferred that there existed a very high and dense atmosphere. Recent observers have not seen anything like it."* This remarkable observation derives an additional interest from the following. "One diligent observer, Schröter, *at the end of the last century,* was able, however, to observe some dark bands on its disc, which he considered as an equatorial zone;"† and "Schröter, when examining Mercury during its transit over the sun, on the 7th May, 1799, saw, or believed he saw, on the black disc of the planet a luminous point. It has been concluded from this observation, which has not, however, been confirmed, that there exist on the surface of Mercury active volcanoes."‡ These three quotations are from the same work, but they stand at

* Guillemin, *The Heavens*, p. 70. † *Ibid.*, p. 73. ‡ *Ibid.*, p. 74.

some pages apart from each other, and the phenomena are nowise regarded as having any connection with each other.

238. I am sorry to state that I am not within reach of any work from which I can learn the exact position of Mercury on the 7th May, 1799; whether or not it was near to its summer or winter solstice, so as to afford to the observers from the earth the nearest possible approach to a circular presentation of his equatorial zone. If so, the "nebulous ring" may have been the same thing as the "equatorial bands" of Schröter, and both of these a broad ring of meteorites, revolving as a crowd of minute satellites around the equatorial region of the planet. The bright spot may have been one of the larger meteorites on its returning journey towards the sun striking the planet, and producing on a grand scale the luminous effects that are visible when meteorites enter our atmosphere, and which occur at elevations where its density is less than that which I attribute to the atmosphere of Mercury.

239. It is unnecessary to make any calculation of the theoretical atmosphere of Venus. The mass and dimensions of this planet approximate so closely to that of the earth that the density of its atmosphere should only differ from ours by so small a fraction that we may consider it as about the same. The only important meteorological differences between Venus and the earth would be those due to its greater proximity to the sun, and the greater inclination of the plane of its equator to that of its orbit.

240. In the first place there should be land and water as upon the earth, but as the quantity of heat received from the sun by Venus is nearly double that which comes to the earth, the atmosphere should be thickly loaded with aqueous vapour. This again would moderate the action of the sun upon the surface of the planet itself, as the vapour would absorb so much of the sun's heat, and retain it in the atmosphere. So far we should have the conditions of a hot, humid, and equable climate as compared with that of the earth; but then the

inclination of the axis, producing such extreme variations between the summer and winter exposure to the sun of the temperate and polar regions, would seriously disturb this equalizing influence of the aqueous vapour. Thus there would be great extremes of evaporation and condensation, and Venus should be a foggy, cloudy, and rainy planet, with some polar snow during the winter, and probably polar glaciers. Its atmosphere thus charged with vapours, and perhaps augmented in density by them, should be easily detected, but its surface configuration would be much obscured by the misty envelope.

241. This theoretical sketch, which is merely a statement of what *must* result if my hypotheses are correct, corresponds as nearly as possible with the conclusions derived from actual observation, as the following quotations will show. As the difficulties of observation have given rise to some differences of opinion in reference to the telescopic appearances of these inferior planets, Mercury and Venus, I purposely limit my quotations respecting them to one work, lest it should be supposed that I have *selected* from among differing authorities only those conclusions which favour my own views. The following quotations are, therefore, both from Guillemin's work on *The Heavens,* which is the only authority I have quoted in reference to the observed phenomena of Mercury.

242. "If we refer to the drawings of Schröter, which represent Venus in three of its phases, we shall notice that the luminous part of the disc is far from terminating abruptly along the line of shade. Its light, on the contrary, diminishes gradually; and this diminution may be entirely explained by the twilight on the planet. The existence of an atmosphere of considerable height has hence been inferred, which, by refracting the rays of the sun, enables them to penetrate into the regions where that body is already set. *Thus the evenings of Venus would be like ours, lighted by twilight, and the mornings by the dawn.*"* Summing up generally the results of various obser-

* Guillemin, *The Heavens*, p. 81.

vations, M. Guillemin says, "Perhaps *a very dense, cloudy atmosphere, constantly charged with vapours arising from the heat, envelopes the globe of Venus,* and thus moderates the rigour of its opposite seasons. A fact which gives to this hypothesis a certain degree of truth, is the observation of a transit of Venus over the sun in 1761. A nebulous ring seemed to surround the black disk of the body; and moreover, at the moment when it was but partly projected on the sun, the contour of the exterior limb of the planet was seen edged with a luminous ring."*

The cloudiness of this planet is further indicated by the differences of the statements of different observers respecting the permanent marking on its surface. Bianchini represents what, in his drawings, appear like two polar and two equatorial continents, which appearances we must, of course, interpret inversely, as these dark regions, which appear like the mapping of continents, should represent actual seas, and the lighter portions the more highly reflecting surface of the land. Referring chiefly to these, Mr. Lockyer, the translator of the English edition of M. Guillemin's book, makes the following note. "Before we quit Mercury and Venus, we must fairly state that the decision and positiveness with which the physical data are given by the old astronomers are by no means borne out by modern investigation, although we might imagine, to say the least, that if the observations of Schröter and others, faithfully recorded by M. Guillemin, were correct, the vastly superior telescopes of the present day would have verified them. This, however, they have failed to do. The different features, although stated to have been seen by De Vico during the present century, have not been observed either by Admiral Smyth or the Rev. W. R. Dawes; indeed the only physical fact which modern observation has placed before us, and this we owe to Professor Phillips and the Rev. T. W. Webb, is the possible existence of snow zones on Venus as on Mars. This,

* Guillemin, *The Heavens,* p. 83.

however, is not certain. We must, therefore, caution our readers against receiving absolutely the inferences drawn from the old observations. If we cannot see the observations on which they were based, we cannot of course verify them."*

243. As regards Mercury, I think that his distance from the earth and proximity to the sun may explain the uncertainties of observation to which these discrepancies are attributable; but this can scarcely be the case with Venus. The sunlight is, of course, in the way sometimes, and irradiation may be troublesome, but the proximity of this planet should afford other advantages to compensate these. If I am right, the observations of the surface of Venus have to be made through the veil of a misty atmosphere, such as must *always* prevail over this planet, even though absolutely opaque clouds should be absent. The effect of this mistiness would be to render the outline forming the boundary between land and sea very obscure and difficult to define, except under the most favourable circumstances. In the above note Mr. Lockyer refers to the vastly superior telescopes of the present day, but he does not appear to have considered that this is not a question of telescopic power, nor even of optical "definition," as that term is technically understood, but of reducing to a minimum the intervening obstructions to the distinct vision of minute differences of light and shade.

244. The Italian names of the astronomers, the two *Cassinis*, *Bianchini*, and *De Vico*, who, besides Schröter, have succeeded in seeing these markings, which our English astronomers have failed to detect, is, I think, very suggestive of another explanation, viz., that the difference may be due to the difference of the atmosphere in which the observers themselves were immersed.

245. I will illustrate this by a closely analogous case, derived from my own experience. During a short stay in Malta I found that on certain days I was able to see the summit of

* Guillemin, *The Heavens*, p. 84.

Mount Etna from the roof of the house in which I lodged at Valetta. The distance in a straight line is more than 100 miles. The outline of the snowy peak was so obscure that I should never have recognized it at all, but that while sailing from Syracuse I watched intently the waning "Mongibello," after the rest of Sicily had sunk below the horizon, and thus the form of the mountain, seen from the south, was clearly impressed on my memory. Even with this aid I had to take special care to guard against illusion; I marked a line on the parapet, the continuation of which cut the middle of what I believed to be the summit, and it was only after observing that from hour to hour no change occurred in the position of the object, that I became satisfied that it was not one of the misty phantoms of the horizon. It was only visible on three days during a fortnight, though I looked for it daily, and the weather was brilliantly clear every day (it was during the latter part of the month of April), and but for this delicate test object I could see no difference between one day and another in the clearness of the atmosphere.

246. Now in this case the problem of distinguishing the outline of the summit of Etna was closely similar to that of distinguishing the outlines of land and water on the planet Venus, if my description of her atmosphere is correct. I had to distinguish between the appearance presented by distant water, the sea horizon, a peak of snow-covered land and the misty background beyond; the impediment to this discrimination being the imperfect transparency of the intervening atmosphere. This analogy becomes closer if Venus is viewed when near to the earth, as her illuminated surface must then be seen obliquely. Now in the case of Mount Etna an increase of atmospheric obstruction too small to be otherwise observable, was sufficient to render the outline of the cone altogether invisible. May not then the difference between the transparency of an Italian and an English sky produce the same effect on the visibility of the dim surface markings of the planet Venus as the

difference between two apparently equally bright southern skies produced upon the visibility of the dim cone of Etna? I should add that one reason why I mistrusted my first observation, and drew the line upon the parapet was, that on the first day, when the cone was visible, I looked through a small telescope and swept the horizon very diligently, but could find no cone, though I could still see it with the naked eye. This, of course, is explained by the fact that merely magnifying an object by no means deficient in magnitude, was no advantage, transparency of the visual medium being the only requirement, and that was diminished by the imperfect transparency of the glasses of the instrument. Lord Rosse's telescope would not have rendered Etna visible on any of the days when I failed to see it with the naked eye.

247. I think, therefore, that we should not regard the negative results of recent observations with superior telescopes, as controverting the positive observations of the Italian astronomers, even though they had instruments of very inferior magnifying or space-penetrating power, for in this case it is not the distance nor the smallness of the object that constitutes the difficulty; the problem is altogether different from that of discovering a minute satellite, defining a doubtful double star, or delineating the boulders in a lunar crater. It consists in reducing to a minimum the sum total of misty obscurations lying between the surface of the planet and the eye of the observer. The planetary markings in question, as described by the Italian astronomers, were scarcely more definite than the outline of Etna, which I saw so obscurely through the mists of the sea horizon, and therefore I think, that the additional impediments of an English sky would be quite sufficient to conceal them, and that until the modern telescopes are applied to this planet under more favourable atmospheric conditions, we may accept the observations of the Italian astronomers, confirmed as they are by Schröter, as the best that have yet been made.

CHAPTER XVI.

THE METEOROLOGY OF MARS.

THE THEORETICAL ATMOSPHERE OF MARS—THE ATMOSPHERE AND CLIMATE OF MARS AND MERCURY COMPARED—THE HYDROGRAPHY OF MARS—THE CLIMATE OF MARS—THE CONDITION OF THE SEAS OF MARS—THE VAPOURS OF THE MARTIAL ATMOSPHERE—NO DENSE CLOUDS IN THE ATMOSPHERE OF MARS—ATMOSPHERIC OBSCURATION DUE TO HOAR-FROST—LIMITS OF THE DEPOSITION OF HOAR-FROST—APPEARANCES DUE TO THE HOAR-FROST—FORMATION OF POLAR ICE MOUNTAINS—GREAT POLAR AVALANCHES, ETC.—EFFECTS OF GLACIER EROSION AT THE POLAR REGIONS OF MARS—DIFFERENCE BETWEEN MY VIEWS OF THE METEOROLOGY OF MARS AND THOSE USUALLY ACCEPTED—PROFESSOR PHILLIPS ON THE METEOROLOGY OF MARS—MY VIEWS OF THE METEOROLOGY OF MARS RESULT INEVITABLY FROM MY PRIMARY HYPOTHESIS—THE DENSITY OF THE ATMOSPHERE OF MARS HAS NOT BEEN DIRECTLY MEASURED—THE CLOUDS DUE TO A DENSE ATMOSPHERE HAVE NEVER BEEN OBSERVED—THE "CLOUDS" DESCRIBED BY MR. LOCKYER ARE ONLY A SEMI-TRANSPARENT AND ABSORBENT VAPOUR HAZE—A CERTAIN DEGREE OF HAZE IS CONSISTENT WITH MY VIEW OF THE ATMOSPHERIC CONDITIONS OF MARS—THE OBSERVED DISTRIBUTION OF THE HAZINESS CORRESPONDS WITH MY THEORETICAL REQUIREMENTS—VARYING LUMINOSITY OF THE DISC OF MARS—MY EXPLANATION ACCOUNTS FOR THE VARYING LUMINOSITY—"THE CIRCUM-POLAR SNOWS" OF MARS—THE HOURS OF DEW-FALL AND MORNING THAW ON MARS—EVIDENCE OF OVERTHROW OF THE POLAR ICE-CONE—DISCREPANCIES IN THE MEASUREMENTS OF THE POLAR DIAMETER OF MARS—AN EXPLANATION OF THESE DISCREPANCIES—THE RELATIVE PROPORTIONS OF LAND AND WATER ON MARS—A NOTE ON SECTION 260.

248. I MUST now pass over the earth to the superior planets, beginning with Mars. The mass of Mars is to that of the earth as 0·1324 to 1, and his diameter 0·519 to 1. $\sqrt{0{\cdot}1324}={\cdot}363$ and $0{\cdot}519^2=0{\cdot}269361$. The total atmosphere of Mars should

therefore be $0{\cdot}1324 \times 0{\cdot}364 = 0{\cdot}0482285 = \text{about } \frac{1}{20}$. And the atmospheric pressure on the surface of the planet $\frac{0{\cdot}0482285}{0{\cdot}26936} = 0{\cdot}179 = \text{about } \frac{1}{5{\cdot}5}$ atmospheres. The mercurial barometer would therefore stand at about 5½ inches at the sea level of Mars: the atmospheric pressure should be about 2¾ lbs. to the square inch, and water should boil at 138° Fahr.

249. The total quantity of atmosphere on Mars would thus be more than double that of Mercury, but its density or pressure on the surface would be only one-fourth greater in consequence of the greater proportional surface due to the lower specific gravity of Mars. But the meteorological and hydrographical effects of these two atmospheres must be very different indeed, in consequence of the great difference in amount of solar heat to which the two planets are respectively exposed. The mean intensity of the solar radiations upon Mercury are sixteen times as great as those upon Mars. Thus while the atmospheric water of Mercury would exist commonly in the gaseous state, the bulk of that on Mars must be condensed and frozen.

250. The quantity of water upon the surface of Mars should be to that of the earth in about the same ratio as that of its atmosphere,—perhaps I should say a little greater on account of the excessive condensation. It should thus be about one-fifth; and if the relative average depths of the oceans of the two planets are about equal, the proportion of land to water on the surface of Mars should be five times greater than the proportion of land to water on the surface of our earth. The proportion of the area of land to that of water on the earth being, according to Sir Charles Lyell, nearly one in four, or one of land to three of water, there should be on Mars about five of land to three of water.

251. As the intensity of the sun's heat upon Mars is to that upon the earth as 0·431 to 1, or rather less than one half, the mean temperature of Mars must be considerably below the freezing point; from the thinness of the atmosphere and

the small quantity of aqueous vapour it must contain, the variations of temperature between day and night must be very great, especially at the polar regions, where, in consequence of the inclination of the axis of the planet, so large a portion of each hemisphere is exposed alternately to several months of continuous sunshine, and several months of continuous darkness.

252. How must these conditions affect the ocean or seas of Mars? The *mean* temperature being below its freezing point, they must be frozen to the bottom; but the surface of the water on all parts of the planet which are exposed with only moderate obliquity to the sun's rays, would be thawed to a depth varying with the duration and verticality of such exposure. Its surface would thus be thawed during the day and frozen again at night, like the surface of our own Alpine glaciers, but the variations would be far more sudden and severe on the Martial ocean.

253. A feathery dew of hoar-frost should begin to fall before sunset, *i. e.*, as soon as the obliquity of the solar rays allows the surface to cool below the freezing point. There should be a continuous current flowing outward from the mid-day regions,—where the thin atmosphere would be greatly augmented by the aqueous vapour,—towards the comparative vacuum on the dark side of the planet. There would be the same kind of action as Sir John Herschel has described as necessarily taking place in the moon if any water exists on that satellite, and which he compares to the cryophorous experiment. There should, however, be some difference between the case of Mars and the moon. The vacuum of Mars being only comparative, the action would be much slower and less decided than in Sir John Herschel's supposed case; and the mean temperature of Mars being so much lower, the freezing point and consequent precipitation of a haze of hoar-frost must commence considerably before reaching the actual boundary between light and darkness,—at that angular distance from solar verticality where the cooling influences of the planetary radiation, aided by those

of the remaining ground-ice, must reduce the surface temperature to the freezing point.

254. Thus there would be no great well-defined masses of vesicular vapour floating irregularly, like our clouds in the atmosphere of Mars:—no cumulus, no cumulo-stratus, nor even cirro-cumulus clouds; and, excepting at the borders of the polar ice, nothing denser than a thin veil of stratus, or cirro-stratus cloud formed of ice crystals, the kind of cloud or mist which, in our atmosphere, makes halos round the moon, and only hides her face sufficiently to exaggerate her beauty, like the gauze "complexion-veil" of the human coquette. The mid-day region and a certain distance around it would but rarely be subject to this small degree of obscuration, as the sun's heat there should, under ordinary circumstances, hold all the vapours it had raised in complete and transparent solution.

255. Thus the human observer watching the planet at the most favourable time—that of conjunction—when his line of vision would nearly coincide with that of the sun's rays,—should see the central portion of the disc well defined and subject to but little variation; but towards the limb on all sides he should meet with some degree of haze or obscurity, though there should be nothing like the contradictory uncertainty attending the observations of the surface-markings of Venus, with her atmosphere of five times the density, loaded with clouds and vapours produced by four times the amount of solar heat.

256. It follows from the above that a deposition of hoar-frost must be continually taking place all round the disc of the planet, and that this will commence at a certain angular distance from the meridian centre of the disc, and increase gradually towards the circumference. The rotation of the planet will, however, produce a considerable difference in the results of this deposition. All that falls on the east and west sides of the planet will be thawed and evaporated by the next day's sunshine, so that the maximum accumulation in these directions

can be but one night's deposition; but on the north and the south there will be a continuous accumulation, which will only be thawed up to a certain latitude by the annual summer presentation of either hemisphere to the sun.

257. The terrestrial observer should therefore see coincident with the zone of thin border-haze an increased brightness due to the daily deposition of hoar-frost. On the eastern and western borders of the disc this brightness should be but slight, as it would be produced only by the light deposit of one night's icy dew; but in the polar regions the accumulation should be considerable and distinctly visible. It should form a circular patch whose boundary must recede and advance with the annual turning of the poles of the respective hemispheres towards and away from the sun. The circumference of this patch should be bounded by a zone of mist or haze produced by the condensation which must be in continual progress where the heated vapour-charged air from the meridian regions is entering the region of frost and precipitation. This should be more dense and definite than the corresponding zone on the eastern and western boundaries of the isothermal line of 32° Fahr., on account of the abrupt variation of temperature being necessarily greater at the north and south boundary line between summer and winter than at the east and west boundary of day and night.

The distance between the mean limits of the north and south patches of accumulated hoar-frost may be taken as an approximate measure of the diameter of the circle over which the sun's rays are capable of raising the day-time temperature above the freezing point. The circumference of this circle would form the daily travelling isothermal line of 32° Fahr. This angular distance would thus enable observers to determine the boundaries at which to look for the commencement of the evening deposition of hoar-frost, and the line of the morning thaw. The latter should appear sharper and better defined than the former.

258. I must not conclude this theoretical vision of the meteorology and hydrology of Mars without referring to some curious results that must follow from the polar accumulations of the hoar-frost deposit. At the poles, and for some distance around them, the annual amount of deposition must exceed the annual amount of thawing and evaporation; and therefore a gigantic glacial mountain must there accumulate, with a continual growth and tendency to assume the conical form. As the deposition of ice crystals would commence before actual sunset, and would probably reach its maximum or even be finished before reaching the boundary line of day and night, (in consequence of the thinness of the atmosphere of this planet and the resulting rapidity of radiation,) the building up of this polar mountain would be very irregular. In mid-winter the lower slopes of its sides would receive the greatest accessions. With the advancing line of daylight the elevation of the zone of maximum deposition would increase until it reached the summit. This coincidence of maximum deposition with the summit would occur twice a year, before and after midsummer. During the summer the only regions receiving any deposition at all would be the summit and its immediate vicinity; while at the same time the sides would be rapidly thawing by the powerful action of the continuous sunshine of the one long arctic summer day. At this season, the slopes of the arctic mountain would be riven by gigantic ice-floods and water-floods;—avalanches, glaciers, and torrents.

259. The tendency of this summer growth of the summit, and undermining of the sides, would be to bring about periodical catastrophes, by the more or less complete toppling over of the mountain cone in the form of a gigantic avalanche. The occurrence of such a catastrophe would be most sensibly indicated to a terrestrial observer by an irregular and temporary extension of the polar whiteness, where the debris of the great avalanche had been hurled beyond the general glacial boundary, and had usurped the region of the summer thaw. If ice pos-

sesses the viscosity attributed to it by Professor Forbes, this tendency to the sudden overthrow of the polar peak would be in a great measure counteracted by the bulging and advancing of the base; but if the phenomena attributed to viscosity are only the results of regelation, the overbalancing tendency of polar growth can be but slightly relieved by such action, and the catastrophes must be of enormous magnitude.

260. The rocks at the base of this great ice peak must exhibit on a grand scale all the effects of glacier erosion. They must be polished, grooved, and dished out so as to form great circular valleys surrounding each of the poles of the planets; and beyond these circumpolar furrows somewhere about the arctic and antarctic circles of Mars, there must be corresponding ridges of moraine, consisting of the materials which the advancing polar glacier has scooped out and driven before it. This perpetual outthrust of the great polar glacier,—this continual erosion of the polar regions of the planet, ever at work since the date of its primeval consolidation, must have produced a sensible alteration of the shape of the planet,—must have flattened it in the immediate neighbourhood of its poles, and bulged it in the moraine regions beyond.

261. Let us now see how far these theoretical expectations accord with observed phenomena. Those who are acquainted with the telescopic appearances of Mars may perhaps be induced while reading the above, to suppose that I have only been fitting my theory to the known facts. My reply to this is simply that I cannot possibly avoid this *appearance,* as it necessarily follows if my theory is sound; but a little further consideration will refute this accusation should it be made. The refutation is this. That many theories have been thus made, *after the facts,* to account for the phenomena of Mars, and all these explanations differ very greatly from mine. They assume a greater quantity of atmosphere than my calculations warrant;—they say that the climate and meteorology of Mars corresponds very nearly to those of our earth; I maintain

that these differ so greatly that none of the creatures of this world could live upon Mars. These theories speak of snow and rain, mine denies the existence of either. They have great difficulty in accounting for the climate, which gives so-called *snow* patches of winter no greater extension than those of our earth. My calculated atmospheric density removes all this difficulty, and explains all the phenomena as inevitable consequences, without any assumptions of internal heat, or any other peculiarity beyond the calculated atmospheric density.

262. The following paragraphs from a paper by Professor Phillips in the *Proceedings of the Royal Society,* for January 26th, 1865, illustrate some of these differences; he says:—

"The relative mean distances from the Sun of Mars and the Earth being taken as 100 and 152, the relative solar influence must be on Mars 100 to 231 on the earth; so that the surface of the more distant planet might rather be expected to have shown signs of being fixed in perpetual frost, than to have a *genial temperature* of 40° to 50°, if not 50° to 60°, *as the earth has taken on the whole.* How is this to be accounted for? Of two conceivable influences which may be appealed to, viz., very high interior heat of the planet, and some peculiarity of atmosphere, we may, while allowing some value to each, without hesitation adopt the latter as the more immediate and effective." "To trace the effects in detail must be impracticable; but in general we may remark that as a diminution of the mass of vaporous atmosphere round the earth would greatly exaggerate the difference of daily and nightly, and of winter and summer temperature, so the contrary effect would follow from an augmentation of it. Applying this to Mars, we shall see *that his extensive atmosphere* would reduce the range of summer and winter, and of daily and nightly temperature. It would, moreover, augment the mean temperature by the peculiar action of such an atmosphere, which, while readily giving passage to the solar rays, would resist the return of dark heat-rays from the terrestrial surface, and prevent their wasteful emission into space. This effect obtains now on the earth, which is rendered warmer as well as more equable in temperature by the atmosphere than it would be without it. It is conceivable that it may obtain upon Mars *to a greater degree*, even without supposing the atmosphere to be materially different in its nature from that round the earth or the surface of Mars, and have any specially favourable or exceptional characters for the absorption and radiation of heat. It seems, however, requisite to suppose a greater communication of heat from the interior of the planet; for otherwise the additional vapour, to which the warming effect is in the main to be ascribed, could not probably be supported in the atmosphere. On the whole we may, perhaps, be allowed to believe that Mars is habitable."

263. The above is a statement of the inferences from the observed appearances of Mars to which astronomers generally have been led, and had I reasoned *à posteriori* as they have, I have little doubt that I should have come to the same. The imperative obligations of my primary hypothesis drove me away from the beaten track of *à posteriori* explanation, into direct contradiction of the "genial temperature" and the "extensive atmosphere" reducing "the range of summer and winter and of daily and nightly temperature," etc., and compelled me to start with a 5½-inch atmosphere for Mars, and a consequent series of meteorological conditions so diametrically opposite to those which have generally been assumed as necessary to account for the polar patches and general markings of this planet; but having once started upon this unauthorized track, I followed up the trail of necessary consequences which it suggested, and found that it led me where at first I little expected to land, viz., upon a vantage ground which has enabled me to view far more clearly and logically than has ever been seen before, the causes upon which may depend the surface phenomena of the best observed and most accurately known of all the planets.

264. I will now proceed to compare some of the details of observation with my *à priori* conclusions. At the outset I must state that I am not aware of any direct observation, by refraction or otherwise, which affords anything like a basis for quantitative estimation of the atmosphere of Mars. The "extensive atmosphere" commonly described is purely hypothetical; assumed to exist, in order to account for the polar "snow patches," and other surface markings. I must, therefore, appeal to the indirect evidence, and ascertain whether this is more favourable to my view of an atmosphere of less than one-fifth of the density of our own, or to the usually assumed atmosphere of equal or greater density than our own.

265. If Mars had the dense vaporous atmosphere thus assigned to it, this atmosphere should be clouded like our own, and these clouds, like our own, should be sufficiently opaque to *perfectly conceal* the body of the planet, wherever they covered it. M. Guillemin says, "If snow falls in Mars, it is because water is there evaporated by heat; hence, the water must spread on the surface under the form of clouds, which condense sometimes in a liquid state in the form of rain, sometimes as snowy crystallizations."* Now we know from terrestrial experience that such rain-clouds and snow-clouds effectually conceal the midday sun; if so, how small a slice of an ordinary nimbus cloud would be sufficient to utterly obliterate the obscure surface markings which the telescope reveals upon this planet. Sir John Herschel tells us that "careful observations, continued for ten years, have also taught us that the dark spots of Mars *retain constantly both their forms and their relative positions on the planet.*" Now this could not possibly be the case with a cloud-bearing planet, subject to rain-fall and snow-storms. Considerable areas must sometimes be veiled with clouds that would utterly obliterate the constant markings, and substitute new and totally different spots and markings. These

* *The Heavens*, p. 200.

would be as variable as the belts of Jupiter, or the cloud-spots of Venus, which have caused so much trouble, and led to such contradictory descriptions by different observers. The very favourable circumstances under which the best observations of Mars have been made would enable the observers to have watched and recorded the daily variation of the clouded zones or regions of the planet; to have described their intermittent opportunities of observing the well-known green and reddish patches, which they believe to be sea and land, and to have told us of the bad-weather obscurations that must have hidden them *completely* from their view.

266. Nothing approaching to this is recorded. It is true that Mr. Lockyer speaks of "clouds," but what sort of clouds? Exactly the sort of clouds that a 5½-inch atmosphere, having a temperature below 32°, could carry. He says, "In 1862 the planet was clearer of clouds *and more ruddy* than in 1864. The suggested explanation is that, when Mars is clouded, the light reflected by the clouds undergoes less absorption than that reflected by the planet itself; and, on one occasion, the spectroscope indicated this increased absorption by revealing the fact that the sunlight was reflected to us, minus a large portion of the blue rays." The italics of the above are Mr. Lockyer's. From this it appears that the uttermost obstructive effort of these "clouds," as they are called, was merely to *diminish* the ruddiness of the planet; they were just capable of making the supposed sandstone of Mars appear more like that which we see in the railway-cuttings near Edinburgh, and less like that around Chester. Such an effect is merely the work of the thinnest haze of invisible cirro-stratus vapour, such as in our atmosphere would conceal the milky way, without obliterating the brighter stars. In reference to his observations in 1862, Mr. Lockyer says, "Although the complete fixity of the main features of the planet has been placed beyond all doubt, daily, nay, hourly changes in the detail and in the *tones* of the different parts of the planet, both

light and dark occur. These changes are, I doubt not, caused by the transit of clouds over the different features."

267. It is perfectly consistent with the conditions I have described that the atmosphere of Mars should be subject to about this extent of vaporous variability, for the *dry* land, subject to the continous glare of sunshine through so thin an atmosphere, must be heated very considerably; under some circumstances possibly near to the boiling point, while the icy seas could only be superficially thawed and raised to the freezing point. Thus the air in passing from the land to the water would be subject to sudden refrigeration, which would cause a mistiness proportionate to the degree of saturation with aqueous vapour. With so thin an atmosphere this could never amount to anything like an opaque terrestrial stratus cloud or fog. The gradual advance of the continuous summer sunshine upon the arctic and antarctic glacier regions must produce extreme variations of temperature, and a consequent fogginess analogous to that which prevails in Arctic Norway during May and June, until the winter snows have gone, but, of course, much thinner. This tendency would attain its maximum when a great avalanche from the polar ice mountain had flung a vast encroachment of ice blocks far beyond the normal limits of the polar ice, and left them stranded upon the land which, during the long arctic summer day, may be heated by the unimpeded sun rays above the Martial boiling point of 138° Fahr. Thus the streams from the melted ice would boil as they flowed over the land, and an epoch of haziness, such as Mr. Lockyer describes, would then prevail over a considerable portion of the planet.*

268. I have already stated that the normal distribution of this haze or thin stratus cloud should be all around the outer borders of the disc, while the central parts of the planet should usually remain clear, on account of the solar and the plane-

* In Sect. 273 reasons are given for concluding that this actually occurred in two places in 1864, just when Mr. Lockyer observed the greatest atmospheric obscurity.

tary radiation and convection being there sufficient to retain in the gaseous state the greater part, if not the whole, of the vapours they have raised. Professor Phillips says, "First, then, in respect to the permanence of the main features of the planet. I submit several drawings made between the 14th of November and the 13th December (both inclusive), the dates being marked on each, for comparison with others made in 1862, partly by Mr. Lockyer, and partly by myself; from which it will immediately appear that no appreciable change has occurred in the main outlines of land and sea, in the longitudes observed. A certain fogginess has been noticed, especially on the 18th and 20th of November, such as does not commonly occur with Jupiter or Saturn; but it seemed to be due to no essential circumstance of the planet, *for it grew less and less as the observation approached the meridian.*"* This fringe of fogginess producing a gradual disappearance of the spots and markings as they approach the borders of the disc, is a constant feature which all observers have recorded. This and the white polar spots have led them to speculate upon the basis of a *dense* atmosphere. My hypotheses of a thin hoar-frost precipitation from a thin atmosphere is, I think, far more consistent with *all* the facts, especially with the absence of any opaque and definite cloud masses in the central parts of the planet.

269. Guillemin says, "Throughout its circumference the disc is more luminous than its central parts." All agree in this observation, which is in perfect accordance with that afternoon precipitation of hoar frost which I have shown to be a necessary consequence of the atmospheric density I have calculated. This increased luminosity of the outer portions of the disc must be exhibited by the thin white film thus daily deposited on those parts of the surface of the planet which would combine with the thin haze or fogginess in accounting for that

* *Proceedings of the Royal Society*, January, 1865, p. 43.

obliteration of the distinctions between the greenish colour of the sea and the orange tint of the continents, which is observed just where this deposit should be the thickest.

270. If Mars has no atmosphere, the luminosity of his disc should be equal all over. If he has an atmosphere capable of reflecting more light than is reflected by the body of the planet, it must have a degree of opacity more than sufficient to conceal all the differences between the greenish and the orange tints of the sea and land surfaces, under whatever angle they may be viewed through it. Seeing that the luminosity of the planet is due to reflected sun-light, and that its atmosphere can only receive its light from the same source, or from the planet itself, I am utterly at a loss to conceive any conditions of density, and of absorption or reflection, or both, pervading *the whole* planetary atmosphere, which can explain the combined phenomena of increased luminosity of the borders of the disc, and increased transparency in the central portions. On the other hand, my explanation meets the whole difficulty in the simplest and most natural manner; as the reflecting powers both of the atmosphere and the surface of the planet must be increased at the borders of the disc by the precipitation of the hoar-frost which I have described; and by the same agency, the spots at the borders would be obscured, while the central parts continued clear.

271. It is unnecessary for me to quote the records of observation respecting the circumpolar "snows" of Mars. Their regular growth around either pole as it turns away from the sun, and their decrease as it turns towards it, are in such obvious and perfect accordance with the theoretical description I have given, that I need make no further comment on the greater facts, though some suggestive details and minor phenomena may here be noted.

272. Professor Phillips tells us that the limit of the winter polar snows reaches to about lat. 50, and that the perennial icy circle which endures the summer sun extends to "about

8° or 10°, more or less, round the pole." If my views are sound, the space between these boundaries, viz., 130°, will give us an approximation to the diameter of the circle within which the sun's rays are strong enough to melt the daily deposits of hoar-frost, and outside of which, to the west and the east, the evening dews will begin to fall, and the morning dew will remain unthawed. Thus, if I am right, we may predicate that at the periods of the vernal and autumnal equinoxes the frozen dew will begin to fall upon the equatorial regions of Mars at about 4.20 p.m., and that the surface of the ground will begin to thaw at about 7.40 a.m.

273. I have said that the periodical overthrow of the polar ice cone would be most sensibly indicated to the terrestrial observer by the irregular extensions of the white polar circle, caused by the debris of the avalanches, which would thus be hurled beyond a portion of its normal boundaries. Such extensions have been observed by Professor Phillips, and they appear to have puzzled him so much as to lead him to suspect that they must be optical illusions. He says, "Snowy surfaces, scarcely more defined, but much more extensive, were observed in parts of the northern regions, not immediately encircling the pole (which was invisible), but in two principal and separate tracts, estimated to reach 40° or 45° from the pole. On one occasion (30th November) two practised observers (Mr. Luff and Mr. Bloxridge) noticed with me one of these gleaming masses of snow very distinct, so much so that, as happened with the south polar snow of 1862, *it seemed to project beyond the circular outline*, an optical effect, no doubt, due to the bright irradiation. This white mass reached to about 40° or 45° from the pole, in the meridian of 30° on my globe of Mars. Another mass was noticed on the 14th and 18th November, in long. 225°, and extending to lat. 50°. In each case the masses reached the visible limb."* I have marked in italics the obser-

* *Proceedings of the Royal Society*, January 26, 1865, p. 43.

vation respecting the apparent projection beyond the circular outline, which was observed on both of the occasions referred to. Professor Phillips attributes this appearance to irradiation, but if I am right, it may be due to the actual heaping of the avalanche material of the overthrown polar ice cone.

274. Before leaving this part of the subject I must hazard a rather bold suggestion, relative to the great diversities in the measurement of the polar compression of Mars. Humboldt tells us that "The compression of the planet Mars at its poles, which, singularly enough, has always been doubted by the great astronomer of Königsberg, was first recognized by Sir William Herschel in 1784. In respect to the amount of ellipticity, however, uncertainty long prevailed. William Herschel gave it as $\frac{1}{16}$ (? $\frac{1}{26}$); according to Arago's more exact measurement with a prismatic telescope of Rochon, it would be much less; in 1824 he found it as 189 : 194, or $\frac{1}{38{\cdot}8}$; and by a later measurement in 1847, it was found $\frac{1}{32}$. Arago, however, is inclined to believe that the compression is somewhat greater than either of the two last named quantities."* Guillemin says, "Arago, who made a series of measures of the two diameters with great care, concluded that the latter (the polar) is shorter than the former by the thirtieth part of its value. Herschel, in 1784, found the same quantity $\frac{1}{26}$; whilst more recent measures appear to reduce it to a third of the value measured by Arago. M. Kaiser (of Leyden) gives $\frac{1}{118}$ for the flattening, as measured during the opposition of 1862."† Schroter's measurement gives $\frac{1}{80}$; Mr. Main's for 1862, $\frac{1}{37{\cdot}59}$.

275. The suggestion I refer to is that the downfall of a few miles of polar ice-cone may produce a sensible variation in the polar diameter of Mars. I do not suppose that this can be great enough to reconcile such extreme variations as those of Schröter and Kaiser; but, comparing the different measure-

* *Physical Description of the Heavens*, p. 369-70. † *The Heavens*, p. 211.

ments made by the same observer with the same instrument at different times, these discrepancies of Arago's measurements (which to me appear the most remarkable) come quite within the range of such an explanation. The total difference between his measurement of 1824 and that of 1827 is about 21 miles. The extreme difference between Herschel and Arago amounts to about 50 miles. Now the winter base of the polar snow-cone has a diameter of about 2,000 miles, and even its restricted summer base is about 500 miles in diameter. This perpetual building up must of necessity, sooner or later, bring about some sort of catastrophe, either a crushing down or a rolling over when the summer heat has weakened the base; thus a reduction of the elevation of the cone to the extent of 20, 30, or even 50 miles, is not an extravagant assumption considering the dimensions of the accumulation. If both cones should happen to give way on two successive summers, the reduction of the polar diameter at the period of the second catastrophe would be equal to the sum of the reduction of the north and south cone minus the thickness of one year's deposit. A catastrophe of the highest magnitude above named is required to explain the extent of the phenomena observed by Professor Phillips (see sect. 273).

276. I have stated that if the average surface inequalities of Mars should relatively correspond to those of the earth, the proportion of land to water on Mars should be five times greater than on the earth. Attached to the paper by Professor Phillips which I have so freely quoted, and to which I am so much indebted, is a carefully drawn chart of the land and sea of Mars; an equatorial projection extending to 60° of north and south latitude. I have divided this into 120 squares, and each of these squares into quarters, and have thus determined the relative area of land and water as there indicated. According to my theoretical requirements of 5 land to 3 water, there should be in 120 parts, 75 land to 45 sea. I find on Professor Phillips's chart 79 land to 41 sea. This is quite as near to the theoretical deduction as can be expected or required.

I have dwelt thus lengthily upon this planet, because the comparative fulness of our knowledge of its physical details renders it capable of testing the more severely my primary hypothesis. I should like to proceed further in tracing the meteorological and other consequences of the atmospheric conditions I have ascribed to this planet, but must forbear at present.

NOTE ON SECTION 260.

276 *a*. When I visited London in order to complete my arrangements for the printing and publication of this work I purchased from Mr. Browning, of the Minories, his recently published *Stereograms of Mars*. With these is included a small *Chart of Mars on Mercator's Projection*, compiled by Mr. R. A. Proctor. Unlike that of Professor Phillips, referred to in sect. 276, this chart includes the polar regions (Professor Phillips's only extends to 60° north and south latitude). My expectations of the results of glacier erosion are there depicted as distinctly as though the chart were an ideal drawing of my own. The theoretical north circumpolar excavation is shown extending as a narrow band of sea completely round the planet at the edge of the north polar ice, and nearly parallel to it. This ring-shaped furrow is named by Mr. Proctor "Schröter Sea," and beyond it is a circular ridge of land exactly answering to the circular moraine which I theoretically require. This is named "Laplace Land." The same features are repeated on the southern hemisphere. The south ring-shaped furrow or erosion-valley is named "Phillips Sea;" and the moraine, which is quite continuous and like the north circular ridge, nearly represents a parallel of latitude, by its polar boundary, has some remarkable promontories on its equatorial side. These are named "Cassini Land" and "Lockyer Land." By colour-

ing the land of this little chart brown and the sea blue, these peculiarities show out very strikingly, the more so because they are the only features in the distribution of land and sea which present any approach to regularity.

Phillips Sea and Schröter Sea are perfectly barred out from all communication with the other seas of the planet by the continuous parallel circular ridges of land forming their shores, and thus they may be at quite a different level from the equatorial seas, in accordance with what I have suggested at the end of section 260.

When the planet is in a favourable position it will be very interesting to observe its outline across these rings of land, in order to determine whether there is any visible heaping up, corresponding to great moraine ridges which I expect to result from the polar erosions; and whether the diameter of the planet measured across from ridge to ridge is sensibly greater than the polar or other diameters.

CHAPTER XVII.

JUPITER.

THE THEORETICAL ATMOSPHERE OF JUPITER—THE ATMOSPHERIC PRESSURE ON JUPITER—HEAT EVOLVED BY THE COMPRESSION OF JUPITER'S ATMOSPHERE—ACTION OF THIS HEAT UPON ATMOSPHERIC WATER—STORAGE OF HEAT BY AQUEOUS VAPOUR—THE TIME REQUIRED FOR COOLING THIS VAPOUR—THE BODY OF JUPITER INVISIBLE—PROFESSOR LEITCH'S SUMMARY OF THE TELESCOPIC PHENOMENA PRESENTED BY JUPITER—HUMBOLDT'S DESCRIPTION OF THE SPOTS OF JUPITER—JUPITER IS PROBABLY WHITE HOT—SPECIFIC GRAVITY OF JUPITER INCONSISTENT WITH SOLIDITY—MY GENERAL VIEWS OF THE CONSTITUTION OF JUPITER—THE BRIGHT SPOTS OF JUPITER—EXPLANATION OF THE BRIGHT SPOTS—JUPITER HAS PROBABLY REACHED HIS PERMANENT TEMPERATURE—NOTE ON MR. BROWNING'S OBSERVATION OF THE "CHANGES IN JUPITER."

277. THE mass of Jupiter to that of the earth is as 338·7 to 1; its diameter as 11·225 to 1. $\sqrt{338 \cdot 7} = 18 \cdot 4$ and $11 \cdot 225^2 = 126$. The total atmosphere of Jupiter should therefore be $338 \cdot 7 \times 18 \cdot 4 = 6232$ times that of the earth. The atmospheric pressure on the surface of Jupiter should be $\frac{6232}{126} = 49 \cdot 5$ atmospheres. Here we come upon such totally different figures from the results of previous calculations, that the theory is subjected to a very severe test; for if the telescopic revelations of the meteorology of Jupiter do not materially differ from those of Mercury, Mars and Venus, all my speculations must break down; and, on the other hand, if the observed phenomena present the very great differences which my theoretical estimate demands, this concurrence of extremes is a correspondingly "ostentatious example" in support of their soundness.

278. The above calculations are based on the usually received assumption that Jupiter is a solid globe such as our earth, the inferior planets, and Mars; and that its visible dimensions represent those of such a globe. I shall hereafter give reasons for rejecting this assumption of solidity, but in the meantime proceed with it in order to make an approximate comparison of the atmospheric conditions of Jupiter with those of the earth and the other planets already considered. Thus the atmospheric pressure on this assumed surface of the planet would be above 740 lbs. per square inch, equal to a column of mercury 134 feet high.

279. Applying the formula of Leslie to Jupiter's atmosphere, as I have already done to that of the sun, this pressure gives us 1237° Centigrade, or 2259° Fahr., or according to Dalton 2475° Fahr., as the temperature produced in the lower regions of Jupiter's atmosphere by its own condensation. This is about the melting point of cast iron, and near to the temperature at which carbonic acid is dissociated into carbonic oxide and oxygen. As the visible surface of Jupiter upon which the above stated measurement of his surface area is based, is obviously the outer surface of his great vaporous atmosphere, the above estimate of both pressure and temperature must, as in the analogous case of the sun, be accepted as an under statement of the pressure and heat evolved in the lower atmospheric regions of this planet. The boiling point of water under a pressure of 49½ atmospheres is about 509° Fahr.

280. According to this, Jupiter must have originally presented a remarkable analogy to the sun; the same kind of actions which I have described as taking place in the solar atmosphere from the dissociation of water would, in the atmosphere of Jupiter, occur from its evaporation. The aqueous vapour of the general atmospheric medium, if simply subject to such a pressure without any accompanying supply of heat, either from its own compression or otherwise, would at once become solid; or, in other words, it would require the same

quantity of heat to *retain* it in the gaseous state when subject to such condensation, as would be necessary to *convert* it from the state of ice of the temperature of space to that of aqueous vapour, 509° Fahr. Thus a part of the original heat evolved by the atmospheric compression would be converted into the heat of vaporization (*i.e.*, the latent heat of steam); just as the greater portion of the heat evolved by solar compression has been shown to be occupied in the work of dissociation.

281. The result of this would be an analogous storage of heat, as Jupiter would thus be enveloped in a lower atmosphere of aqueous vapour,—dry steam, which could only condense at its outer surface at a rate which would be limited by the possibilities of radiation. Outside of this sphere of clear vapour there should therefore be a stratum of vesicular vapour—a *cloud-sphere*, corresponding to the solar *photosphere*. This cloud-sphere would limit the radiation from the dry vapour, and from the nucleus of the planet, by receiving and radiating back their heat, just as our terrestrial radiations are returned to the earth's surface on a cloudy night.

282. I will not attempt to estimate how long must be the period that this muffled radiation should continue in order to reduce the whole of the vapour envelope, and the surface of the planet itself, below 509°, and thus permit the existence of a liquid ocean in Jupiter's valleys, as for reasons that I shall state hereafter I do not believe that it has yet occurred, or ever will; but, on the contrary, that all the water on Jupiter is now, and will permanently remain, in a state of vapour.

283. My primary hypothesis thus demands that Jupiter shall be enveloped in such a depth of vaporous atmosphere that his body must be hidden from human observation; and that his visible disc shall be but the reflecting surface of his enveloping cloud-sphere, which, to whatever possible stage of cooling down the planet may have arrived, must still be too dense for our vision to penetrate. This is fully confirmed by the united testimony of all competent observers. In old books the dark

belts of Jupiter are described as bands of cloud stretching across the visible face of the planet, or as seas with varying tides. Better telescopes, more accurate observation, and a sounder knowledge of the effects of light reflected from cloud masses, have corrected this idea, and have established the conclusion that the bright visible disc of Jupiter is the outer surface of his cloud envelope, and the dark bands are long openings or deep cuttings through it.

284. The following compact summary by Principal Leitch, of Queen's College, Canada, of the general results of modern observation, will show at a glance how completely they fulfil all my theoretical requirements. "In the case of Jupiter and Saturn, it is obvious that we see only the outer shell, within which the bodies of the planets are concealed. The disc of Jupiter presents very singular phenomena. There are indications of constant commotion, and the markings of the belts often present very perplexing forms, of which no account can be given. We only know that the visible disc is not a fixed and solid crust. It is like the visible envelope of the sun, which conceals the solid nucleus in its interior. The shadows of the satellites are seen as dark spots when they cross the disc of the planet, and the satellites themselves can also at the same time be detected by powerful telescopes. Besides these there are other spots of which no account can be given. They sometimes appear in clusters, as shown in our figure of the planet. They have a proper motion like the spots on the sun, and probably are due to the same general cause—the rotation of the body, combined with the higher temperature of the equatorial regions. They will therefore correspond with the circular storms or cyclones of the atmosphere of our globe. There is no evidence that we have ever as yet seen the kernel within the outer shell of Jupiter. The usual explanation of the dark belts of Jupiter is, that they are the more transparent parts of Jupiter's atmosphere, while the brighter parts are the region of clouds which reflect the light more abundantly. In this hypothesis we see

the body of the planet down through the transparent region of dark belts: but it is more probable that, in the dark belts, we see only a part of an interior shell, and that the real body may lie far beneath. The dark belts would, in this way, correspond to the penumbra of the spots in the sun, which are only an uncovered part of the stratum immediately under the luminous envelope."*

285. The analogy to the sun which follows so necessarily from my working out of the hypothetical atmosphere of Jupiter is thus referred to by Humboldt, "Occasionally (it was so according to Schwabe's observations in November, 1834) the spots of Jupiter, viewed with a magnifying power of 280 in a Fraunhoper's telescope, resemble small spots on the sun, having nuclei surrounded by penumbras; but even then their degree of blackness is less than that of the shadows of the satellites. The nucleus is probably part of the planet itself. These spots sometimes divide into two or more, also resembling in this respect the spots of the sun, as was already recognized by Dominique Cassini in 1665."

286. I have little doubt that Jupiter is still red-hot, or rather white-hot, that a vast depth of aqueous and other vapour surrounds it, and that these, together with the free oxygen and free nitrogen, form a very much greater atmosphere than that which I have calculated. I think it extremely probable that the temperature of dissociation of water has been reached by the original atmospheric compression of Jupiter, that he must have manifested some degree of general solar phenomena, and that if we could see him shaded from the solar rays, he would appear like a phosphorescent, or rather a fluorescent ball, by the illumination of his vaporous envelope, due to the light it absorbs from the glowing world within.

287. This may at first sight appear an extravagant assumption, but a little consideration will, I think, show that the

* *God's Glory in the Heavens*, p. 180-1.

appearance of extravagance is entirely due to the novelty of the idea, and not to its inherent character. The assumption of solidity and anything like terrestial temperature, such as are usually attributed to Jupiter, is, in spite of its familiarity, a really extravagant hypothesis; for there is no kind of matter of which we have any knowledge that could form a globe having the dimensions and specific gravity of Jupiter unless it were heated to a state of fusion.

The solid shell enveloping liquid and gaseous matter lighter than itself which has been suggested, and is still generally adopted, crumbles and melts away at the touch of sound reasoning based upon the established facts of experimental science.

288. The urgent necessity of bringing this Essay to a close, and of resisting the temptation to ride off upon the many issues it presents, compels me to abstain at present from a detailed argument of this question, and I will therefore do no more than summarize my views of the constitution of Jupiter, which are, 1st, That the bright surface which we see is an envelope of cloud, chiefly aqueous, like the clouds of our own atmosphere.

2nd, That outside of this cloud-sphere is an atmosphere somewhat denser than that above our own clouds, and of similar composition to our own atmosphere.

3rd, That below this cloud-sphere is a sphere of heated and uncondensed gases consisting chiefly of aqueous vapour, carbonic acid, and our atmospheric mixture of nitrogen and oxygen, and having a temperature continually increasing downwards proportionately to the increasing pressure. This temperature probably reaches that at which carbonic acid is dissociated into carbonic oxide and oxygen.

4th, That below, and partly diffused and intermingled with these lighter gases, is a profound ocean of the denser vapours of such volatile metals and metalloids as may exist in Jupiter. All of these must be dissociated from oxygen.

Finally, That below this ocean of vapours, the arrangement of which will be determined by their specific gravities and mutual powers of diffusion, there is a planetary nucleus which includes the denser and more fixed constituents of Jupiter, but which, like the nucleus of the sun, must be but a very small kernel compared with the great volume of outer vapours. If its dimensions exceeded a small fractional part of the whole volume of the body which is measured as the planet Jupiter, it must raise the mean specific gravity above that which has been determined for this planet.

If my views of the constitution of the sun are sound, the above description of Jupiter must be equally correct. The conclusions in both cases are based on similar facts and precisely the same reasoning.

289. The following description of the *bright* spots of Jupiter by Sir John Herschel* is very suggestive when regarded in the light of this view of the constitution of Jupiter: "But the most singular phenomena presented by the belts of Jupiter is the occasional appearance upon them of perfectly round, well-defined, bright spots, (not unlike the discs of satellites as they are occasionally seen projected upon the planet when passing between it and the earth, only smaller). They vary in situation and number, as many as ten having, on one occasion (Oct. 28, 1857), been seen at once, but, so far as hitherto observed, only on the southern hemisphere of Jupiter. They were first noticed by Mr. Dawes in the spring of 1849, but first described and figured by Mr. Lassell, March 27, 1850."

290. The reader should now refer to the quotation from Humboldt, section 285, and consider it in reference to this description of the bright spots. An explanation of this "singular phenomenon" will thus be suggested. If my explanation of the constitution of Jupiter is correct, the reaction of the gravitation of Jupiter's satellites must effect upon their Primary the same kind of disturbances as that of the planets upon the

* *Outlines of Astronomy*, art. 512.

sun. The great diameter of Jupiter, the great depth of his atmosphere and comparatively small dimensions of his nucleus, must, as in the case of the sun, produce an important amount of irregularity between the gravitation of the satellites upon the nucleus and that upon the gaseous matter of the planet, the consequence of which must be the formation of cyclones similar to those producing the sun spots. If the spots described in the foregoing quotation from Humboldt are such cyclones, there must be corresponding eruptions, and these of no small magnitude, as the descriptions of some of the dark spots show that the spot cavities must be nearly, if not quite, large enough to envelope our earth. These eruptions must be great outbursts of aqueous vapour, bearing with it some of the denser vapours of the lower gaseous strata. They must produce upon Jupiter great cloud-prominences exactly analogous to the flame-prominences of the sun. In the midst and at the base of these cloud-prominences there will probably be some amount of flame due to the oxidation of carbonic oxide and of volatile metallic vapours whose temperature of dissociation is lower than that of water.

291. If this is the case, Jupiter has probably reached his permanent temperature, which must have been attained as soon as he cooled down to that point at which his radiations into space exactly compensated the amount of heat obtained by the impact and compression of the quantity of fresh atmospheric fuel which the disturbing action of his satellites supply. This temperature, for the reasons I have already stated, should, at the base of the aqueous vapour, be not less than a white heat, or that at which cast-iron fuses, and sufficient to dissociate carbonic acid. In the regions of the *planetary* gases, *i.e.*, of the gaseous metals, the heat of compression must be sufficient to effect the dissociation of water, and thus *some* of the aqueous vapour carried down into the dark spot-vortices must be dissociated, and by its explosive recombination assist in ejecting the material of the bright spots.

NOTE ON MR. BROWNING'S OBSERVATIONS OF THE "CHANGES IN JUPITER."

291 *a*. Since the foregoing chapter was written a communication from Mr. Browning appeared in *Nature*, Dec. 2, 1869, page 138, from which I extract the following:—"During the months of October and November the planet Jupiter has presented a spectacle of singular and almost unexampled beauty. The belts on the planet are more than usually numerous, and they display a greater variety of colours than I have ever yet seen ascribed to them. The equatorial belt, which has been for years the brightest part of the planet, is now not nearly so bright as the light belts to the north and south; usually it has been free from markings, now it is often covered with markings, which resemble piled up cumulus clouds: it has generally been colourless, shining with a silver-grey, or pearly lustre—now it is of a rich deep yellow, greatly resembling the colour of electrotyped gold. . . . The poles of the planet are ashy blue, and the darker belts nearest to them present a darker tint of the same colour. The bright belts next these are pearly white, and shine more brilliantly than any other portion of the planet. The dark belts next to the central bright belts are coppery red. As already mentioned, the central belt, which has been for years a pearly white, is now a rich golden yellow. . . . Spectrum analysis has taught us to suspect that any change in the colour of light proceeding from an object, indicates a change in the object itself. If Jupiter, the largest planet in the solar system, has still retained so much heat as to shine partially by his own light, the present considerable change in colour may enable spectroscopists to obtain some information on this interesting subject."

The above is very welcome to me, as I have felt some compunctions respecting my own hardihood in ascribing so high a temperature to this planet, on which so many astronomical authors have supposed a mean temperature like that of our earth to exist; and have described its dense cloudy atmosphere as the provision designed to compensate, by resistance to planetary radiation, for its remoteness from the sun. Mr. Browning is the only companion that has yet enlivened the solitude of my speculative wanderings upon Jupiter. His suggestion to spectroscopists is, I think, likely to be a fruitful one, as the continuous spectrum due to the reflected light from the outer clouds may be diluted by a sufficient train of prisms, and if the lesser glow presents any bright bands they may possibly be examined like those of the solar prominences.

CHAPTER XVIII.

SATURN.

THE ATMOSPHERE OF SATURN AND THE HEAT EVOLVED BY ITS COMPRESSION—THE VISIBLE SURFACE OF SATURN LIKE THAT OF JUPITER—SATURN MUST BE CHIEFLY COMPOSED OF GASEOUS MATTER—ORIGIN OF THE SUPPOSED TEMPERATURE OF THE LIGHTER PLANETS—REACTION OF THE GRAVITATION OF SATURN'S SATELLITES COMPARED WITH THAT OF JUPITER'S—THE CONSEQUENCES OF THIS POWERFUL REACTION—SATURN SHOULD, LIKE THE SUN, EJECT GASEOUS PROMINENCES AND VOLLIES OF METEORIC SOLIDS—THE PLANE OF THE ORBITS OF SATURN'S EJECTIONS—THE RINGS OF SATURN EXPLAINED—THE RINGS OF SATURN COMPOSED OF SMALL BODIES—SATURN'S RINGS A NECESSARY RESULT OF AN UNIVERSAL ATMOSPHERE—SATURN MUST HAVE RINGS IF MY EXPLANATION OF THE SOLAR PHENOMENA IS SOUND—THE RINGS OF SATURN COMPARED WITH THE EJECTIONS FROM THE SUN—CONFIRMATION OF MY GENERAL HYPOTHESIS AFFORDED BY THE PHENOMENA OF SATURN'S RINGS—THE CHANGES OF THE RINGS ACCORD WITH THEORETICAL DEDUCTIONS—THE RINGS MAY BE SUBJECT TO REGULAR PERIODICAL CHANGES—VARYING BRILLIANCY OF THE RINGS—THE SPOTS OF SATURN—SATURN PROBABLY HAS A PHOTOSPHERE—IS SATURN SELF-LUMINOUS?—THE NEBULOUS APPENDAGES OF THE RINGS—THE ORBITS OF THE RING COMPONENTS ARE NOT NECESSARILY CIRCULAR.

292. THE mass of Saturn is to that of the earth as 101·364 to 1, and his diameter as 9·022 to 1. $\sqrt{101 \cdot 364} = 10$ (nearly) $9 \cdot 022^2 = 81 \cdot 4$. The total atmosphere of Saturn (excluding the rings) should therefore be $101 \cdot 364 \times 10 = 1013 \cdot 64$. And the atmospheric pressure on the surface of the planet $\frac{1013 \cdot 64}{81 \cdot 4} = 12 \cdot 5$. The pressure per square inch will be 187½ lbs. According to Leslie's formula the temperature due to this amount of com-

pression is 310° Cent., or 590° Fahr. According to Dalton it should be 625° Fahr.

293. The boiling point of water under the pressure named is about 377° Fahr., and therefore Saturn should be surrounded with an envelope of aqueous vapour, due to the same actions as I have already described as taking place in the atmospheric portion of the matter of Jupiter. Its quantity, however, would not be so great. This difference of quantity would not affect the general telescopic appearances, and, therefore, the visible phenomena of the belts, etc., should be nearly the same in both planets. The surface of Saturn is thus described by Sir John Herschel, "The figure represents Saturn surrounded by its rings, and having its body striped with dark belts, somewhat similar, but broader and less strongly marked than those of Jupiter, and owing doubtless to a similar cause."*

294. The specific gravity of Saturn being still lower than that of Jupiter, I am irresistibly led to deny its solidity, and even to go further, and maintain that we are justified in believing that it is still hotter than Jupiter, and that the proportion of its gaseous constituents must also be greater. In order to suppose otherwise we must again invent new forms of matter, for the whole range of our chemistry does not include any *inorganic* substance, which either in the solid or liquid state, and under only the moderate pressure of our atmosphere, has so low a specific gravity as that of Saturn,—only $\frac{3}{4}$ of that of water at 60° Fahr. Subject to the internal pressure of the mass of this planet, even hydrogen itself would vastly exceed this density, unless its condensation were overpowered by the expansive force of heat. Solidity of the whole of the observed orb is, therefore, physically impossible (except to the poetical creators of new and ethereal minerals), and if a liquid or solid portion does exist, it cannot extend beyond the very core of the planet.

295. If Saturn and the other light planets are thus mainly

* *Outlines of Astronomy*, art. 514.

composed of gaseous matter, the mechanical work of gravitation exerted in their aggregation and compression, must have produced a vastly greater evolution of heat than I have calculated as due to the compression of merely that portion of the general atmospheric medium constituting their supplementary envelope; and this high temperature, after being reduced to a certain point, would be maintained by the gravitation of their satellites, acting in the manner described in sects. 291 and 297.

296. The total mass of the satellites of Saturn has not, I believe, been calculated, but the united mass of all the satellites of Jupiter is estimated at $\frac{1}{6000}$ of that of the planet. Now Saturn's sixth satellite, Titan, alone has a diameter of $\frac{1}{16}$ of its primary, and thus his *volume* is $\frac{1}{4000}$ of the planet. The mean distance of the first four satellites of Saturn is $\frac{1}{4}$ of the mean distance of Jupiter's four attendants. The other four satellites still remain to be added, so that I may safely estimate the power of reaction of the gravitation of Saturn's satellites upon the planet as about twenty times that of Jupiter's. Their power of disturbing the geometrical regularity of his rotation must be proportionally greater, and all those atmospheric irregularities produced by irregular rotation to which I have ascribed the sun-spots, must be operating in an exaggerated and exceptional degree in the gaseous envelopes of Saturn.

297. This being the case, and as Saturn is travelling with the sun in his flight through space, these disturbances must reproduce upon the planet all the phenomena described in Chapter V. as maintaining the solar heat. Thus Saturn must be, even more completely than Jupiter, a miniature or model sun. Like the sun, his nucleus must be reeling within a profound envelope of dissociated gases, which must be thus stirred up into great vortices, and correspondingly upheaved with still greater relative violence, as the mass of his satellites compared to their Primary is so much greater than that of the planets to the sun. Dissociated water would probably play a far less important part

than in the sun, as the atmospheric film of Saturn must be so much thinner; and, therefore, the heavier dissociated gaseous metals of the interior would be upheaved in greater relative abundance, with consequences that I shall presently consider. Into the depth of these vortices the fresh atmospheric matter of newly entered space would be whirled, and there compressed and dissociated. All the heat-evolving agencies described as occurring to maintain the solar heat would be repeated, and the decalorized atmospheric matter would in like manner be left behind. Thus a high permanent temperature would be maintained, higher than that of Jupiter, in spite of Saturn's lesser mass, in consequence of the very much more powerful agitation and instirring of atmospheric fuel, due to the greater disturbing power of the satellites.

298. Assuming that the materials of Saturn are similar to those of the earth and the sun, there should be, as in Jupiter, immediately below the outer envelope of atmospheric matter, an ocean of gaseous metals, restrained from oxidation by the dissociating force of heat, and these would be arranged somewhat in the order of their density.* The relative planetary reaction of the satellites being at least twenty times greater than that of the satellites of Jupiter, and the envelope of aqueous vapour being thinner than Jupiter's, these dissociated metals would be ploughed out far more profoundly, and Saturn would not merely eject the cloudy matter described in section 290. The vortices of Saturn must not only perforate the atmospheric envelope, but must penetrate beyond the stratum of dissociated light metals of our ocean salts (sodium, magnesium, potassium, etc.) into the region of the dissociated material of our rocks and metal-veins, and in so doing must carry with them a core of atmospheric gases, the water of which would be dissociated by the heat evolved by the compression to which the accompanying free gases would be submitted at those

* All I have said in section 288 respecting the internal constitution of Jupiter must be understood as applying equally to Saturn.

depths. This must be again ejected along with the necessarily upheaved gases of the heavier metals, and the mixture of the explosive elements of water, combustible metals, and free oxygen, on surging outwards upon the surface of the planet must explode with tremendous fury, and thus mimic all the solar phenomena. The flaming prominences, the vollies of the corona, the zodiacal light, the meteoric rings, and the zone of asteroids, would be repeated on a scale which, though but in miniature, compared with the meteoric efforts of the sun, should be, relatively to the dimensions of the planet, much greater, on account of the greater relative mass of the disturbing satellites, and the greater relative proportions of condensible matter that would accompany the atmospheric gases.

299. As the angular velocity of Saturn's rotation is about fifty-eight times greater than that of the sun, the projectiles from the planet would have a greater tendency to assume circular orbits. As the great majority of the ejections would, as in the case of the sun, be made from the vicinity of the equator, the plane of these orbits would, for the most part, deviate but little from that of the planet's equator, and these deviations would be gradually rectified by the predominant gravitation of its bulging equatorial regions, and the co-operating gravitation of the general resultant of the combined masses of the meteorites themselves.

300. What would be the telescopic appearance of such a multitude of meteorites, all flying around the equator of Saturn, at different distances from his surface, but all in orbits having nearly the same plane? Precisely that of the greatest telescopic marvel of all the marvellous heavens, the rings of Saturn.

301. That these rings cannot be continuous solids nor liquids, nor a number of concentric rings, nor rings of irregular mass, as supposed by Laplace, has recently been so fully demonstrated, that I need not repeat the well-known arguments; and that they must be made up of a multitude of small bodies,

such as meteorites or minor satellites, is now so generally admitted, that it is quite unnecessary for me to discuss this confirmatory conclusion.

302. Thus the great mystery of Saturn's rings is resolved into a simple consequence, a demonstrable and necessary result of the operation of the familiar forces, whose laws of action have been determined here on earth by experimental investigation in our laboratories. No strained hypotheses of imaginary forces are required, no "ethers" or other materials are demanded, beyond those which are beneath our feet and around our heads here upon this earth; all that is necessary is to grant that the well known elements and compounds of the chemist, and the demonstrated forces of the experimental physicist, exist and operate in the places, and have the quantities and mode of distribution described by the astronomer; this simple postulate admitted, these wondrous appendages spring into rational existence, and like the eternal fires of the sun, the barren surface of the Moon, the dry valleys of Mercury, the hazy equivocations of Venus, the seas, the continents and polar glaciers of Mars, and the cloud-covered face of Jupiter, follow as necessary consequences of the fundamental hypothesis of an universal atmosphere.

303. If Saturn had no rings, my explanation of the zodiacal light, the meteoric zones, and the asteroids, would be half refuted by their absence. If any other planet had rings, and Saturn were without, this portion of my speculative fabric would be hopelessly shattered by such a contradiction of the theoretical requirements; for Saturn, by reason of his magnitude, mass, and preponderating system of satellites, is the one exceptional planet of our system which, according to the absolute demands of my hypothesis, should most completely mimic the meteoric efforts of the sun.

304. If this explanation is correct the rings of Saturn should present modifications similar to the meteor zones of the sun. There should be first, a zone corresponding to the zodiacal light,

composed of the smaller and lighter particles of ejected matter, and including those of temporary orbit and immediate return. It should, as I have explained, sections 192 and 216, include a great proportion of the junior meteors, and it should, like the zodiacal light, be for these reasons subject to continual variation. Outside of this we should have a zone corresponding to that which sometimes drops its masses upon our earth, and beyond this another analogous to the zone of the asteroids. This outer zone should, like that of our asteroids, be characterized by its stability; and for the same reasons, viz., that it must consist of the greater exceptional ejections, and probably include the results of an earlier stage of the planet's existence when it had a higher temperature and more explosive energy than that which it can manifest at present. Such a zone would be subject to no visible changes due to modern accessions.

305. I must request the reader to carefully examine the above, and to compare it with what I have already stated in reference to the sun, so that he may be satisfied that these detailed results must of necessity follow if my primary hypotheses are correct. Unless he does so he will read it merely as a description of recent telescopic observations rather than a series of necessary logical deductions which I might have made without any knowledge of these modern revelations of the telescope. I am anxious to impress this clearly, because the evolution of such an apparent anomaly as the rings of Saturn out of an hypothesis framed only to explain solar phenomena, and worked out when I supposed, in accordance with my early elementary reading, that the rings of Saturn were continuous solids or liquids, and when I had but the duskiest notions concerning the newly discovered dusky ring, and knew nothing whatever of the recently observed changes of the rings, has impressed me more forcibly than anything else as a glaring confirmation of the general soundness of these views. When I commenced these estimates of the atmospheres of the planets for the sole purpose of testing the primary hypothesis of an

universal atmosphere by comparing their calculated with their observed atmospheres, I had no idea of thus coming upon this explanation of Saturn's rings, which has been forced upon me in the same manner as my unexpected conclusions respecting the meteorology of Mars. It was not until after I had thus worked out these explanations, and was looking up further information on the subject (astronomical details being quite out of the range of my customary studies) that I learned that the inner or dusky ring has already been compared to the zodiacal light on account of its remarkable outward physical resemblances, and that a meteoric origin has been attributed to this and the other rings. This explanation, however, differs from mine in the same manner as the usually received meteoric explanation of the zodiacal light. The meteors are regarded as bodies that have come to Saturn from without, just as the meteors that fall upon our earth, and which are supposed to form the zodiacal light and to bombard the sun, are regarded as external cosmical bodies invading our solar system. This explanation shows no reason why Saturn should be so specially favoured while all the other planets are neglected by these wanderers.

306. In accordance with my theoretical anticipations it has been found that the outer ring is the stable portion of the system, that the inmost of the bright rings is, according to M. Otto Struve, subject to considerable changes. He has compared the measurements made during the last two hundred years, and concludes that it has manifested an amount of inward growth, which, if continued, will bring its inner edge to the planet in about one hundred and twenty years. This extent of growth is, however, disputed by other astronomers, but some degree of change is generally admitted. The changes of the dusky ring corresponding to our zodiacal light are unquestionable; and it is even supposed by some to have actually come into existence but recently, as it was discovered simultaneously in 1850 by Mr. Bond in America, Mr. Dawes in England, and Father Secchi at Rome; and was not seen before, either by

these observers or others using telescopes of far higher power than those with which it can now be easily observed.

307. The strict accordance of all these changes with my explanation of the origin of the rings needs no further comment; but I will venture one more suggestion. It is this: that Saturn may, like the sun (sect. 78), be subject to large periods of meteoric activity, and that the recent development of the dusky ring is due to this. If such is the case, the inward growth of the bright ring and the development of the dusky ring should occur together just as they have been observed; the dusky ring representing the smaller and more temporary results of the same eruptions, which would display their greater projectile efforts in permanent additions to the nearest portion of the bright rings.

308. The observed variations of brilliancy in the different rings accord with my explanation of their origin as completely as the degrees of their mobility. The inmost ring of all, composed of the smallest, lightest, and consequently most feebly projected particles, with a small sprinkle of the denser masses in the course of the commencement of their flight, is just of the filmy character to be expected from the size of its component parts and the quantity due to their temporary existence. The inner portion of the bright rings should be the most closely packed from being within the range of the ordinary projection of the denser masses, and it accordingly presents the greatest degree of brightness; it has been compared to *burnished* silver. The outer ring formed of the largest and least abundant, or the extraordinary projectiles, should, on account of its more discrete composition, exhibit a somewhat different appearance; telescopic observers have detected such a difference, and compare the surface of the outer ring to that of *frosted* silver. Now if we examine with a moderate magnifier the surface of frosted silver, and compare it with a burnished surface similarly magnified, we find that the difference in effect is due to a number of minute prominences more or less rounded, of

which the surface of the frosted metal is made up. In like manner, if I am right, and the magnified surfaces of the rings presenting the burnished and frosted appearances could be still further magnified, the outer or frosted one would show the prominences due to the larger individual meteoric particles of which it is composed, while like the burnished silver the inner rings would still exhibit their apparent continuity of surface. If the magnifying power be carried far enough, even the burnished silver would appear rough, lumpy, and corrugated; so also would the surface of the inner bright ring.

309. If I am right, the spots on Saturn should form a far more important feature on his disc than they do on Jupiter's. I have not yet met with any *comparative* account of these spots; nothing further than a general statement of the existence of such spots on both planets. The prevalence of the spots should also vary with the positions of the satellites, and thus be subject to considerable variations. With such wondrous and fascinating phenomena before them as those connected with the rings, it is not surprising that the observers of Saturn should have paid but casual attention to his spots beyond using them as a means of determining the period of his rotation. Special observation directed to these features of the disc of Saturn will doubtless settle some of the questions I have raised.

310. In section 291 I have stated reasons for believing that Jupiter has reached his permanent temperature. Precisely the same reasoning applies to Saturn, but in applying it to this planet we are forced to the conclusion that his present and permanent temperature must be higher than that of Jupiter. The very much greater relative mass and *proximity* of the satellites of Saturn must, as already stated, produce a far greater stirring of his atmosphere, affording thereby a proportionally greater supply of fresh atmospheric fuel, and thus the temperature at which his radiations should balance the heat communicated by such supplies must be higher, even after allowing for the difference of protecting atmosphere. It is therefore

probable that Saturn may still have a photosphere below his cloud-sphere. If so, it must of course be vastly thinner and less potent than the solar photosphere; incapable of fully vaporizing all the water of the planet's atmospheric envelope, and thus will allow the formation of an enveloping cloud-sphere like that of Jupiter.

311. If such is the case, Saturn should present some indications of self luminosity, in spite of its cloudy envelope. In accordance with this theoretical expectation, I find that telescopic observers have described it as "a golden ball." Whence comes this golden glow? The reflection of solar light from clouds of aqueous vaporous matter cannot produce it; but if the light of a thin photosphere were faintly visible through a veil of aqueous vapour, which, as we know, absorbs most readily the blue rays, a golden ball exactly describes the appearance to be expected.

312. If the foregoing views are correct, something analogous to the corona of the sun must exist around Saturn, and besides this, there must be recent erratic masses flying in various courses non-coincident with the rings. These may exist in great abundance though no human observer has ever seen them. They must be far too small to be visible singly even with the aid of modern telescopes. In order that such fragments shall thus be visible, they must be closely heaped together so as to be seen by the aid of their concurrent reflections. Such an aggregation should occur in the form of what I may call the prevailing or resultant orbit of the whole, which for reasons I have stated (sect. 299) should be nearly circular and coincident with the plane of the planet's equator. The nebulous appendages, "like clouds of less intense light lying on the ring,"* that have been observed *when the ring is seen edgewise*, are just seen at the time and place when such a phantom should be visible according to this explanation; for

* Guillemin, *The Heavens*, p. 248.

next in abundance to those in the equatorial plane should be the wanderers who deviate but little from it, and these would be most visible when seen in the profile of the general resultant of their orbits. They would thus, if sufficiently numerous, give just that nebulous thickening to the ring which is represented. I should observe that all such wanderers would be subject to gradual reclamation by the predominant gravitation of the ring and the equatorial mass of the planet, which must ever be tending to rectify erratic orbits, and bringing them into general coincidence with the visible ring. These "clouds" "lying on the surface of the ring" may, however, be atmospheric phenomena; for the mass of the rings is nearly equal to that of the earth, and thus they must have about the same amount of atmosphere as the earth has, though of less density in consequence of greater extension. Such an atmosphere should be subject to cloudiness, or rather to cirro-stratus haze formations like those of Mars. These vaporous strata, if visible at all to telescopic observers, would be seen when viewed edgewise, just as we see our thin cirro-stratus clouds forming dark bands when viewed edgewise near the horizon.

313. It does not by any means follow that the orbits of all the components of each ring are circular. They may all be elliptical provided their planes coincide, and the eccentricity does not exceed the breadth of the ring. The optical result of these orbits would be a circular ring provided the axes of the ellipses were distributed in every direction. The outside radius of the visible ring would correspond to the widest excursions of the revolving bodies from the centre of their Primary, and the radius of the inside of the ring to their nearest approach.

CHAPTER XIX.

URANUS AND NEPTUNE.

THE TELESCOPE DOES NOT REVEAL THE ATMOSPHERIC PHENOMENA OF URANUS—THE PHYSICAL CONSTITUTION OF URANUS—THE RELATIVE GRAVITATION OF THE SATELLITES OF URANUS AND JUPITER — THE DENSITY OF URANUS COMPARED WITH THE GRAVITATION OF ITS SATELLITES—THE THEORETICAL ATMOSPHERE OF NEPTUNE—NEPTUNE MUST BE PARTLY A GASEOUS PLANET—THE DENSITY OF NEPTUNE COMPARED WITH ITS SATELLITES — CONNECTION BETWEEN THE NEBULAR HYPOTHESIS AND MINE — SPECTROSCOPIC OBSERVATIONS ON THE LIGHTER PLANETS.

314. M. GUILLEMIN says, "Observations have, at present, given us no information on the physical constitution of Uranus. No feature is visible on the disc at such a distance. Astronomical calculations can only tell us of its mass, which is fifteen times that of the earth: taking this and its volume into account, we find for the density of the matter of which it is composed, a value equal to $\frac{1}{16}$ of that of our earth; the density of Uranus, therefore, is a little more than that of ice."* This being the case, a calculation of its theoretical atmosphere, such as I have made for the other planets, is of little value for comparison with observed atmospheric phenomena. I may, however, state in passing that the total atmosphere of Uranus should be about fifty-four times that of the earth; but this is spread over so great a surface that its pressure will be a little more than $2\frac{1}{2}$ times that of our own atmosphere, or 38 lbs. to the square inch.

* *The Heavens*, p. 263.

315. After the speculations into which I have been unexpectedly drawn in applying the principles of my explanations of the solar phenomena to Jupiter and Saturn, even the scant information that is afforded by direct observation of this planet has considerable interest. The density of this planet is intermediate between that of Jupiter and Saturn, and if my views of the constitution of these two planets are sound, Uranus should, like them, be mainly composed of gaseous matter, and must have the general constitution of a minor sun. Much of the heat evolved by the compression of its gaseous constituents (I refer now to planetary, not to atmospheric gases) must be still maintained, and the source of this high permanent temperature ought to be traceable to the amount of disturbing reaction of its satellites.

316. Uranus has four satellites, concerning the existence of which there can be no doubt. The existence of three others is very doubtful. The mean distance of the four satellites from their Primary is 248,288 miles. The mean distance of those of Jupiter is 667,697 miles. Assuming that their masses, relative to their primaries, are equal, the united gravitation of these four satellites of Uranus upon itself is about eight times that of the united gravitation of Jupiter's satellites upon Jupiter. It will be remembered that I estimated the gravitation of Saturn's satellites at about twenty times that of Jupiter's.

317. If I am right in attributing the low specific gravity and the assumed gaseous composition and high temperature of Jupiter, Saturn, and Uranus to the fuel-supplying agency of the gravitation of their satellites, the gravitation of the satellites of Uranus should, like its density, be intermediate between those of Jupiter and Saturn, and according to the best data I am able to obtain, this appears to be the case. But perhaps it will be said that eight is nearer to one than to twenty, and therefore that the density of Uranus should be nearer to that of Jupiter than to that of Saturn, which is not the case; the density of Jupiter being to that of the earth as 0·227 to 1; of

Saturn as 0·131 to 1; and of Uranus as 0·167 to 1. Thus it is lighter than it should be according to such a mode of estimating. This lightness follows as a natural result of the inferior dimensions of Uranus. If all the other conditions remained the same, and its dimensions were intermediate between those of Jupiter and Saturn, there can be little doubt that the additional pressure would bring its specific gravity nearer to that of Jupiter than to the density of Saturn.

318. If the telescope supplies no available information respecting the atmosphere of Uranus, the same is of course the case with Neptune, the atmospheric accumulation of which, according to my method of calculation, should be 82 times as great as that of our earth, and its pressure at the commonly estimated surface of the planet a little more than 4½ times that on our earth, or 68 lbs. to the square inch.

319. The density of Neptune is about one-third greater than that of Jupiter, or about one-third of that of the earth. Thus its specific gravity is not quite double that of water, and rather less than that of sulphur. Considering the pressure of the planetary mass (eighteen times that of the earth) upon itself, the superficial portion of its planetary matter must still be gaseous, and its solid nucleus far below, and thus, like Saturn, Uranus, and Jupiter, it will be capable, though in a minor degree, of mimicking the vortices and outbursts of the sun, provided it is supplied with a sufficient amount of satellite power. Such satellite reaction, if I am right, is absolutely necessary to explain the maintenance of its observed degree of lightness, and the high temperature which I connect with such specific gravity.

320. Here, however, we have a different case from those of the two preceding planets, which, being lighter than Jupiter, required, according to my hypotheses, a great relative amount of satellite reaction to account for the high temperature necessary to resist the condensation of their constituent materials. Neptune has a greater density than Jupiter, though its inner

strata are subject to so much less pressure, therefore it should have a less effective supply of satellites. In strict accordance with this theoretical expectation only two have been discovered, and these at much greater distances from their primary than the corresponding satellites of Saturn and Uranus. I am, of course, aware that these two satellites may not represent the whole of Neptune's attendants; the recent discovery and great distance of this planet renders it quite possible that the existing amount of telescopic discovery is by no means exhaustive, and rather more satellite power is, I think, required to explain the actual density of Neptune.

321. If I am right in thus connecting the small density of these outer planets with the heat-sustaining disturbances of their satellites, some mathematical problems of great interest are thereby opened, the solution of which will subject my hypotheses to the severest possible test. The connection between these and the nebular hypothesis of Laplace is very obvious, interesting, and suggestive. I might have simplified, and apparently strengthened, most of the foregoing speculations by taking the nebular hypothesis for granted; by assuming as a matter of course that the sun and all the planets were originally gaseous, and have been condensed and cooled down from that condition; but I have abstained from doing so, because, as already stated, I believe that this building of one hypothesis upon another hypothesis is a vicious proceeding, and in spite of illustrious examples and high authorities, I maintain that such a custom is damaging to the sound progress of science, and is "more honoured in the breach than the observance." Nevertheless, two collateral hypotheses, each independently deduced from strict inductions of observation and experiment, may be placed side by side, and their relations to each other examined, compared, and worked out to the great illumination and possible extension of both, if sound.

322. Spectroscopic observations, so far as they go, are confirmatory of these views of the constitution of the lighter

planets. Dr. Roscoe, summarizing the results of Mr. Huggins' researches, says, "In the spectrum of Jupiter lines are seen, which indicate the existence of an absorptive atmosphere about this planet. These lines plainly appeared when viewed simultaneously with the spectrum of the sky, which, at the time of observation, reflected the light of the setting sun. One strong band corresponds with some terrestrial atmospheric lines, and probably indicates the presence of vapours similar to those which float about the earth. Another band has no counterpart amongst the lines of absorption of our atmosphere, and tells us of some gas or vapour which does not exist in the earth's atmosphere. From observations upon Saturn it appears probable that aqueous vapour exists in the atmosphere of that planet, as well as in that of Jupiter."* Could we strip off the veil of cloud in which these planets are enveloped, the spectroscope might reveal a great deal more; but looking, as we must, upon the outer surface of this envelope, air and water are all that we may expect to discover. If it is possible to isolate and separately examine the *spots* of these planets, something further may be revealed, if my view of their gaseous composition is correct; but if these spots are, as usually supposed, mere cumuli of the general cloud matter, their examination promises nothing of special interest.

* *Spectrum Analysis*, p. 230.

CHAPTER XX.

THE NEBULÆ.

THE BRILLIANCY OF THE NEBULÆ—THE NEBULÆ MAY BE CLUSTERS OF SEMI-SOLAR ORBS LIKE JUPITER AND SATURN—THE ATMOSPHERE OF SUCH A GROUP—EFFECTS OF THE MUTUAL GRAVITATION OF AN IRREGULAR CLUSTER OF GASEOUS ORBS—EFFECTS OF GEOMETRICAL DISTRIBUTION OF NEBULOUS CLUSTERS—EFFECTS OF IRREGULAR DISTRIBUTION OF NEBULOUS CLUSTERS—VARIABILITY OF FORM OF NEBULOUS CLUSTERS—VARYING LUMINOSITY OF NEBULOUS MATTER—VERIFICATIONS OF MY HYPOTHETICAL SKETCH OF THE NEBULÆ—MODERN VIEW RESPECTING THE DISTANCE OF THE NEBULÆ—THE VARIABLE AND FLASHING STARS—FORMATION OF NEBULÆ BY THE AGGREGATION OF STARS—ACTION OF AN ISOLATED GROUP OF STARS UPON EACH OTHER—FACTS SUGGESTIVE OF THE STELLAR ORIGIN OF SOME OF THE NEBULÆ.

323. If I am right in thus ascribing a semi-gaseous constitution to the outer and lighter planets, a solution is at once suggested to the riddle of the nebulæ. We know that there are clusters of stars of various forms and modes of partial aggregation, some very irregular, but most of them tending to the globular, the annular, and the elliptical. The number of their components seem to vary as widely as their arrangement. Sir John Herschel says, "It would be a vain task to attempt to count the stars in one of these *globular clusters*. They are not to be reckoned by hundreds; and on a rough calculation, grounded on the apparent intervals between them at the borders (where they are seen not projected on each other), and the angular diameter of the whole group, it would appear that many clusters of this description must contain, at least, ten or twenty thousand stars."

The resemblance between these star clusters and the nebulæ has been pointed out again and again, and they are usually regarded as allied phenomena; but there is one distinction, the great importance of which will be presently seen. I allude to their brilliancy. That of the star clusters proper is stellar or solar brilliancy, while that of the nebulæ is little more than a phosphorescent glow. It has been estimated to vary from $\frac{1}{1500}$ to $\frac{1}{20000}$ of the light of a single sperm candle, consuming 158 grains of material per hour, viewed at a distance of one quarter of a mile.

324. From this I should infer that the brilliancy of the star clusters is solar, while that of the nebulæ may be more nearly allied to that which I believe would be displayed by Saturn, if we could see it independent of reflected sunlight. If the views which I have expounded are sound, there is no broad line of distinction between suns and planets, as regards physical constitution, and we may expect to find that the heavens are peopled with every gradation of material aggregation, from meteoric dust to the mightiest of suns. In the double and multiple stars we find suns revolving about suns; in our own system we find orbs of most varying dimensions chained together by the link of universal gravitation, and arranged without any reference to their magnitude. If I am right, every star *must* have its revolving attendants, or it would cool down, contract, and cease to be a sun. The small luminosity of the nebulæ suggests this question, viz., What would result if a number of bodies similar to Jupiter, Saturn, Uranus, and Neptune, were united as a system, without any one great dominating central sun chaining their flights to concentric orbits, but all whirling among each other, and mutually and continually disturbing each other's paths, and rendering revolutions and rotations as irregular as the staggering and reeling of a drunkard.

325. For the sake of illustration I will suppose one hundred of such bodies were thus assembled, and so nearly united that,

in the contest of atmospheric appropriation, they should gravitate as a whole. They may be of very various dimensions, but I will suppose that the *mean* mass of each shall be equal to that of Jupiter. Their total mass will, therefore, be a hundred times that of Jupiter, and their total atmosphere, as measured by that of Jupiter, will be $100\sqrt{100}=1,000$ times as great. Thus each individual member of this system, having the dimensions of Jupiter, will obtain ten times as great an atmosphere as though it stood alone. If 10,000 of such bodies were thus united, each would have one hundred times as much as it could singly obtain; thus there would be an abundant accumulation of atmospheric fuel and aqueous vapour.

326. The components of an *irregular* cluster of bodies, thus constituted and thus supplied, would be not merely perforated with eddies, but riven to their very centre,—moulded and shaped throughout by the whirling hurricane of their whole substance. When in the centre of a tornado of opposing gravitations the tortured orb would be twisted bodily into a huge verticose crater, into the bowels of which the aqueous vapour would be dragged and dissociated, and then, entangled with inner matter of the riven sphere, hurled upwards again to burst forth in an explosion of such magnitude that the original body would be measurably presented as but a mere appendage—the rocket-case —of the flood of fire it had vomited forth. The atmospheric water would thus be alternately dissociated and recombined, and, when ejected far enough outwards, condensed into cloudy vapours, which would be illuminated by the flashing outbursts and the radiations from the glowing gaseous orbs and their ejected meteoric matter.

327. The distribution of this flaming, incandescent, and cloudy matter would depend upon the arrangement of planetary or semi-solar elements of the celestial compound. If these were arranged in a globular, annular, elliptical, or other aggregation having sufficient geometrical symmetry to permit the constituent orbs to move in concentric or other parallel orbits,

the mutual disturbances would be sufficiently regular and moderate to fall short of producing such wholesale ruptures as described in the last section, and thus the meteoric ejections would be mere prominences, and the cloudy matter would be arranged as a cloud-sphere around each individual planetary element. The luminosity of this vapour would of course depend upon whether the magnitude and the disturbances of the body which it enveloped were sufficient to maintain merely a glow or a decided photosphere; if the latter, the luminosity would vary with the depth of photosphere. Thus such geometrical assemblages might vary from a dull red glow, too faint for human eyes, aided even by the best telescopes to detect, up through every gradation to the brilliancy of true stars. This degree of brilliancy would be reached when the photospheric matter should be so deep that its radiations should have sufficient power to dissolve completely all its circumambient vapours into clear gases.

328. Where no such regularity of distribution existed, and the mad riot of irregular wholesale explosive mutual interference prevailed—where, instead of the outbursts of superficial prominences, we should have whole worlds explosively disembowelled, —mutual bombardments of contiguous planets would be of perpetual recurrence, each would be flinging its meteoric ejections into the sphere of its neighbour's gravitation, and their flashing meteoric matter and cloudy envelopes would be mingled, interchanged, and stretched out from one to another. The larger the number of the elements and the greater the individual and aggregate mass, the more complete would this vaporous communication become, on account of the greater proportionate share of atmospheric fuel and vapour that each would possess.

329. According to the foregoing, the geometrically arranged systems would be subject to little visible change when viewed from time to time, as the parallel movements of the unaltered elementary constituents would not sensibly modify the general figure of the combination. The irregular combinations would obviously be subject to continual irregularities. The necessity

of this is so obvious, that I need not dwell any further upon it. As parallelism of orbit is the essential feature of the regularity I refer to, the spiral variety of nebulæ must be intermediate between the utterly irregular and those which I roughly designate the geometrical nebulæ. The spiral nebulæ should accordingly present an intermediate degree of variability.

330. The luminosity of the materials of a nebulous system would thus present every degree of variation, from photospheres of varying thickness to eruptive flames of varying dimensions, and consequently varying brilliancy; from these to incandescent but not flaming gases and vapours, and to heated and reflecting solid meteoric particles and illuminated clouds of discrete vaporous matter. The latter must form the outermost vanishing fringe of the whole of every system, and in the irregular nebulæ must constitute minor fringes to the streaks of ejection-flashes, and a body of thin mist uniting the whole. In the more distant nebulæ the brighter portions only would be visible, but every additional effort of telescopic grasp and concentration of their rays would reveal a fresh gradation of the fainter lights.

331. The necessity of completing this overgrowing essay prevents me from citing the observed verifications of this hastily drawn sketch of the theoretically required phenomena of the nebulæ, all of which follow if my general hypothesis is applied to the case of mutually gravitating clusters of such bodies as our lighter planets. The only special assumption involved is, that such aggregations of such planetary or semi-stellar bodies exist;—a very moderate assumption considering the closely sustaining analogies which the heavenly mechanism so abundantly affords. The telescopic verifications I allude to will at once present themselves to the minds of every reader who is conversant with the details of astronomical phenomena; and I refer those readers who are not, to any of the standard astronomical works wherein the phenomena of the nebulæ are treated at any length.

332. As a correction to one portion of the information that will be derived from all but the most recently revised astronomical treatises, I may mention that the hypothesis which regarded the nebulæ as outlying systems of stars existing in remote regions, far beyond our milky way, is now pretty generally abandoned, and that it has received a final blow from the spectroscopic researches of Mr. Huggins. These show that the nebulæ are composed of gaseous matter; that this is the case even with the brightest stellar points of the resolvable nebulæ. He even goes further, and finds that some of the groups called "star clusters" are composed of bodies which give a spectrum of bright lines, and whose luminosity, therefore, comes from gaseous matter, less heated and less dense than our solar photosphere. The particular gases which he has recognized in all the nebulæ are nitrogen and hydrogen; and besides the bright lines of these gases, another, due to some gaseous body which is not at present understood. The spectra of the nebulæ differ from the star spectra in precisely the same manner as the spectra of the solar prominences differ from the spectra of the solar disc.

333. The explanation of the mysterious phenomena of the flashing and variable stars comes so obviously within the grasp of my general hypothesis, that I need scarcely expound its application to them. Given a system corresponding to the binary or more complex star systems, or a solar system so heavily laden with planets that the central sun shall be made to rotate about a common centre of gravity, situated somewhere about its own circumference, and all the observed phenomena must result. When Mr. Huggins applied his spectroscope to one of the most remarkable of these, he found that its spectrum indicated a great conflagration of hydrogen, similar to the solar prominences; that this outburst was, in fact, a monstrous stellar prominence.

334. An irregular cluster of suns, sufficiently near to each other to mutually gravitate with sufficient force to convert each

member into a flashing star, like τ, *coronæ*, would probably become a nebula. To understand what I mean, the reader must remember what I have said about the gaseous constitution of the sun (sect. 131 to 136), and consider that, in accordance with this, even the solid or viscous nucleus must become gaseous by the expansive force of its own heat, if the pressure now condensing it were removed. If the components of a system of such intensely heated *gaseous* bodies were whirled, distorted, riven, and disembowelled in the manner described in sect. 327, they would probably be separated into smaller bodies, as a mass of falling water is broken into drops by atmospheric resistance. That a number of stars should thus fall together, in such a manner that their mutual gravitation should drag them so nearly into collision as to result in spiral and irregular orbits about each other, is by no means improbable.

335. Let us imagine, for example, that a densely aggregated portion of the milky way, including a thousand stars, or say the Pleiades, were carried quite away from the galaxy of surrounding suns, and without any alteration of their grouping, were placed in a vacant region of space, where no appreciable outside force of other gravitating bodies could affect them. They would immediately become subject only to their own mutual gravitations; they would rush towards each other, but not in direct collision, for no two could act upon each other without the diverging influence of the attraction of all the rest, and thus a set of complex orbits would be established. They would move around a common centre, but with such complex and wholesale interferences, that the figures of their orbits would lose all visible regularity. Ultimately, by a series of actions similar to those I have described (sect. 216), they might settle into concentric parallel orbits, and become globular, annular, or elliptical clusters, but, in the meantime, would more or less completely fulfil my description of a nebula.

336. This explanation of the possible origin of some of the nebulæ, and even of solar systems, is suggested mainly by the

remarkable fact that nebulæ are usually found in comparatively barren or starless regions of space, as though a stellar concentration had occurred, and left a vacant space around. One of our great astronomers (Sir William Herschel, I believe), when sweeping the skies with his telescope in search of nebulæ, was accustomed to tell his amanuensis to "prepare to write, nebulæ are coming," whenever he reached a starless region. The facts connected with the disappearance and re-appearance of nebulæ, and the observations of Mr. Pogson, Professor Luther, and M. Anwers, of a nebulæ apparently changing into a variable star,* have a very interesting and direct bearing upon this view of the possible origin of some of the nebulæ. I must, however, refrain at present from further speculation or remark on this subject.

* Herschel's *Outlines of Astronomy*, Note K.

CHAPTER XXI.

CONCLUSION.

DO MY HYPOTHESES EXPLAIN THE CONNECTION BETWEEN SUN-SPOTS AND TERRESTRIAL MAGNETIC DISTURBANCES? ETC.—AMOUNT OF ELECTRICAL DISTURBANCE DUE TO SOLAR EJECTIONS—COMETS—AN EXPLANATION OF MY PREFACE—WHY I HAVE VENTURED BEYOND MY DEPTH WITHOUT FEAR OF DROWNING.

337. Before concluding I must reply to one question which forces itself irresistibly forward in connection with the general subject of this volume. It is this. Does my explanation of the solar phenomena throw any light upon the mysterious connection that is observed to exist between the aurora borealis, terrestrial magnetic disturbances, and solar outbreaks? It should do so if at all complete. A reference to sections 182 and 183 will, I think, show that it does. The electrical disturbances there indicated could not occur without producing inductive polarization of the dielectric matter extending between the sun and the earth, which would be displayed as electric disturbance upon the earth. That the action is thus *induced,* and not *conveyed,* by the outflying particles is evident from the phenomena described in section 196, which are but typical of other observations. *"The electrical influence travelled with the light."*

338. If the ejection of a mixture of steam and particles of water from a boiler a few feet in diameter (such as was exhibited a few years since at the Polytechnic Institution of London) could produce the mimic lightning thereby displayed, how marvellous must be the electric convulsions produced by the

monster outbursts of the sun? The magnitude of these electric disturbances must be comparable with that of the solar heat and solar light, and, like the heat and light, their radiance must reach the earth and sensibly affect it. But this is a large subject that I must not attempt to treat while hurrying thus to close this volume, for it includes the ordinary as well as the extraordinary phenomena of terrestrial magnetism.

339. I intended to have applied the hypothesis of an universal atmosphere to the explanation of the phenomena of comets, upon which I think it throws much light, but my notes on this subject are too extensive to be brought within this overgrown Essay. I therefore throw the whole of this subject over to the supplementary Essay which I shall complete and publish as early as possible.

340. In the preface to this volume I expressed a fear that I may have fallen into unseen mistakes, and a request that if so, the errors may be dealt with in a philosophic rather than a querulous spirit. In writing this I was not merely phrase-making in obedience to conventional forms of humility-humbug, but was expressing a fear which the reader who has travelled through the preceding pages, will readily understand, as he will doubtless have detected that I am not a mathematician, and have only that amount of general knowledge of Astronomy which every full-grown human being ought to possess. I may add, that this little knowledge having of necessity been acquired by self-teaching, there may be gaps in its continuity unseen by myself—some of those chasms of misconception into which the self-taught student is so liable to fall. But then it will perhaps be said that I had no right to venture thus beyond the domain of my own especial studies. This I stoutly deny for the following reasons.

341. There is no man old enough to be an expert in all the sciences, and yet all the sciences are but one science, and all our subdivisions are merely artificial devices for the convenience of study. Hence if every man confined himself to his own

particular branch of special knowledge, the divine unity of Creation would remain unknown, and the highest object of all science,—the uplifting and purification of the human mind by the unselfish contemplation of the marvellous harmonies of the universe, would be unfulfilled. The new-born science of celestial chemistry could not have come into existence without the previous wedding of the laboratory to the observatory; and if we take a general survey of the progress of human knowledge during the present generation, it will be seen that the greatest strides have been made by those who have boldly stepped across the conventional boundaries that mark the customary subdivisions of the sciences. No man can thus step out of his own particular path without some risk of tripping. To the self-inflated pedant who gabbles technicalities and professes intellectual infallibility, such a false step would be fatal if witnessed by the world, but the true philosopher may trip or stumble, recover himself, and go on without abasement; his object being not to display his own paces, but to travel onward in the road of truth. Unless then I am one of the pedants, I risk nothing in launching thus boldly my first philosophical venture; for if it fails, its refutation must teach me much that I require to know, and if it succeeds, it will teach something to others.