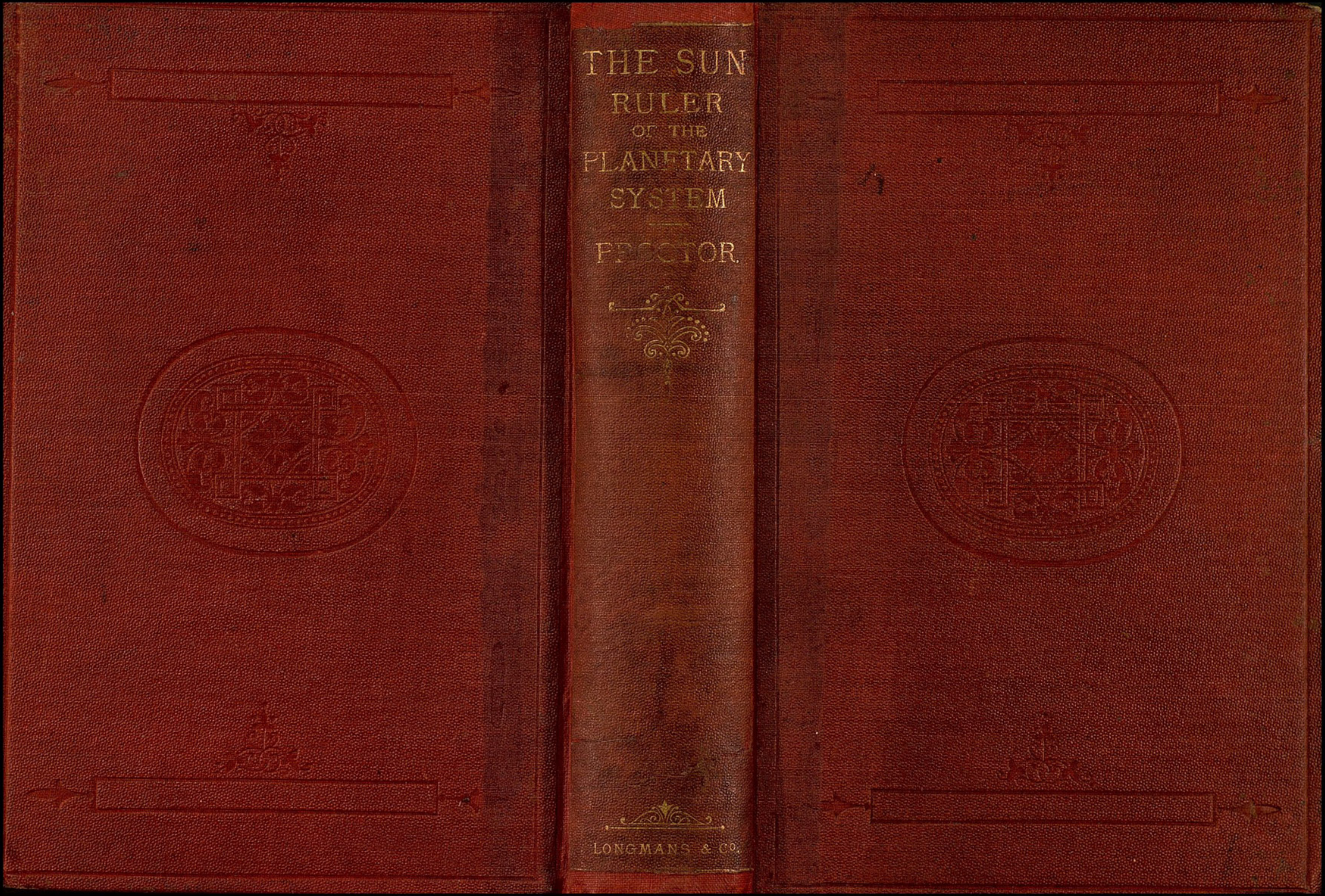


# THE SUN
# RULER
# OF THE
# PLANETARY
# SYSTEM

PROCTOR.

LONGMANS & Co.

~~90~~

~~660~~

A. K. Bulwer

30

# THE SUN.

LONDON: PRINTED BY
SPOTTISWOODE AND CO., NEW-STREET SQUARE
AND PARLIAMENT STREET

THE SUN.

as seen by the author on September 25, 1870.

WITH A TELESCOPE 2¼ INCHES IN APERTURE,
AND A POWER OF ONLY 26 DIAMETERS.

M&N HANHART. IMP.

# THE SUN:

## RULER, FIRE, LIGHT, AND LIFE OF THE PLANETARY SYSTEM.

BY

RICHARD A. PROCTOR, B.A. F.R.A.S.

AUTHOR OF 'OTHER WORLDS THAN OURS,'
'SATURN AND ITS SYSTEM,'
ETC.

---

Beyond expression bright
Compared with aught on earth, metal or stone;
Not all parts like, but all alike inform'd
With radiant light, as glowing iron with fire....
* * * *
What wonder then if fields and regions there
Breathe forth Elixir pure, and rivers run
Potable gold, when with one virtuous touch
Th' arch-chemic Sun, so far from us remote,
Produces, with terrestrial humour mix'd,
Here in the dark so many precious things?
MILTON.

---

*WITH TEN LITHOGRAPHIC PLATES (SEVEN COLOURED) AND ONE HUNDRED AND SEVEN DRAWINGS ON WOOD.*

LONDON:
LONGMANS, GREEN, AND CO.
1871.

# PREFACE.

WHEN I had completed my treatise on Saturn and its System, I formed the design of preparing a separate treatise on each of the planets Mars and Jupiter, and then another and larger treatise on the Sun. Circumstances, which it is needless to particularize, prevented me from carrying out this design at that time, and indeed threatened to withdraw my attention altogether from scientific pursuits. That my plans, though delayed, have not been lost sight of during the last four years, is evidenced by the appearance of many papers of mine on Mars, Jupiter, and the Sun, in several quarterly, monthly, and weekly journals. These, if collected, would of themselves suffice to form volumes of no inconsiderable dimensions on these several orbs; while my 'Other Worlds than Ours' presents a sort of summary of my researches on these and other astronomical subjects. But it is only quite recently that I have been able to resume my original design.

The delay has not been without its advantages, however. A work on the Sun has at the present time a far greater interest than it would have had four years since; while I have been able to obtain a much wider and more complete view of the subject than I should probably have thought necessary had I completed the work at that time.

My primary object in the present volume has been to furnish a full account of the remarkable discoveries which have been effected by observers of the Sun, whether by means of the telescope, the spectroscope, polariscopic analysis, or photography. It will be seen that Chapters IV., V., and VI., in which I deal with these discoveries, together constitute more than one half of the main text. In these chapters the labours of the Herschels, Schwabe, Carrington, Secchi, De la Rue, Stewart, and others in examining the solar surface; the later observations of Huggins, Zöllner, Respighi, Secchi, Lockyer, Young, and others in the study of the prominences and chromosphere; and the observations which have been made during the past two centuries on the phenomena presented by the solar corona, have been dealt with at considerable length.

But it seemed desirable that a separate and complete explanation should be given of all those matters which specially appertain to the application of spectroscopic analysis to the study of solar physics. Without an account of these matters, many of the most interesting discoveries made in recent times would be almost unintelligible, and it did not seem fitting to refer the general reader to the valuable but costly works of Roscoe and Schellen. Further, I think such a mode of treating spectroscopic analysis as I have adopted in

Chapter III. of this work, more likely to be of use to the reader than a fuller but less simple account. In one respect, indeed, Chapter III. presents what is wanting in every treatise on the analysis which I have hitherto seen: the matter in pp. 128–156 exhibits what really happens when the light from the object studied is sent through a battery of prisms. In Chapter III. I give an account of the principles of Browning's automatic spectroscope, and exhibit a plan of my own by which this principle may be extended so as to include a second battery. I think that in future applications of spectroscopic analysis, the plan illustrated at p. 139 is likely to be found of considerable utility.

Another large section of the book is devoted to the question of the Sun's distance. In Chapter I. will be found a very full account (the fullest popular account yet published, I believe) of the researches which have been made up to the present time into this subject: while in Appendix A the transits of 1874 and 1882 (already attracting much notice) are dealt with at length, and the best means for observing them effectively are fully considered. The subject is one to which I have given much attention. In constructing the pictures from which Plates IX. and X. have been reduced (by photolithography), every circumstance of the transit of 1874 was taken most carefully into account; and I think that I may safely say of these views, the four which accompanied them in Vol. XXIX. of the 'Astronomical Society's Notices,' and Plate VIII., that they are far the most accurate graphic representations that have yet been made of this or any other transit. The views on pp. 446, 447, though small, illustrate the transit of 1882 more accurately than any views yet made, so far as I know.

The note on pp. 443–445 serves to indicate the circumstances under which my investigation of the transits of 1874 and 1882 led me to indicate my dissent from the views which the Astronomer Royal had propounded on the same subject. It was not easy—or rather it was not possible—for me to exhibit my results without pointing out how and why they differed from Professor Airy's, which had been for so many years before the public. The accuracy of my results has now been established, and has indeed never been doubted. The only question which remains at issue is whether I was right or wrong in regarding the corrections as important. While expressing my own unchanged conviction that the corrections are of vital importance, I am quite content to let others decide whether this conviction is well or ill-founded.

In Chapter II. a detailed account is given of the Sun's influence as ruler over the system of planets. Many of the relations here dealt with are novel and I think interesting. In the last three chapters I deal with the Sun's physical condition, his position as fire, life, and light of the solar system, and his place and motions among his fellow-suns. It will be seen that in Chapter VIII. I have presented reasons for considering that the most important work science has to accomplish is to show how the Sun's action can be more fully utilized than it is at present, so that before the Earth's stores of force are exhausted (as they must some day be), resources which can never be exhausted —because unceasingly restored—may be rendered available.

The treatment of the subject of eclipses in Appendix B is novel. It seemed to me that there was room for a more thorough explanation of the general laws on which the recurrence of eclipses depends, than finds place in our text-books of astronomy*—the authors of

---

* In this connection, it is necessary for me to note that I have had occasion to claim in fig. 8, p. 21, and foot-note, pp. 25, 26, a drawing which appears at p. 185 of Mr. Lockyer's *Elementary Lessons in Astronomy*. This drawing, and other matter which he has paid me the compliment of employing in his useful little book, may be of small importance, and it would be absurd to suppose that in a text-book no materials should thus be borrowed. But it has been convenient to me here—and may be convenient again—to make use of my own work; and I would rather not appear to borrow (at least without proper acknowledgment) from Mr. Lockyer. This feeling seems so natural, that it has been with surprise I have found Mr. Lockyer objects to it. In a letter to Messrs. Macmillan, *for* me, he has expressed a somewhat angry satisfaction that though he has borrowed in the same work and way from many others, no one has remarked upon it but myself. (A mistake, however.) My remarks were sufficiently gentle, and all I asked was that 'by associating my name with my own results (some of which had only been obtained after much labour) he would place it in my power to employ those results.' For, else, I should seem to be acting unfairly towards him, by using his work without that acknowledgment which justice and courtesy alike require in such cases. I certainly had no thought of angering him, and it has been with sincere regret that, since then, I have seen his anger leading him to say and do many things which I am sure he will regret in the cooler after-years.

which have been content for many years to follow each other, as respects this matter, along a somewhat unsatisfactory track.

It only remains for me to point out that in some of the notes I deal with matters and employ methods of treatment which would not be suitable for the main text. The general reader can omit these notes altogether, as they are not necessary for the elucidation of the subject and have only been introduced for the benefit of those more advanced students of astronomy who might desire to see certain points more thoroughly dealt with than they could be in the body of a popular treatise like the present.

RICHARD A. PROCTOR.

LONDON: *December* 1870.

# CONTENTS.

## CHAPTER I.

### THE SUN'S DISTANCE AND DIMENSIONS.

## CHAPTER II.

### THE SUN AS RULER.

## CHAPTER III.

### ANALYSING SUNLIGHT.

## CHAPTER IV.

### STUDY OF THE SUN'S SURFACE.

## CHAPTER V.

### THE PROMINENCES AND THE CHROMOSPHERE.

## CHAPTER VI.

### THE CORONA AND ZODIACAL LIGHT.

## CHAPTER VII.

### PHYSICAL CONDITION OF THE SUN.

## CHAPTER VIII.

### THE SUN—OUR FIRE, LIGHT, AND LIFE.

## CHAPTER IX.

### THE SUN AMONG HIS PEERS.

## APPENDIX A.

### APPROACHING TRANSITS OF VENUS AND THE BEST MEANS FOR OBSERVING THEM.

## APPENDIX B.

### ECLIPSES.

## TABLE I.

## TABLE II.

# ILLUSTRATIONS.

## PLATES.

## WOODCUTS.

# THE SUN.

## INTRODUCTION.

It would be difficult to form an idea of the length of time during which the phenomena of day and night, and of the varying seasons, transpired without attracting men's attention to the orb which governs both the day and the year. That the science now called Astronomy had its origin in the consideration of the Sun's apparent motions we can scarcely doubt. Clear indications remain, indeed, that the earliest efforts of men to determine the motions of the celestial bodies were directed to the great centre and ruler of the planetary scheme. But when those efforts were first made, what were the first conceptions of astronomers as to the nature of the solar motions, and how, in the process of time, those conceptions assumed the form described in the earliest records of astronomical research, we probably shall never know.

Nor, so far as my purpose in these pages is concerned, is it of any great importance that the truth in these matters should be ascertained. It would doubtless form an interesting subject of study to trace the first progress of men as they endeavoured to elucidate the secrets of the heavens. Even what we know of the early researches of astronomers is full of interest, and not wanting in instruction. Their failures as well as their successes teach us a useful lesson of patience and of perseverance. The confidence with which at times they insisted on adopting erroneous theories may serve to teach us a lesson of modesty and caution. And the gradual process by which observation, and thoughtful reasoning upon observation, led men to the successful solution of so many noble problems, is as full of interest to the thoughtful student as the most spirit-stirring scenes of history. But where, as in the present instance, it is the object of the writer to exhibit the clearest possible picture of what is, it may be gravely questioned whether it is wise to present the full history of a series of researches which proceeded often on erroneous hypotheses. It is sufficiently difficult to convey in the compass of a single volume clear and accurate conceptions of a wide astronomical subject; and the task is rendered much more difficult where the history of false or imperfect theories is mixed up with the account of recognised truths.

But there is yet another reason for not undertaking to give in this work a history of the progress by which men attained their present conceptions respecting the Sun, nor even attempting the far easier task of showing how, by a series of simple observations, the position of the Sun in the solar system, and especially with reference to our Earth, might have been ascertained. The fact is, that such an undertaking would differ but little from an attempt to combine a complete history of astronomy—or at the least a complete discussion of all known astronomical relations—with that special discussion of the Sun's nature and condition which forms the essential object of this treatise. For there is not a single chapter of a treatise on general astronomy which is not more or less associated with the relations presented by the Sun; and it may even be said that there is not a single subject dealt with by astronomers which does not owe its chief interest to such an association.

Therefore I find myself compelled to forego that mode of treating my subject which had seemed the best when I was dealing with the planet Saturn. One can, without prolixity, discuss the gradual progress of research by which the relations presented by a single planet have been, or might be, ascertained. But a preliminary research of this sort would require, in the case of the Sun, a volume—and no small one—to itself.

The course I propose, therefore, to adopt is as follows:—I shall pass over all that portion of the history of astronomy which relates to the determination of the Sun's central position in the system he governs. I shall give no account of the methods by which the nature of the Sun's diurnal motions were determined, nor shall I show how from this knowledge, combined with the gradual survey of the terrestrial globe and the recognition of the Sun's apparent annual progress around the sidereal heavens, the ancients recognised the fact that either the Sun travels yearly around the Earth, and is carried also daily round with the heavens, or else that this Earth on which we live speeds yearly around the Sun, rotating daily on her axis. The observations on the planets by which the true interpretation of these apparent motions of the Sun was eventually obtained must also remain undiscussed.

We are to begin, then, by regarding the Sun as the recognised centre of the solar system, ruler over a scheme of worlds, on which he pours forth abundant supplies of heat and light.

So regarding the Sun, we shall first be led to inquire into the distance of the great luminary, in order that we may determine his real dimensions. The fundamental problem of astronomy—the determination of the Sun's distance—a problem which has at the present time a special interest on account of those approaching transits of Venus from the observation of which astronomers hope to obtain new and better measures—will therefore form the subject of the opening chapter of this work. This chapter will naturally include the consideration of the Sun's dimensions, and of the scale of the solar system generally.

Then, next, the question of the Sun's mass, and of the influences which he exerts by reason of his mass, will come to be considered. We shall measure the might of the giant which rules the whole family of planets, and consider the limits of his domain. We shall inquire what motions it is in his power to control at this or that distance, and so determine the limits of his power to gather fresh materials from out the surrounding spaces, either in such sort as to recruit his own mass, or as to enlarge the crowd of relatively minute bodies which circle continually, as we know, around him.

Then we shall proceed to discuss what the spectroscope has taught respecting the actual materials which constitute the Sun's substance, placing this inquiry before those chapters which deal with the telescopic aspect of the Sun's surface, the prominences, and the corona, in order that there may be no break in the narrative in passing from the era of unaided telescopic research to the recent era of mixed telescopic, spectroscopic, photographic, and polariscopic observation. In other words, an account of the principles of spectroscopic research, including so much of the history of spectroscopic analysis as is sufficient to make the subject clear (and therefore necessarily including an account of the Sun's general structure as indicated by the solar spectrum), will precede the narrative of that long series of researches which commenced with the discovery of the solar spots. The chapter on spectroscopic analysis will close with an explanation of those special modes of spectroscopic research which have seemed unintelligible, or rather incredible, to many, but yet depend on principles of exceeding simplicity.

Then will follow chapters describing the discoveries which have been made by aid of the telescope—and its allies, the spectroscope, the polariscope, and photography—respecting the aspect and general condition of the Sun's photosphere, the coloured prominences, the corona, and so on. The physical condition of the Sun, the amount and probable source of his heat, and other like questions, will next be dealt with. And, finally, we shall consider the system of suns, and the position which our Sun holds in that system, so far as the researches yet made by astronomers enable us to deal with this noblest of all subjects of study.

Thus we have before us a sufficiently wide range of research. Our progress will lead us to consider some of the most successful attempts yet made by man to resolve the mysteries of the universe. We shall have to deal with much that invites reflection and speculation—with much that may be explained by the thoughtful study of evidence already obtained;—but also with much that continues, and may perchance continue for many years, altogether perplexing. We have, in fine, to deal with a subject which is full of interest, but whose real grandeur and significance, as well as its vast difficulties, are but now beginning to be rightly understood.

# CHAPTER I.

## *THE SUN'S DISTANCE AND DIMENSIONS.*

THE DETERMINATION of the Sun's distance is not only an important problem of general astronomy, but, so far as the subject of this treatise is concerned, it may be regarded as the very foundation of all our researches. For until we know the Sun's distance we can determine neither his bulk nor his weight; and our views even as to his physical condition will be found to depend in an important degree on the estimate we form respecting those two elements. A minute error in the solution of the problems on which the determination of the Sun's distance depends would not only result in adding or withdrawing hundreds of millions of cubic miles from the Sun's volume, and many multiples of the Earth's mass from his weight, but our conceptions of the size of solar spots, the height of the coloured prominences, the velocity of solar currents and cyclones, and many other such matters, would be rendered proportionately erroneous. It is, therefore, of the utmost importance that we should have accurate views respecting the modes of research by which the Sun's distance has been estimated, and that we should know the probable limits of error in the resulting determinations. The subject has a special interest at the present time, because preparations are even now being made for the application, in the winter of 1874, of one of the most effective methods at the disposal of astronomers for the solution of this most noble problem.*

Let us consider the general nature of the problem, in order that we may the better appreciate its difficulties. Many are disposed to wonder that astronomers should not long since have mastered what seems to be an almost elementary problem of the science; and it has appeared as a blot on the fair fame of astronomy that errors and differences of millions of miles should have followed the attempt to solve this particular problem. Then, too, when the resulting errors in the determination of the distances of the outer planets, and the still larger errors in the determination of the distances at which the fixed stars lie from us, are considered, the inquiry is suggested, 'Where is the boasted accuracy of the most exact of the sciences?' We shall see as we proceed that the great wonder is, not that the estimates made by astronomers should differ, but that even the vaguest ideas should have been formed respecting the Sun's distance. The problem, as presented in its simplicity (the simplicity of perfect difficulty),

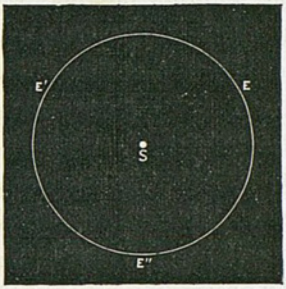
---

* See Appendix A, for an account of what is anticipated from the observations to be made on the transits of 1874 and 1882.

seems at first sight one that no human ingenuity could avail to solve.

In fig. 1 let S represent the Sun, and E E′ E″ the orbit of the Earth. Then on the scale of the figure, a dot meant to represent the Earth should have dimensions so minute that it would be altogether invisible to the naked eye. The line representing the Earth's path is broad enough to obliterate more than a hundred such dots placed side by side across its width. Now, it is the inhabitants of the globe represented by this tiny dot, who have to measure the distance separating the inaccessible globe S from E E′ E.″ The globe on which they live is continually rotating on its axis, it is sweeping onward with inconceivable velocity on the path E E′ E″, yet from this rotating, onward-rushing, and relatively minute orb, the observations are to be made by means of which the vast distance of S has to be determined. Still considering fig. 1, let us, in yet another way, picture to ourselves the nature of the problem to be dealt with.

FIG. 1.



Suppose the Earth on the orbit E E′ E″ to be represented by a globe one inch in diameter.* Then the Sun at S would be represented by a globe 9 feet in diameter, and his distance from the orbit E E′ E″ by no less than 320 yards. Let any one who has noticed the 300 yards' range in rifle shooting consider how minute a disc one inch in diameter would appear at that distance, and he will at once recognise how difficult a problem the astronomer has to solve in determining the Sun's distance; for that minute and scarcely perceptible disc subtends the very angle on whose exact measurement the solution of this problem depends. But even when any one has pictured to himself the difficulty of determining the exact angle subtended by an inch disc at that distance, and how easily the angle might be over-estimated or under-estimated by a considerable fraction of its real value,—even then he will not have realised the actual difficulty of the problem. Let him reverse the illustration, and picture the difficulty of determining by observations made from points within this inch disc the distance of a station 320 yards off. Yet *even then* he will have underrated the difficulty of the actual problem astronomers have to solve. He must suppose the two sets of observations to be made by different observers, at different seasons, in different weather, with different instruments; that each set of

---

* It is convenient to remember that a bronze halfpenny is exactly one inch in diameter; so that an exact representation of a great circle of the earth, on the scale we are now considering, can be conveniently referred to.

bservations has to be corrected for a wholly different series of conditions; that each observer's station is shifted continuously by two distinct forms of motion, which must both be taken into account (involving a careful reference to the question of time) before any satisfactory use can be made of the observed results.

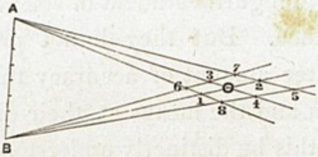
Such, in a general sense, is the nature of the problem astronomers have to deal with. The conditions of the problem are not of their fixing; all they can do is to face as resolutely and skilfully as they can the difficulties which the problem presents to them. They have done this so well that the history of the problem has become to the thoughtful student of science as interesting as a romance. But they do not pretend to have secured a greater amount of accuracy than the nature of the problem and the means at their disposal render possible. Let this be distinctly understood beforehand,—*Absolute accuracy in the solution of this problem is simply out of the question.*

And now let us consider how the problem is to be attacked. In reality no less than six methods have been successfully applied; but there is only one method which is strictly geometrical in its nature, and this method must be the first to engage our attention.

In determining the distance of an inaccessible object, the geometer first measures a base-line, and then observes the bearing of the object from either end of that line. He thus has the means of determining the distance of the object with an exactness proportional to the accuracy of his observations.

Suppose, for example, the object is at O (fig. 2), and that the observer measures the base-line A B and observes the bearings B O and A O. Then, if he has done this accurately, he can draw a picture, as in the figure, accurately representing his observations, and he can measure either A O or B O in this picture, and, by comparing these measurements with the length of A B in his picture, he can tell what relation the actual distances A O or B O bear to the actual base-line A B.

FIG. 2.



Thus, if his base-line A B is 800 feet long, so that each of the divisions in the figure represents a length of 100 feet, then if B O be found to contain fifteen of these divisions, he will know that O is 1,500 feet from B. Usually, however, instead of merely drawing a figure (a process in which errors may creep in through the imperfection of rulers, compasses, and so on), the surveyor would apply trigonometrical calculation to determine B O or A O as accurately as his observations permitted.

But now, if either his base-line or his bearings be wrongly determined, it is clear the distances A O or B O will be wrongly estimated from them. The effect of a wrong measurement of the base-line is too obvious to need special discussion: clearly the error of B O or A O will be precisely proportional to the error of A B. But the error resulting from wrong estimates of the *bearings* requires to be attentively considered.

Suppose the bearing A O wrongly observed, and placed as A 1 or A 2. Then if the bearing B O be correctly observed, the resulting error will be O 1 or O 2 respectively. On the other hand, if the bearing A O be correctly observed but B O misplaced as B 3 or B 4, the error will be O 3 or O 4 respectively. If the bearings A O and B O be both misplaced outside, or both inside, the true direction of O, the place of the point O will be calculated as if at 5 or 6 respectively. And, finally, if the bearings are misplaced in different ways—that is, one inside O and the other outside—the point O will be calculated as if at 7 or 8, respectively.

Now, under favourable conditions, a skilful observer, though he must needs make *some* error in estimating his bearings (for no instruments can be absolutely perfect), would yet bring the estimated point relatively very near to O; in other words, though he might set it at a point out of place in the same way as any of the points 1, 2, 3, … 8, the *area of error* corresponding to the area 5 7 6 8 would be small compared with the area A O B.

But now suppose that instead of such a triangle as A O B, our observer has to deal with a triangle shaped like *a o b* in the next figure—an *ill-conditioned* triangle, to use an expression of Sir John

Herschel's. It is at once seen that a very small error in either of his bearings will set the observer far wrong in his estimate of the distance of *o*. Suppose he has rightly determined the position of *a o*, but has the bearing *b o′* or *b o″* in place of the true bearing *b o*. He

FIG. 3.

has the large error *o o′* or *o o″*, instead of the relatively small error o 4 or o 3 in the case pictured in fig. 2.

Now, the first important astronomical problem in distance-measuring—a problem infinitely less difficult than that of determining the Sun's distance—involves this very difficulty to a degree far greater than is indicated in fig. 3. I refer to the measurement of the Moon's distance.

If E (fig. 4) represent the Earth, the Moon would be placed somewhat as at M, and if it were possible to make use of two observatories situated as at *a* and *b* at opposite extremities of a diameter of the Earth, the

FIG. 4.

actual difference of bearing of the Moon would be represented by the small angle *a* M *b*. As a matter of fact, however, even this small angle has to be reduced considerably, because from *a* or *b*, the Moon would be on the horizon, and the estimate of her position rendered unsatisfactory by atmospheric refraction. The angle $a$ M $b$ is about a degree and a quarter, and it affords a very satisfactory idea of the skill with which ancient astronomers employed their relatively ineffective instrumental means, that their estimate of the Moon's distance differed from the truth by only a fiftieth part. Modern astronomy has so completely mastered the problem of determining the Moon's distance, that the estimate now adopted can scarcely exceed or fall short of the truth by so much as twenty miles, or less than a ten-thousandth part of the whole.

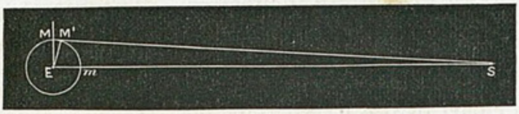
But when the method thus shown to be available in the case of the Moon is applied to the Sun, it is found to be absolutely ineffective. The nicest observation fails to show any measurable difference in the Sun's position according as he is viewed from one or another part of the Earth's surface. It is true that there *is* a difference, and indeed a difference which is large compared with some quantities which astronomers are in the habit of dealing with; but as a means of estimating the Sun's distance, this direct reference to what is called parallactic displacement may be regarded as wholly ineffective.

Other methods, then, must be adopted. I proceed to consider two methods which suggested themselves to ancient astronomers. It is interesting to consider even those attempts which have failed; for they show the real difficulty of the problem we are engaged upon.

It occurred to Aristarchus of Samos (who flourished some twenty centuries ago) that the illumination of the Moon by the Sun affords a means of estimating the Sun's distance.

If *m* M′ M (fig. 5) represent the Moon's path about the Earth E, and S be the place of the Sun, we know that the Moon is half full when near M. But clearly it is not when the Moon has reached the point M, such that M E *m* is a right angle, that she is exactly half full, but when she is at the point M′, such that E M′ S is a right angle. If, then, we can only determine the arc *m* M′, or find out how soon after new moon the Moon appears exactly half full, we can tell in what proportion the distance of the Sun exceeds the Moon's distance; for in that case we have the angle M′ E S as well as the right angle at M′, and thus the shape of the triangle E M′ S is assigned, and with it the proportion of E S to E M′, which is what we require.

Fig. 5.

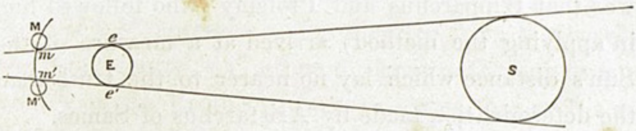


Let us pause to notice the ingenuity of this method. The point to be determined is, in reality, the distance between M and M′, or the angle M E M′, which is the same as the angle at S. In other words, instead of estimating the angle which the Earth's radius subtends as seen from S, this plan requires that we should determine the angle which the Moon's distance subtends as seen from S;—a much easier problem, first, because the latter angle is 60 times as great as the former, and secondly, because the necessary observations can be made at one terrestrial station.

Ingenious as the plan was, however, it was totally inadequate to meet the real (but as yet unknown) difficulty of the problem. Aristarchus estimated the Sun's distance E S at nineteen times the Moon's, or (roughly) at a twentieth of its true value.

However, we should perhaps regard the estimate by Aristarchus as corresponding to those modern estimates of certain stellar distances, regarding which astronomers only say that they do not fall short of a certain value, without claiming to know how far they may exceed it.

FIG. 6.



The next plan of attack was devised by Hipparchus.

Let M (fig. 6) represent the Moon just entering the shadow of the Earth E, S being the Sun. It is clear that if the Sun were just as large as the Earth, the shadow's width *m m'* would be exactly equal to the Earth's diameter. If the Sun were less than the Earth, the shadow at *m m'* would be wider than this; and if the Sun were greater than the Earth, the shadow at *m m'* would be narrower than the Earth's diameter. Hipparchus reasoned that if *m m'* is known, then by combining this measure with our knowledge of the Moon's distance and the Sun's apparent diameter, we can determine the Sun's distance.*

This method, like the former, was exceedingly ingenious, because it promised to enable a single observer, by merely timing the duration of a lunar eclipse, to solve a problem which, attacked directly, requires very delicate observations, made at stations very far apart.

Again, however, the as yet unknown vastness of the Sun's distance foiled the ingenuity of astronomers. We now know that the plan just described is utterly inadequate; and we can readily understand how it was that Hipparchus and Ptolemy (who followed him in applying the method) arrived at a measure of the Sun's distance which lay no nearer to the truth than the determination made by Aristarchus of Samos.

Thus it came to pass that until the time of Tycho Brahe the received estimate of the Sun's distance was no greater than five millions of miles; nor is it too much to say that the methods applied by Aristarchus and Hipparchus might equally well have given any result whatever, from a million miles to infinity. In other words, the limits of error by these methods, and with the means available to ancient astronomers, actually exceed the quantity to be determined.

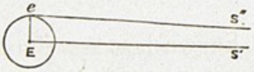
We come now, however, to the methods belonging

---

* We can determine at once the angle included between $e\,m$ and $e'\,m'$; it is easily seen that the angle subtended by the Sun's semi-diameter exceeds this angle by twice the Sun's horizontal parallax.

to modern astronomy. Before dealing with them it will be convenient to indicate the quantity which—instead of the distance—is the *direct* object of the researches to be described. Of course the distance is what astronomers really require; but this distance is determined (at least as far as direct surveying methods are concerned) by the measurement of the angle between lines directed towards the Sun's centre from different parts of the Earth. For convenience, one of the points is taken to be at the Earth's centre as E (fig. 7). Now, if E S′ represent a line directed from E towards the Sun's centre, *e* S″ a line directed to the Sun from a point *e* on the Earth's surface, so placed that *e* S″ is an horizon-line (that is, square to E *e*), then the angle between the lines E S′ and *e* S″ is called the Sun's horizontal *parallax*,* and this is the quantity which astronomers set themselves to determine in the first place. Of course, the distance of the Sun becomes known so soon as this angle is determined; and

FIG. 7.



---

* As the Earth is not a perfect sphere, horizontal parallax is different in different places. Further, the Earth's path is eccentric, and so there is a variation depending on her position in her orbit. To secure uniformity, the results obtained by astronomers are always referred to the horizontal parallax of the Sun at his mean distance and for a place on the Earth's equator—or the *mean equatorial horizontal solar parallax*, as it is called. It may perhaps be useful to remind the reader that this expression means merely the apparent length of half the greater diameter of the Earth's disc as seen from the Sun (at his mean distance).

throughout the remainder of this chapter, besides mentioning the parallax deduced by each method, I shall always mention the corresponding distance.

Six several methods have been devised, each at least as ingenious as the methods of Aristarchus and Hipparchus, and each requiring an exactness of observation which would have seemed to the old astronomers altogether hopeless of attainment.

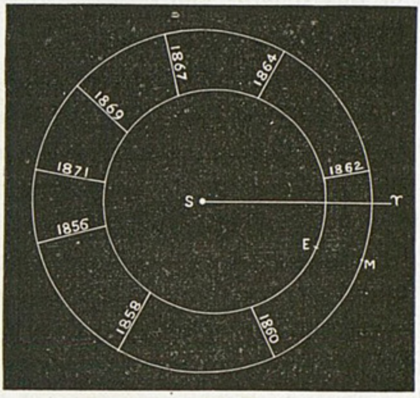
The first two methods to be described are intimately associated with Kepler's laws of the planetary distances.

So long as no known law associated the distances of the planets from the Sun, it did not seem advisable to attempt to measure the distance of any planet from the Earth as a preliminary to determining the Sun's distance; for further observations were required in order to determine what relation the latter distance bore to the former. But so soon as Kepler proved that the distances and periods of the planets are associated by a simple law, it seemed a promising course to attack—instead of the Sun—some planet which approaches us within a less distance.

Let us consider, for example, the case of the planet Mars, in order that we may judge how much is to be gained by the course suggested. In doing this we are still following the actual order of events, for the first determination of the Sun's distance by modern astronomers, and with modern instrumental means, was founded on observations made upon the Planet of War.

In fig 8, the orbits of Mars (M) and the Earth (E) about the Sun (S) are accurately laid down,—the line S♈ representing a fixed line from which astronomers measure the motions of the planets around the Sun.* Now, it is seen at once that when near M, Mars is much nearer to the Earth's path than the Sun is; so that when the Earth and Mars are in conjunction in this neighbourhood, it becomes an easier problem to determine the distance of Mars than that of the Sun.

Fig. 8.



To put the matter simply, the Earth at E *looks larger* as seen from M than as seen from S; and to say she looks larger is the same as saying that she subtends a greater visual angle; and the visual angle she subtends from Mars precisely measures the displacement which Mars will show as seen from different parts of the Earth.

Supposing Mars thus favourably situated, and that two observers, one at E and the other at E′ (fig. 9),

---

* This line is only introduced to explain the unsymmetrical aspect of the two paths; to show, in fact, that these are intentionally eccentric.

observe the planet whose centre really lies at M, to lie on different points, *m* and *m′* of the celestial sphere. Then this arc *m m′*, if measured accurately, would at once give the actual displacement of Mars corresponding to the distance E E′ between the observers; for though in the figure *m m′* is closer to M than to E, yet in reality the celestial sphere on which we thus estimate the place of Mars may be regarded as infinitely far off, so that M E is as a mere point at the centre of this sphere; and therefore the arc *m m′*, as estimated from the Earth, is precisely the same as though it were estimated from M,—or, in other words, this arc measures the angle *m* M *m′* and therefore the equal angle E M E.′ Thus we learn at once the angle E M E′, and as we know the base-line E E′, we deduce the distance E M by a very simple process of calculation.

FIG. 9.

But what is essential to the accuracy of the result is that the arc *m m′* should be accurately measured. Whatever error we make in this measurement will produce a proportionate error in our final estimate of the distance of Mars.

Now, if there were no stars in the background of the heavens it would be absolutely impossible to measure *m m′* as accurately as our purpose requires. The problem would be quite as hopeless as the attempt to measure the Sun's distance by a similar process.

But the presence of stars upon the celestial vault, and the certainty which the astronomer possesses that these stars are at a distance incomparably exceeding that of Mars, make the measurement of this arc *m m′* feasible. The stars serve as index-points. In fig. 9 for example, a star is supposed to be placed at *s*; now while it would be hopeless for an astronomer to attempt to determine the direction of either line E M or E′ M′ in space, without reference to any star, it is quite easy to measure the arcs *m s* and *m′ s* with a very considerable degree of accuracy, and so to determine the difference *m m′*.*

And here one point in which the modern possesses an enormous advantage over the ancient astronomer, lies in the fact that spaces on the heavens which are blank, so far as naked eye vision is concerned, are shown by the powers of the telescope to be occupied by multitudes of minute stars,—and the minutest star serves quite as well as a large star for such observations as we are here considering. So that the astronomer need be under no anxiety lest Mars, during the period when he is nearest to us, should approach no

---

* Here I have supposed *s*, *m*, and *m′* to lie all on the same arc, which of course would not ordinarily be the case. It is easily seen, however, that it falls quite within the scope and bearing of ordinary astronomical observation, to measure not only the distances but the bearings of *m* and *m′* from *s*, and so,—two sides and an included angle of the triangle *s m m′* being determined,—to determine the third side *m m′*. I may notice here in passing that quite a large proportion of the details involved in the various processes applied to the problem considered in this chapter are necessarily left untouched, or are barely mentioned. A volume much larger than the present would be required to exhibit these details in full and in all their bearings.

star near enough to render the required measurements effective.

So soon as the distance of Mars has been calculated the distance of the Sun can be determined by the application of Kepler's third law. There is a preliminary process depending on the circumstance that S E and S M are not the mean distances of the Earth and Mars; but this process is perfectly simple, since observation has shown what is the true figure of each orbit. Thus Kepler's third law by showing us the exact relation between the mean distances, shows us the exact relation between S M and S E; and therefore between E M and S E.

The plan here described was the first from which astronomers obtained any satisfactory estimate of the Sun's enormous distance. Kepler, after a careful study of Tycho Brahe's observations of Mars, had already confidently stated that the Sun's parallax is not greater than 1′ (or in other words that the Sun's distance is not less than 13½ millions of miles). But Tycho Brahe's observations were such as we should now call altogether rough. Cassini proposed and carried out a much more exact series of observations. At his suggestion the Paris Academy of Sciences sent Richer to Cayenne, while Cassini himself, Römer, and Picard, observed Mars at different French stations. The parallax of Mars was indeed not measured, for the instrumental means of the observers were insufficient. But Cassini calculated that if the parallax had exceeded 25″ the means employed ought to have exhibited its effects. A parallax of 25″ in the case of Mars (situated as when Cassini's observations were made) corresponds to a solar parallax of 10″. Cassini expressed his conviction that the solar parallax is not greater than 9″·5—in other words, that the Sun's distance is not less than 85,500,000 miles.

The next application of this method involved the comparison of observations made by Lacaille at the Cape of Good Hope and by several astronomers at different European stations. The parallax deduced was 10″, corresponding to a distance of 82,000,000 miles.

I shall presently have occasion to mention other and much more trustworthy results obtained by this method in recent times.* For the present, however, I pass on to other methods.

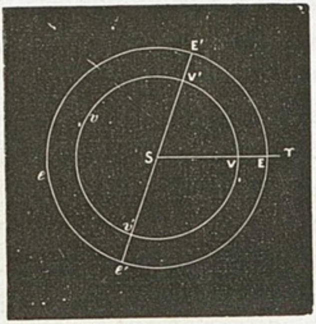
The path of Venus lies even nearer to the earth's orbit than that of Mars does. Fig. 10 represents the

---

* It might seem that as Mars comes into opposition at intervals averaging about 780 days, the method could be applied frequently, and so the results due to it could be rapidly improved upon. As a matter of fact, however, only those observations made when Mars is in opposition near perihelion are of service. From fig. 8, it will be seen that favourable opportunities do not occur at short intervals. The figure shows the successive conjunction-lines of the Earth and Mars between the years 1856 and 1871. It is seen that only the conjunctions of 1860 and 1862 are favourable, and *those* not so near as they should be to perihelion. (The wide distances separating conjunction-lines in this neighbourhood as compared with the opposite, are due to the relatively rapid motion of Mars near perihelion.) The opposition of 1877 will be exceptionally favourable, as the conjunction-line will fall nearly midway between those of 1860 and 1862. It is necessary for me to remark that fig. 8 is copied from a drawing of my own, illustrating a paper in the *Popular Science Review* for January 1867. Mr. Lockyer has copied

relation between the orbits E *e* and V *v* of the earth and Venus; S ♈ as before representing the line from which astronomers measure the motions of the planets round the Sun. It will be seen by comparing fig. 8 with fig. 10 that when the Earth and Venus are in conjunction as at E and V, the distance separating them bears a smaller proportion to the Earth's mean distance from the Sun, than that separating the Earth and Mars when in conjunction. But then there is a circumstance in which Venus is less favourable for the purpose of astronomers than Mars. When the Earth and Mars are in conjunction as at E and M, fig. 8, the sun is on the opposite side of the earth, and so Mars is seen on a dark sky; but when Venus and the Earth are in con-

Fig. 10.



this drawing, with additions derived from my charts of the planetary orbits, at page 185 of his *Elementary Lessons in Astronomy*. To this I of course make no objection whatever, as such drawings are meant to be copied. Only as he has omitted to refer the drawings to me, I am debarred from using my own work without showing my title to it. A similar remark applies to the figure at p. 184 of Mr. Lockyer's work, and to certain considerable portions of the letter-press.

junction, as in fig. 10, Venus lies directly towards the Sun, and even though visible (in powerful telescopes), yet is seen under very unfavourable conditions. The background of the sky is bright, and none but the chief stars can be discerned, unless the telescope is very large and powerful, in which case it is not so well adapted as a more manageable one would be, for the class of observations required. Even the leading stars are but faintly seen; and as Venus may not lie near any of them, the kind of measurement which was available in the case of Mars becomes too precarious for the purpose of determining any parallactic displacement of the planet.

Hence the direct observation of Venus, when nearest to us, after the manner applied to Mars, is not a very valuable method of determining the Sun's distance. It may yet be applied successfully (according to the plan proposed in 1848 by Dr. Gerling of Marburg); but even if it should, the method now to be considered is preferable.

When Venus and the Earth are in conjunction, she is not commonly on a direct line between the Earth and the Sun. She would be so, if the path V V′ *v* lay in the same level with the path E E′ *e*; but this is not the case. If we suppose the path E E′ *e* to lie in the plane of the paper, then the path V V′ *v*′ must be supposed to intersect that plane in the line *v*′ V′ the half of *v*′ V V′ lying slightly below the plane of the paper, and the half V′ *v* *v*′ slightly above, the short lines near V and *v* showing the greatest amount of separation between the two planes on account of this tilt. Hence, unless the conjunction happen to take place when Venus is at V′ or *v*′, Venus will not be on a direct line joining the Earth and the Sun. But when Venus is within a certain distance of these two points,*—the nodes of her

---

* It is easy to calculate how near Venus must be to a node in order that she may be visible on the Sun's disc. The extreme limits will be those corresponding to the case when the disc of Venus as seen from all the Earth save one point only, lies outside the Sun's disc, but as seen from that one point just touches the disc on the outside. Further, on account of the ellipticity of the two orbits, the exact extent of these limits will be different for conjunctions occurring when Venus is at V′ and at *v*′. To estimate these limits strictly, the exact values of the distances of Venus and the Earth from the Sun when in longitudes corresponding to S E′ and S *e*′ may be taken from the *Nautical Almanac*, and combined with the estimated diameter of Venus. The formulæ to be used are sufficiently simple, and will occur at once to the mathematical reader,—or may be deduced from my paper on the transit of Venus in the Monthly Notices of the Astronomical Society for March 1869 (vol. xxix.). But for our present purpose such exactitude is not needed. It may be well, however, to determine in a general way the limits in question; for which purpose we may assume that no transit need be considered in which the line joining the centres of Venus and the Earth does not meet the Sun. Hence, Venus must not have a greater geocentric latitude than the Sun's apparent semi-diameter. Now the distance of Venus from the Earth, and when she is in inferior conjunction, is to the Earth's distance from the Sun as 277 to 1,000. So that when her apparent latitude is equal to 16′ (which we may take as the Sun's semi-diameter) her heliocentric latitude is to 16′ as 277 to 723, or is about 6′·1. The inclination of her orbit being 3° 24′, or 204′, it is easily calculated that she must be within about $1\frac{2}{3}$° of a node, at the epoch of inferior conjunction, in order that a transit may occur. Thus at each node there is an arc of about $3\frac{1}{3}$° along any part of which Venus, if in inferior conjunction, will be projected on the Sun's disc; and as there are two nodes, the total range out of the 360° of her orbit along which transits can occur, is about $6\frac{2}{3}$°, or a 54th part of the whole. Hence, on the average there will be one transit in 54 conjunctions, and as conjunctions occur at average intervals of 583·920 days, there will be on the average one transit in $86\frac{1}{3}$ years. But this average would only correspond to the case where a very large number of conjunctions was considered.

orbit, as they are called,—she will be so nearly on the line joining the Earth and the Sun, that as seen from the Earth she will appear on the Sun's disc.

Now when a conjunction of this sort takes place, the observation of Venus's apparent place is rendered much easier, since the disc of the Sun forms a sort of index-plate, as it were, on which we can estimate her position. So that if Venus's distance is to be determined at all from observations of her parallactic displacement as seen from different parts of the Earth's surface, it is clear that the proper time for attacking the problem is when Venus is in transit.

Fig. 11.

Let us see, however, how this may be most effectually contrived.

Suppose that as seen from the point E (fig. 11) on the Earth at any moment Venus (V) is seen as at *v* on the Sun's disc; whereas as seen from E′ she appears to be at *v′*. Then it seems at first sight as though nothing could be simpler than to determine the distance *v v′*; and then having the arc *v v′* and the length of the base line E E′ we could at once determine the distance of Venus. For *v v′* is the arc between two lines from Venus containing the angle *v* V *v′*, which is equal to E V E′. So that we know the angle E V E′. For example, if the arc *v v′* were shown to be thirty-five seconds as seen from the Earth, then the angle *v* V *v′*

would be greater (because V is nearer to $v$ $v'$ than E is) in the proportion of E $v$ to $v$ V, or roughly as 7 to 5, so that the angle $v$ V $v'$ (or E V E$'$) is an angle of about forty-nine seconds. Now suppose that the stations E, E$'$, are so placed on the Earth's surface that the base line E E$'$ is known to be about 6,000 miles in length: then the distance E V exceeds 6,000 miles in the proportion that the radius of an arc of forty-nine seconds exceeds that arc, or roughly as 4,200 to 1. Hence E V is about 25,200,000 miles, and E S exceeds this distance in the proportion of about 7 to 2, or comes out equal to nearly 90,000,000 miles.

But for many reasons this direct method of solving the problem of the Sun's distance has not been hitherto applied. In the first place it is absolutely necessary that the observations made at E and E$'$ should either be made exactly at the same moment, or that the difference of time should be exactly known, so that the two observations may be fairly compared together. But for this purpose we must know the exact position of the stations E and E$'$ on the Earth, so as to be able from the apparent time at these stations to infer the true time at Greenwich or some other fixed station. It is easily seen that a very slight error in the determination of the longitude of either station would make the whole series of observations useless. For example, suppose the observer at E recorded the place of Venus on the Sun's disc at apparent noon for his station; and that the longitude of the station was *supposed* to be such that this epoch corresponded exactly with the epoch when the observer at E′ recorded the Sun's position. Then if the supposition were correct, the above process would be available. But if E were ten or twelve miles to the east of the supposed longitude, apparent noon would occur a minute or so* earlier than at a place in that longitude. But, in one minute, Venus, as seen in transit, moves over an arc of about two seconds on the Sun's face, so that the observer at E noting her place a minute or so too soon (so far as the comparison with the other observer's record is concerned) would set her two seconds of arc out of place. But our problem is one in which seconds of arc are all-important.

But this is not all. The determination of the exact place of Venus on the Sun's disc at any epoch would be a matter of extreme difficulty. It would be necessary to determine, not merely her distance from the Sun's centre, but her bearing from that point, and a very slight error in either determination would (in so delicate an inquiry) cause a considerable error in the determination of the Sun's distance. There is, indeed, a way of getting over this difficulty which I touch upon in appendix A; but though it gives, in my opinion, the very best method of determining the Sun's distance now available to us, it requires (as will be seen) a preliminary knowledge which was not possessed when the observation of Venus in transit was first proposed as a means for solving the problem we are upon.

Accordingly Halley proposed (in 1716) a plan for

---

* The exact difference of time would of course depend on the latitude of the station.

evading the observational difficulties. He suggested, that instead of attempting to estimate the position of Venus on the Sun's disc at any moment, the observers at two stations such as E and E′, should time the passage of Venus along her chord of transit. Neglecting for a moment the consideration of the Earth's rotation, Venus would seem to the observer at E to describe such a path as *l v m*, while to the observer at E′ she would seem to describe such a path as *l′ v′ m′*. Now if we know the length of time she takes in describing these chords, we know the length of the chords, since the rate of Venus's motion across the Sun's disc (the same of course for both stations on the assumption that the Earth is not rotating) is known from the tables independently of her actual distance. Hence it is a very simple problem in geometry to determine the distance separating the chords *l m* and *l′ m′*, and thence as in the former case to determine the distance of Venus, and so that of the Sun.

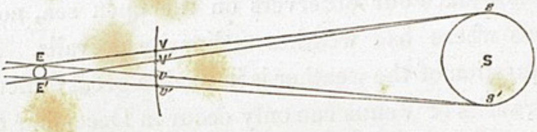
Nor does the fact that the Earth is rotating prevent us from applying this method; though it causes the problem to be somewhat more complicated. Venus in fact does not describe quite a straight chord across the sun as seen from any station; nor does she move quite uniformly; nor again is her rate of motion across the Sun's face exactly the same as seen from different stations. But all these points are such as the astronomer is quite accustomed to take into account, nor do they in themselves detract one whit from the certainty with which Halley's method can be applied.

But there is one effect of the Earth's rotation which has to be very carefully considered in weighing the value of Halley's method. It is absolutely necessary (since the duration of the transit is to be timed) that at each station the beginning and end of the transit should be visible. Now a transit may last a considerable time—as long indeed as eight hours; and it may not always be easy to find two stations—one far to the north, and the other far to the south, at each of which both the beginning and end of the transit will be favourably seen. For it must be remembered that a large part of the Earth is unfitted for the observer's purposes. We must not place our observers on the open sea, nor in regions where bad weather ordinarily prevails. And this question of the weather is in itself a great difficulty. For transits of Venus can only occur in December or in June, as is obvious from a consideration of fig. 10, where E′ and *e*′—the points near which the Earth must be that Venus in conjunction may be near a node—correspond to the Earth's position on about December 8 and June 6. Now, at a northern station in June, or at a southern station in December, fair weather may be commonly expected, but the reverse holds as respects the northern station in a December transit, and the southern station in a June transit. So that the difficulty of finding two stations, one northern the other southern, both well suited for observing both the beginning and end of the transits, and at both of which there is a fair prospect of clear weather at both epochs, is a very serious one.

In Appendix A we shall see more about the circumstances here considered, which it will be understood are not such, ordinarily, as to prevent Halley's method from being applied, though they call for the most cautious exercise of judgment in the selection of stations for the purpose. At present it suffices to say that the difficulty led Delisle, in anticipation of the transit of Venus in 1761, to propose another method.

Reverting to fig. 11, it will be clear that as V moves onward in the direction of the arrow* the time must come when the transit is just beginning at some point on the Earth's surface, from whence the first view (as it were) is obtained of Venus in transit. Some interval must elapse before the transit has begun for the whole Earth—or at least for all that hemisphere which is turned towards the Sun. And at some point on the Earth's surface—which will clearly be nearly opposite the point just referred to—the transit will seem to begin later than at any other station. Now, neglecting matters of detail, and considering Venus as a point for the moment, we may reason in this way on observations of the kind considered:—

FIG. 12.



---

* We suppose the Earth at rest, or—which is the same thing—consider only Venus's motion relatively to a line constantly joining the centres the Sun and Earth.

Suppose Venus at V (fig. 12) when the transit first begins, so that a line E V *s* just touches both the Earth and Sun, and that Venus is at V′ when the transit has begun for the whole Earth, so that E′ V′ *s* just touches both the Earth and Sun. Then we know the length of E E′, and therefore we know the length of V V′ which is less than E E′ in the proportion before used, of about five to seven. We also know exactly how long Venus has taken to traverse this arc V V′, and therefore, since we know how long she takes to complete the circuit of her orbit, we know what proportion the known length V V′ bears to the circumference of her orbit. This gives the circumference, and thence the radius of her orbit, whence, as before, we learn the radius of the Earth's orbit.

Here we have supposed the Earth at rest. But as the motions both of the Earth and Venus are known, the relative motion of Venus is known, and so the conditions of the problem are as fully ascertained as in the simpler case actually dealt with.

We thus see that the phenomena presented at the commencement of the transit are sufficient for determining the Sun's distance. So also are the phenomena presented at the end of the transit; since it is obvious that as Venus passes from *v* to *v*′ similar relations will be presented, but in a contrary order.

All that is required, then, for a successful application of Delisle's method is that one observer should have a favourable view of the commencement (*or* end) of the transit from some place on the Earth where the transit begins (or ends) nearly at the earliest, and that another should obtain a favourable view of the *same* phase at some place where the transit begins (or ends as the case may be) nearly at the latest. Both observers must time the commencement (or end) of the transit most carefully. Then to compare the two observations, in order to tell the absolute interval of time between the two, we must know the exact longitude of the two places; for the observations will of course be referred to local time, so that in order to compare them, we must refer them to some standard time, as that of Greenwich or Paris.

Here, then, are the difficulties in Delisle's method: unless the longitude of each station is accurately known, and furthermore, the exact local time at which transit begins or ends, the determination of the Sun's distance will be inexact. As respects the former point there is little difficulty, only the observers must stay some time at their respective stations, making suitable observations to determine the longitude of the station. This can be done either before or after the transit as may be convenient. But as respects the determination of the local time at which the transit begins (or ends as the case may be), there will be a difficulty if bad weather precede and follow the epoch at which the phase occurs. For we can only determine local time exactly when the weather is clear, so that we can make suitable observations on the stars; and during a few days of cloudy weather, the best astronomical time-pieces will get a second or two wrong. In

Halley's method the clock may be altogether wrong, yet if its rate be fairly good, the duration of the transit will be accurately determined; but in Delisle's the clock must show absolutely correct time.

Here again, however, the difficulties, so far from being insuperable, are only such as astronomers are in the habit of dealing with and mastering.

Both methods were applied during the two transits of the eighteenth century. Of these one took place in June 1761, and the other in June 1769. Both occurring during the summer of the northern hemisphere, the Earth's northern pole was bowed towards the Sun; and in this respect the circumstances of the transits differed importantly from those of the transits which are to occur in 1874 and 1882, for both these will take place in December, when the southern pole of the Earth is bowed towards the Sun.

The transit of 1761 was not observed in a very satisfactory manner. It was a transit for the observation of which Delisle's method was somewhat better suited than Halley's,* and the astronomers of the eighteenth

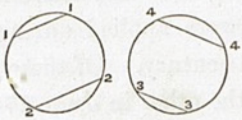
---

* It is worthy of notice that in the case of two transits separated by an interval of eight years, the former is commonly best suited for Delisle's method, the latter for Halley's. Both the transits will occur at the same season, that is, Venus will either be near her ascending node at both transits, or near her descending node at both. For an interval of eight years corresponds almost exactly to thirteen revolutions of Venus, so that, supposing Venus near a node when in inferior conjunction, she will be near her node at the conjunction occurring eight years later (or after five synodical revolutions). Now, it so happens, that the line of these successive conjunctions in the same neighbourhood continually regrades round the ecliptic, one interval being in fact about 2½ days less than eight years. Thus the latitude of Venus at successive eight

century were not so well prepared to deal with the difficulties of that method as those of our time will

yearly conjunctions near a node differs by the amount of Venus's motion in latitude (near a node) in the course of 2½ days. Thus the apparent path of Venus across the Sun's disc would (as supposed to be seen from the Earth's centre) be as 1 1, 2 2, fig. 13 (I), when she is near her

FIG. 13.



I II

rising node or when both the transits are December ones; and as 3 3, 4 4, fig. 13 (II), when she is at a descending node, or when both the transits are June ones. [The distance between 1, 1 and 2, 2—and between 3, 3 and 4, 4, would be about that shown in the figure, and the slope with reference to the ecliptic is of course the same (approximately) in all transits. But the pair of lines may have any position whatever as respects distance from the Sun's centre. Now, if either line in either figure falls very near the centre of the Sun's disc the other will not fall on the Sun, and there will be only one transit during those years when this conjunction is travelling past the node. This will happen in no inconsiderable proportion of these passages—a circumstance requiring notice because our treatises on astronomy commonly assert that transits of Venus occur at successive intervals of 8, 121½, 8, 105½, 8, 121½, &c. years, which is far from being strictly correct. In fact, instead of two transits occurring at every such passage, very nearly half the passages would supply only one transit. The list in Lalande's astronomy is wholly untrustworthy in this respect, as any one will find who will calculate the distances of Venus from her nodes at the conjunctions referred to in that list.] To resume;—Taking the case of December transits illustrated by fig. 13, we see that at the first transit when the path is as 1, 1, the northern station from which the path will appear lowest down on the Sun's disc, will give the longest interval; and the advantage of applying Halley's method will depend on the greatness of this interval as compared with the shorter interval during which the transit lasts as seen from some southern station. Now the Earth is rotating, and the effect of the Earth's rotation *considered alone* is to give Venus a continual slight westerly displacement in all places where the Sun is prove themselves. Yet the result of the observations then made, which were interpreted as giving a solar

moving from east to west—that is, at all places save those close by the south pole, at which the Sun (being above the horizon all day, or nearly all day), moves through part of the day from west to east. Hence at northern stations, where Venus's path is longest, she is hastened on her path by this westerly displacement; and so the lengthening of her period of transit is diminished and the value of Halley's method *pro tanto* reduced. At southern stations the shortening will be increased at places where the transit occurs during the mid-day hours, and diminished where the transit occurs during the midnight (nominal) hours. But at the former stations the apparent path of Venus will not be thrown so far south as at the latter; so that at the southern stations also we find that the greatest possible shortening due to parallax cannot be combined with an additional shortening due to the Earth's motion of rotation. But at the second transit of this set—when Venus appears to follow such a path as 2, 2 (fig. 13), the reverse is the case. At the northern station Venus's path is thrown southwards and so shortened, while her motion across the Sun's disc is hastened (by the effects of the Earth's rotation) and therefore also shortened; whereas at the southern stations, where Venus's path is most lengthened, her motion across the Sun's face is retarded and so lengthened. Halley's method is then applicable under the most favourable conditions for securing a considerable time-difference. Similarly, it may be shown that at a June transit, when Venus's path is as 3, 3 (the first of a pair), Halley's method is not so favourably applicable as at a June transit when her path is as 4, 4 (the second of a pair).

Theoretically this is just, but practically, especially in December transits, the difficulty of securing suitable stations near the pole which is turned towards the Sun, may altogether change the conditions. As a matter of fact, indeed, the approaching transits of 1874 and 1882 are exceptions to the rule; and I have been able to demonstrate that, so far from Halley's method being most favourably applicable in 1882 (as the Astronomer-Royal had inferred from reasoning resembling the above), there is no reasonable chance of its being applied *at all* in 1882, the only two southern stations where it is possible to apply the method being such that the Sun will be barely 5° above the horizon,—a state of things preventing all exact observation, and assuredly not justifying expeditions to stations so near the south pole that the observing parties would inevitably have to winter there. On the other hand, I have also been able to demonstrate that Halley's method, besides all the advantages parallax of 8″·65, corresponding to a mean distance of about 94,500,000 miles,* was a great improvement on any before obtained—and better, in fact (though this was due to chance), than that deduced from the more complete and satisfactory observations made in 1769.

The transit of June 1769 attracted an amount of attention both in England and on the Continent which afforded very creditable evidence of the scientific enthusiasm of the men of the last century. The Royal Society presented a memorial to King George III., requesting that a vessel might be fitted out at Government expense to convey skilful observers to one of the stations which had been judged suitable for observing the phenomenon. The petition was complied with, and after some difficulty as to the choice of a leader,

---

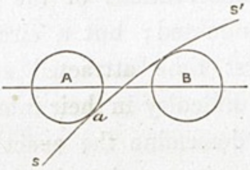
arising from its simplicity, will be applicable under more favourable circumstances than Delisle's, in 1874. It is necessary to observe that there is nothing hypothetical about this conclusion. The difference between my conclusions and those before adopted arises simply from my having taken into consideration facts which had been (mistakenly) imagined to be such as might safely be neglected. Since my results were published, papers by Peters, of Altona, and by Hansen, the eminent German mathematician, have confirmed all the views I had insisted upon. See further, Appendix A.

* It is convenient to notice that if the solar parallax were 10″ the distance of the Sun would be 81,738,420; and the distance corresponding to any other value of the parallax can be deduced by simply dividing 817,384,200 by the number expressing such parallax. The table in Ferguson's *Astronomy*, complacently quoted in Chamber's *Handbook*, at p. 248, is incorrect, owing to the erroneous estimate of the Earth's mean diameter on which the table is based. Oddly enough, Mr. Chambers has combined the correct estimate for the parallax at present adopted, with Ferguson's incorrect values. It would almost appear as though the figures had been simply quoted without being tested in any way, were not such an idea incredible.

the ship Endeavour, of 370 tons burden, was placed under the command of Captain Cook. Continental astronomers also betook themselves to the most advantageous posts for observing the transit, and when at length Venus came between the Earth and the Sun, her arrival and passage were watched by observers at Wardhus, Kola, and Kajeneberg, at St. Petersburg, Orenberg, Yakutsk, Pekin, Manilla, Batavia, Otaheite, St. Joseph (in California), Kola, Hudson's Bay, and other well-chosen stations.

Most of the observations of the transit were well and skilfully conducted; but a circumstance, which now for the first time attracted serious attention, caused a certain difficulty in their interpretation. The observers had to determine the exact interval of time occupied by Venus in crossing the Sun's disc—or else in certain cases the exact moment of time when she began to cross the solar disc. Now, if Venus presented the appearance of a mathematical point, the observation would be simply described in the words I have just used. But as Venus has a disc of appreciable dimensions, the question arises, whether the commencement or end of transit shall be considered with reference to the moment when the disc of Venus just touches the Sun's disc on the outside, or to the moment when her disc just touches his on the inside, or, finally, to the moment when her centre is crossing the Sun's edge. As to the last of these cases, we may dismiss it at once from consideration, because no observer, however experienced, could pretend to determine within a second or two when exactly one half of the disc of Venus was upon the Sun's. Either, then, the observer must note when Venus is, as at A (fig. 14), just touching the edge of the Sun's disc, S S′, on the outside, or, as at B, just touching that edge (the *limb* as astronomers call it) on the inside. Now, taking the case of ingress, if the observer knew exactly where Venus would begin to cross the Sun's disc, as at *a*, and very nearly the true instant of such ingress, he might, if the weather were very favourable, determine within a second or two the moment when the uniformity of the limb S S′ was marred by the encroachment of the disc of Venus. But as a matter of fact neither one condition nor the other is fulfilled, and so, even under favourable circumstances of weather, he might not detect the exact commencement of ingress. Nor is this all. By favourable weather, I mean something more than clear weather. In weather which seems perfectly clear the telescopist will often see the edge of the Sun's disc absolutely *rippled* through the effects of atmospheric disturbance, and when the Sun is near the horizon, even in good observing weather, the outline of the disc is often egregiously disturbed. Under such cir-

FIG. 14.



cumstances nothing would be more difficult than to assign the exact moment when external contact took place. But these are the very circumstances to be looked for in the observations necessary for determining the Sun's distance. The Sun *must* be low down at all the stations most suitable (in other respects). For the essential point both in Halley's method and Delisle's—nay, in any method whatever for determining the Sun's parallax—is that two observers shall be as far as possible from each other on the illuminated hemisphere of the Earth; so that they must be near the *rim* of that hemisphere. In other words, they must be near that great circle of the Earth along which the Sun is seen exactly on the horizon. The Sun then will be nearly on the horizon at such stations, and will be viewed necessarily under somewhat unfavourable conditions.

It was this consideration which led, and probably will always lead, to the selection of internal contact as the proper phase to be observed. In attempting to time the moment when Venus is just within the Sun's disc, either at ingress or egress, there will, of course, be difficulties arising from atmospheric disturbances; but on the whole the observation of this phase is easier than the observation of external contact.

But, unfortunately, a certain peculiarity affects the appearance of Venus when just within the Sun's disc. Instead of appearing circular, she assumes (just before she leaves the Sun's edge at ingress, or just as she reaches it at egress) a pear-shaped aspect, as at 1 (fig. 15), or such an aspect, as at 2 or 3, according to the nature of the telescope made use of, the conditions of the atmosphere, and the visual power of the observer. Thus the question arises, whether the assumption of this aspect is to be regarded as the true moment of internal contact, or whether that is the true moment when the circular part of Venus's apparent outline, if continued so as to form a complete circle, would just touch the Sun's outline. Between one moment and the other several seconds occur, and the whole question is one of seconds.

FIG. 15.

Here is a difficulty of grave importance; for it is to be noticed that a practised eye would be needed to determine the moment when the outline of Venus would just touch the Sun's if undisturbed, and even a practised eye might well be deceived on such a point. On the other hand, though an observer might time the exact moment when the outline of Venus is just clear of the Sun's (and within it), either at ingress or egress, yet as this moment depends (as already mentioned) on the qualities of the telescope and on the observer's visual powers—probably also on the state of the weather—there is room for a considerable error to creep in.

I shall discuss further on the interpretation of the peculiarity just considered. At present the point to be attended to is the actual effect produced by this peculiarity, upon the trustworthiness of the results obtained in 1769. The observers were for the most part unprepared for the serious difficulty which the peculiarity actually introduces. Although in 1761 Venus had been distorted at ingress and egress, while Halley himself had in 1753 noticed that Mercury exhibited a similar distortion, it had not occurred to astronomers to discuss the phenomenon and so to be enabled to point out how the difficulty might be got over. In fact, the whole matter had been so completely overlooked, that, as I have said, few of the observers in 1769 were prepared for the peculiarities which Venus presents when in interior contact with the Sun.*

The result was that much difficulty was experienced in interpreting the observations. It will be understood that more observations were made than were actually necessary for the solution of the problem. One pair of observations, if absolutely exact, would have sufficed to determine the Sun's distance; but as errors must to

---

* There is nothing more remarkable in the history of astronomical observations than the little preparation made for important occurrences, such as transits and eclipses, so far as the actual observation of the phenomena is concerned. Abundant preparation is made as far as instrumental means are concerned; but again and again the history of astronomy exhibits cases where the actual phenomena of transits and eclipses take the observer by surprise, and so are not observed as well as they might have been, even when abundant information has been in reality available for the instruction of those who are to take part in watching such occurrences. There is much, for example, in the history of eclipse-observations that is exceedingly painful to the real lover of science, more particularly in the wearisome repetition of observations which have already revealed all they can reveal, and in the apparent dearth of invention as respects the devisal of new modes of research.

a greater or less extent affect all observations, it was necessary that many sets should be made in order that the mean result should be taken. Now, in all such cases the amount of reliance which is to be placed on the final or *mean* result depends on the closeness with which the several results aggregate round the mean. Precisely as we should place little reliance on the deduced mean of a series of ordinary measurements which differed considerably *inter se*, so astronomers would not be prepared to accept with confidence a value of the Sun's distance which was the mean of several discordant results.*

Now, the observations made in 1769 were in this respect much more satisfactory than those made in 1761; but owing to the peculiarity I have mentioned they did not accord so well by any means as might have been anticipated.

It is not too much to say that in the attempts first made to determine the Sun's distance by means of the transit observations of 1769, no settled principle was adopted. It was not much to be wondered at, therefore, that the estimates of the solar parallax varied between somewhat wide limits; so that whereas some mathematicians made the solar parallax as great as $9''\cdot2$, others obtained a value of only $7''\cdot5$. The corresponding values of the Sun's distance are respectively

---

* The total number of observers in 1761 was no less than 63—thus distributed: 13 in North Europe, 8 in England, 15 in France, 6 in Spain, Portugal, and Italy, 16 in Germany, and 3 in other places. In 1769 there were observers at 50 stations in Europe, 6 in Asia, 17 in America, and at one station in Polynesia.

87,890,780 and 108,984,560 miles,—a range compared with which recent discrepancies seem insignificant.[*]

During the years 1822 and 1824, Encke re-examined the whole series of observations made on the transits of 1761 and 1769. With wonderful patience, especially when we consider the nature of his materials, he combined together the results of no less than 149 observations made during the former transit, and as many made (at 75 stations) during the second. From the transit of 1761 he deduced for the Sun's parallax the value 8″·49, and from the transit of 1769 he deduced the value 8″·60;[†] while, by combining the two, he arrived at 8″·5776, the value which was employed during so many years in the 'Nautical Almanac' and other like works. The corresponding distance, viz., 95,274,000 miles, held its ground during all those years in popular treatises on astronomy.

It is somewhat surprising, considering the evidence

---

[*] The results of the transit of 1761 are thus summed up in Dr. Bruhn's *Life of Encke* (*Johann Franz Encke, sein Leben und Wirken*):—Short obtained a parallax of between 8″·47 and 8″·52; Pingré, 10″; Rumowski, 8″·35; Planmann, 8″·2; Audefredy, 9″·2. From the transit of 1769, adds Bruhn, Wm. Smith deduced a parallax of 7″·5; Hornby, 8″·78; Pingré, 9″·2, 8″·88, and 8″·43; Lalande, 8″·8; Lexell, by Euler's method, between 8″·65 and 8″·86, whence he finally adopted 8″·8. The value of the Sun's distance corresponding to all these results can be at once deduced from Table II. at the end of this work; save only the distance corresponding to 7″·5 (in Bruhn's work it is written 7′·5) which is stated above.

[†] The actual results deduced by Encke were—from the transit of 1761, 8″·490525 ± 0·060712″; from that of 1769, 8″·6030 ± 0″·0460; from the two combined, 8″·5776 ± 0″·0370—3·0120$\delta\rho$, where $\delta\rho$ is the correction for the Sun's semi-diameter (estimated by Encke at 958″·424). Later, Encke deduced the value 8″·5716, by introducing some corrections in his work.

which was afforded by the discrepancies between the observations made in 1761 and 1769, that this result should have been regarded with such confidence, since it needed but a brief examination of the basis on which Encke's result was founded, to see that no faith whatever could be placed in three at least out of the five numerals in the expression 8″·5776. Delambre regarded 8″·6, very justly, as the most probable value of the solar parallax half a century ago. It was rightly admitted that the observations of Venus in transit afforded the most reliable results; but Delambre, Bessel, and other astronomers of eminence, were far from adopting the value 8″·5776 with that implicit confidence which caused the corrections made in recent times to attract so much notice, and so greatly to surprise the general public.

The observation of Venus in transit being admittedly the most trustworthy method of determining the Sun's distance, it might have been supposed that no new results could serve to throw doubt on those deduced by Encke, until the time should come when Venus would again cross the Sun's face—that is, until the year 1874. But the rapid progress of science during the past half century, though it has not served to alter the relative value of different methods for determining the Sun's distance, has enabled astronomers to apply some of the less powerful methods in so much more effective a manner than of old, as to obtain more trustworthy results than had followed from the best method of all when less skilfully applied.

Amongst these relatively inferior methods, no less than four are novel; but a very brief description must suffice for each.

It had been noticed by Laplace, towards the close of the last century, that among the perturbations of the Moon there is one which depends on the Sun's distance. Suppose $M_1$ $M_2$ $M_3$ $M_4$ to represent the Moon's path around the Earth, E. Then clearly as the Moon moves from $M_4$ through $M_1$ to $M_2$, she is disturbed by the Sun's action, which is here greater on the Moon than on the more distant Earth. On the other hand, while the Moon is moving from $M_2$ through $M_3$ to $M_4$, she is disturbed, because the Sun's action is greater on the Earth than on her. Without entering into an exact investigation of the effects thus produced, it is abundantly evident that the Sun's perturbing effect in the former case will be greater than in the latter, because the radius of the orbit $M_1$ $M_2$ $M_3$ $M_4$ clearly bears a greater proportion to $M_1$ S than to $M_3$ S. The Moon's orbit is indeed so minute compared with the Sun's distance, that the difference is very slight; but still there is a difference. When the Moon is at $M_1$ the Sun tends to pull her more strongly away from the Earth than he tends to pull the Earth away from the Moon when the latter

Fig. 16.

is at $M_3$; and a similar preponderance holds for other and corresponding positions of the Moon and Earth. It follows that there is a slight variation in the Moon's motion depending on this cause alone, and readily admitting of being estimated theoretically, while the continued observations made by astronomers on the Moon's motions suffice to show how great the perturbation really is. It is only necessary to compare the theoretical with the observed value to deduce the Sun's distance; only, of course, the accuracy of the result will depend on the number and accuracy of the observations. Laplace, with the best observations available in his time, deduced for the Sun's parallax the value $8''\cdot6$, corresponding closely with the value subsequently deduced by Encke from the transits of Venus. 'It is remarkable,' wrote Laplace, 'that an astronomer, without leaving his observatory, by merely comparing his observations with analysis, has thus been enabled to determine the distance of the Earth from the Sun—an element the knowledge of which has been the fruit of long and troublesome voyages in both hemispheres.'

But this method is clearly one which modern astronomers can apply much more effectively, because the observations at their disposal are so much more numerous and so much more exact. Accordingly, Hansen, the eminent mathematician and lunarian, announced in 1854, in a letter addressed to the Astronomer Royal, that this method, applied with the aid of his new tables of the Moon, gave a solar parallax of $8''{\cdot}9159$, corresponding to a distance of 91,659,000 miles.

Another method, depending on the apparent motions of the Sun, was applied (with a very similar result) by Leverrier.

If the Earth had no satellite she would travel on her elliptic orbit round the Moon, with no other perturbations than those produced by the planets. But since she has a satellite, whose mass is an appreciable though small aliquot part of her own, she is disturbed precisely in the same way, though not to the same extent, that the Moon is disturbed. The Moon travels once in a lunar month around her orbit, but the point round which the Moon moves is not the centre of the Earth, but the centre of gravity of the Earth and Moon; and around that centre of gravity the Earth also travels once in a lunar month. Now, precisely as an observer on the Moon would have in effect the range of the Moon's orbit around this centre of gravity, as a base-line by which to estimate the Sun's distance, so the observer on the Earth has the range of the Earth's orbit around the same centre of gravity for the same purpose. The diameter of this last-named orbit is indeed very small—little more, in fact, than three-fourths of the Earth's own diameter; but by the radius of this small orbit the Earth is sometimes in advance and sometimes behind her mean position. In other words, her motion in longitude (that is, her angular motion round the Sun) is not equable. Over and above the variation of her velocity due to the ellipticity of her path, there is this alternate advance and (relative) retrogression, having for its period a lunar month. Obviously, the observed effect, so far as the astronomer is concerned, is an apparent irregularity in the Sun's motion, having the same period of one lunation. The effect is exceedingly minute: it is less than the displacement of the Sun as seen from different parts of the Earth; and, as we have seen, *this* effect could never be employed to determine the Sun's distance. Why, then, it may be asked, is the other and smaller effect available? For this reason simply, that the daily observations made on the Sun in the meridian supply a fund of materials for estimating the effect in question. Such observations are made (severally) at one station by one telescope, and if not by one observer, yet by a series of observers who are always working together, so that their relative modes and powers of observation are comparable together.*

Leverrier, by the careful study of an enormous number of observations on the Sun, made at the principal observatories in Europe, came to the conclusion that the Sun's parallax is 8″·95, corresponding to a mean distance of 91,330,000 miles. Mr. Stone, however, has detected a numerical error in M. Leverrier's calculations; and when this error is corrected the value 8″·91 results, corresponding to a

---

* The great point, however, is that all the observations are meridional. Were extra-meridional observations of the Sun as trustworthy as those made on the meridian, the Sun's distance could have been long since determined through those effects of the Earth's rotation which depend on the length of her diameter.

distance of 91,739,000 miles.* Mr. Simon Newcomb, of America, has, by the application of the same method, deduced the parallax 8″·84, corresponding to a distance of about 92,500,000 miles.

MM. Fizeau and Foucault applied a method differing wholly in character from any that had before been thought of. It seems at first sight incredible that the ingenious combination of revolving wheels or mirrors should serve to determine the Sun's distance; but such is the case. The essential point in the new method is the direct measurement of the velocity with which light travels. This velocity had been determined in two ways by astronomers, or rather it had been discovered in one way, and the deduced result had been confirmed in another. When Jupiter is in opposition—

---

* Every method of solving the problem of the Sun's distance has its special difficulties. In Leverrier's method, the accuracy of the result is wholly dependent on the accuracy of our estimate of the Moon's mass; for clearly on this estimate depends the extent we are to assign to the Earth's monthly orbital motion around the common centre of gravity of the Earth and Moon. But the Moon's mass is only measurable by observations determining the amount of the nutation of the Earth's axis, a quantity of the same minute order as the solar parallax itself. Still, this method has the advantage of depending on a very large number of observations, both as respects the determination of the Moon's nutation, and that of the inequality of the Sun's motion. It is obvious that the latter inequality may be employed either to determine the Moon's mass when the Sun's distance is known, or *vice versâ*. It has been employed both ways, Delambre having deduced the value of the Moon's mass by this mode. The way in which the inequality is applied will depend on the question whether the Moon's mass or the Sun's distance is supposed to be best known *by other methods*. At present it is assumed that the Moon's mass is the more accurately determined element; but doubtless after the transits of 1874 and 1882 the inequality in the Sun's motion in longitude will be applied to determine the Moon's mass from the known distance of the Sun.

at which time he is nearest to us—the eclipses and occultations of his satellites were found to occur a few minutes earlier than had been calculated; whereas when he is near conjunction these phenomena occur before the calculated time. Römer first pointed out the meaning of this observation. He showed that the phenomena really occur at the calculated epochs, but that the light which brings to us the account of those phenomona reaches us more quickly when Jupiter is nearer to us than when he is farther away. Bradley afterwards found in this discovery the explanation of the aberration of the fixed stars. If light travelled with infinite velocity we should see the stars in the same direction whether the Earth was at rest or in motion. But as the velocity of light, though very great, is yet not infinite, the apparent direction in which the light from a star reaches the terrestrial observers is affected by the motion of the Earth, according to a law precisely similar to that which causes the apparent direction of the wind to be affected by the motion of an observer who is rapidly carried onwards in a carriage or other vehicle. And when Bradley determined the amount of the aberration of the fixed stars at different seasons, he found—and astronomers have since abundantly confirmed the result—that the precise velocity assigned to light by Römer was that required to account for the peculiarity which affects the apparent place of every star in the heavens, as the Earth sweeps onward on her yearly orbit.

But it will be seen that neither observation supplied the means of directly determining the velocity of light in miles per second. All that was known was—first, that light takes a certain interval of time in crossing the Earth's orbit (or some known chord of that orbit); and, secondly, that the velocity of light bears a certain proportion to the Earth's velocity in her orbit. Until the exact dimensions of the Earth's orbit are known, neither of these facts informs us of the real velocity of light. Judging, however, from Encke's estimate of the Sun's distance, astronomers concluded that light travels at the rate of no less than 192,000 miles in a single second of time.

It might seem altogether hopeless to attempt to estimate directly a velocity so enormous as this. Remembering how the velocity of sound has been measured, and considering only the application of a similar method to the case of light, how utterly futile does the very thought of such an attempt appear! We can make a signal when a sound is heard at one station, and observers at another station can note how long the sound takes in reaching them; because where the stations are at a considerable distance, an appreciable time elapses before the sound travels from one to the other. But the very best signal we can use is some visible signal (in preference, I mean, to some electric signal), i.e., a light-message, which travels so quickly that we can wholly neglect the time it has taken, in comparison with that taken by the sound. Obviously, we have no such means of measuring the passage of light, for what is our signal to be if the velocity with which it is conveyed is so largely to exceed the velocity of light that we can neglect the time occupied in its transmission? But if we had even a satisfactory answer to this question (instead of having none whatever), the problem would yet be insoluble in this manner. Suppose the light were shown at a distance of 500 miles—about the limit at which any terrestrial light could be seen, even under the most favourable atmospheric conditions—yet the time occupied by the light-waves in traversing this distance would be but about the 360th part of a second. What instrument or what observer could note such an interval—to say nothing of measuring it, which would yet be absolutely essential to the successful solution of the problem?

I shall not here enter into a full account of the means by which Foucault and Fizeau solved a problem apparently so intractable, referring the reader to 'Pouillet's Physics' and other works in which the subject of light is dealt with. The general principle of the method employed by Fizeau may be thus presented. Suppose we see an object by light-rays which have been caused to traverse a long path by means of several reflections. Now conceive that the continuity of the long path is simultaneously broken at regular intervals at two points, one near the beginning the other near the end of the path, the path being broken—re-made—broken—re-made, and so on. Then if light travelled with infinite velocity, the light-rays, which at any instant traversed the first part of the path at a time when the path was *made* there, would traverse the last part also, because at that same instant the path would be complete *there* also. But light not travelling with infinite velocity, the light-rays which pass the first part of the path may be stopped by the break in the second part, if the interval between the making and breaking be but short enough. Now, Fizeau had a revolving toothed wheel, and matters were so arranged that when a tooth of this wheel was opposite a certain small aperture, the path of light was broken both at its beginning and end, for the light had to pass through this aperture, and then, after pursuing a long course, to pass out again through the same aperture. Now, when the revolution was moderately rapid, light-rays which passed through the aperture found the aperture open when they came back again to it; but by causing the revolution to be very rapid indeed, so that a very minute fraction of a second elapsed between the passage of successive teeth across the aperture, it was possible to cause the light-rays which had entered while the aperture was open to be prevented from passing out again by the interference of a tooth of the wheel. It is easily seen that when the wheel revolved at this particular rate there would be a total eclipse so far as light coming through the aperture was concerned. For light which went in when any portion of the aperture was free, would return to the aperture when just that portion of the aperture was closed. Then, as Fizeau had the means of telling at what rate the wheel was revolving when total eclipse thus occurred, he could tell precisely what fraction of a second elapsed between the passage of tooth after tooth across the aperture; and knowing the length of the path traversed by the light-rays he could measure the velocity of light with a considerable degree of accuracy. Foucault adopted an arrangement in which a plane mirror was caused to rotate very rapidly, and the principle of his plan (which, to be fully explained, would require more space than is here available) depends on the duration of visual impressions.* Fizeau's method, in some respects inferior to Foucault's, resulted in assigning to light a velocity of 194,600 miles per second; but Foucault's gave a velocity of only 185,300 miles per second, falling considerably short of the estimate of 192,000 miles above referred to. So satisfactory were Foucault's experiments, that this discrepancy was held gravely to affect the estimate of the Sun's distance, on which the latter value is based. It would follow, if Foucault's experiments were held to indicate truly the velocity of

---

* It may be thus exhibited:—An image of a certain wire is seen directly, and when a certain plane mirror which can be rapidly revolved is in a certain position, the image of the wire is caused to appear in coincidence with the wire seen directly. The mirror is so rotated as to take up once in each rotation the required position; and so long as the rotation is slow the reflected image makes successive appearances. With an increase in the velocity of rotation the image appears continuously in one place—owing to the continuance of visible impressions. Now, for moderate velocities of rotation, the position thus taken up by the image coincides with that of the wire seen directly. But with a very rapid rotation the position of the mirror suited for causing the image to be visible (after reflection), no longer accords appreciably with the position required when the mirror is at rest. Accordingly, the image appears appreciably separated from the wire seen directly; and the amount of separation, combined with the known velocity of rotation, supplies the means of estimating the velocity of light.

light in the interplanetary spaces (as well as in air), that the Sun's mean distance would be but 91,400,000 miles, his parallax 8″·942.

Yet another method was suggested by the Astronomer Royal. He pointed out that instead of comparing the position of Mars on the sky (when the planet is near opposition) as seen from different stations, an even more satisfactory estimate of the planet's distance, and so of the Sun's, might be obtained by observing how far the diurnal rotation of the Earth, by shifting the place of any fixed station, affected the apparent position of the planet. It is clear that if the station E (fig. 9) is supposed to be carried by the Earth's rotation to E′, the observer can as effectively compare the distances at which the observed places of Mars, *m* and *m*′, lie from a fixed star *s*, as though there were two observers, one at E and the other at E′ at the same instant of time; for astronomers know well how to take into due account the motion of Mars during the interval.

In 1862 this method was employed, as well as the former method of treating observations of Mars. The result was to confirm the impression that the Sun lies nearer to us than had been so long imagined. Stone, of Greenwich, by combining the two methods, deducing the solar parallax first from observations of Mars made at Greenwich alone, then from observations made at Greenwich and Capetown, then from observations made at Greenwich and Williamstown, and combining all these results, deduced a solar parallax of 8″·943, with a probable error of 0″·051. This corresponds to a distance of about 91,400,000 miles, with a probable error of about 500,000 miles. Winnecke, by combining observations of Mars made at Poulkowa and Capetown, deduced a parallax of 8″·964, corresponding to a distance of about 91,200,000 miles. Newcomb, from the observations of Mars in the same year, deduced a parallax of 8″·855, corresponding to a distance of about 92,300,000 miles.

Besides these, there were estimates by Leverrier, Stone, Newcomb, Pogson, and others, founded on the re-examination of processes already referred to, or on observations enabling those processes to be applied anew with more or less chance of exactitude in the results.

By the year 1864 it had become abundantly clear that the accepted estimate of the Sun's distance was too great. All the new values clustered around a value of about 8″·9 for the parallax, corresponding to a distance of about 91,850,000 miles.

Thus astronomers were led to re-examine the observations of the transit of 1769 in order to see whether they could be so interpreted as to correspond with the result pointed to by so many independent researches. Powalky subjected these observations to a new discussion and deduced the value 8″·832, corresponding much more closely than Encke's with the recent determinations. But astronomers were not satisfied with his labours, because he rejected some of the more important observations made in 1769, without assigning sufficient cause, and also because the agreement between those observations which he retained was by no means exhibited satisfactorily. As Admiral Manners said, when addressing the Astronomical Society in 1862, 'The weight of Encke's value could scarcely be said to be affected by Powalky's discussion.'

Newcomb, of America, was more successful. He deduced the value $8''\cdot 87$ by a method altogether more satisfactory than Powalky's. But still the agreement between the different observations was not so satisfactory as could be wished; nor had Newcomb adopted any fixed rule for interpreting the observations of internal contact, which, as I have said, are affected by the peculiar distortion of Venus's disc at that moment.

It was reserved for Mr. Stone, of Greenwich, to solve in a satisfactory manner a difficulty which had long perplexed astronomers. We have seen that there are two phases which an observer of Venus in transit might try to catch—the moment when the connection between the disc of Venus and that of the Sun appears to break at ingress or form at egress, and the moment when the outline of Venus, if undisturbed, would just touch the outline of the Sun. Now, if we consider why Venus is distorted so peculiarly near the time of internal contact, we shall see which of these phases corresponds most closely to the real moment of internal contact. The matter is sufficiently simple. Owing to a peculiarity of vision, every bright object appears somewhat larger than it really is, the borders of the image on the retina affecting the neighbouring part of the retina. It follows obviously that every dark object will appear somewhat smaller than it really is, for the background on which the dark object is depicted will be relatively bright, and so its image on the retina will encroach on that of the dark object. Thus, suppose the arc s s′ v′ (fig. 17) to represent part of the true outline of the Sun, then, to the observer, the outline would lie as shown outside this circle; but if the circle s′ v v′ represents the true outline of Venus, her apparent outline would lie as shown inside this circle. It is clear, then, that the part of the outline of Venus which lies upon the Sun seems to belong to a smaller circle than the part of the outline lying outside the Sun. This is true until the actual moment when the true outlines touch, when the appearance would be that shown in fig. 18, the connecting ligament being the finest conceivable line; and obviously (referring to ingress), the undistorted part of the apparent outline of Venus would have seemed to belong to a circle touching the Sun's outline some seconds earlier. Similar remarks apply to egress.

Fig. 17.

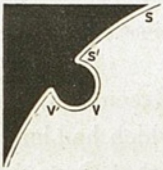


Fig. 18.

So that if an observer could note the moment when the discs of Venus and the Sun were apparently united by a fine line, as in fig. 18, he would have ascertained the moment of real contact; whereas if he had noticed the time when the circular part of Venus's outline would touch the Sun's, if continued undistorted, he would have caught quite a distinct phase, which we may call apparent contact. As a matter of fact, only the finest possible telescope, and great care and observing skill on the part of the astronomer, would render it possible to notice the moment when such a ligament as is shown in fig. 18 broke or formed. Usually a much coarser ligament only would be noticed; but the observation of the sudden breaking or formation of the ligament would be regarded as an approximate observation of real contact.*

Now, what Mr. Stone did was to infer, from the account actually given by the different observers, whether real or apparent contact was noticed; and he introduced a definite correction, according to the best estimate we have of the effects of irradiation in enlarging the solar disc and diminishing that of Venus.

By adopting this definite rule, Mr. Stone not only deduced a solar parallax corresponding closely with

---

* In Appendix A will be found some further remarks on this point, in relation to the expectations connected with the approaching transits. It has seemed to me that the view taken by Mr. Stone of the value of those expectations is altogether too despondent. It is quite true, as he points out, that different observers will see a ligament of different breadth—in other words, will catch a different phase; but we need not therefore fear that a corresponding error will be introduced. If each observer does but note the apparent breadth of the ligament, there will be the means of in large part eliminating this cause of error.

that obtained by the various other methods described above, but (which is much more important) he brought the observations of the transit into most satisfactory accordance. To quote the words of Admiral Manners' address on presenting to Mr. Stone the Gold Medal of the Royal Astronomical Society—'Mr. Stone's investigation includes every complete observation from internal contacts of the transit, and yet, notwithstanding this circumstance, all the observed durations are represented with a degree of accuracy which is, beyond all doubt, within the limits of probable errors of observation. Mr. Stone has effected this object by simple interpretation of the words of the observer, inferring from his language, and from the accompanying circumstances of the case, whether the phenomenon noticed by him referred to an apparent or a real contact.'

The result thus deduced assigned to the Sun a parallax of 8″·91, with a probable error of 0″·03, corresponding to a distance of about 91,730,000 miles, with a probable error of about 300,000 miles. In other words, the Sun's distance, according to this latest, and in many respects most satisfactory determination, is found to lie between about 92,030,000 miles and about 91,430,000 miles. If we assume 8″·9 as, on the whole, the best value for the parallax, implying, as it does, that as yet we have no knowledge of the next decimal figure, we have for the Sun's mean distance 92,000,000 miles;* as we ought not to admit any

---

* The very value, be it noted, which Professor Smyth, Astronomer

significant figures beyond the second. Doubtless, after the observations which are to be made on the transits of 1874 and 1882, astronomers will be able to extend the value of the parallax to a second decimal figure, and so have three significant figures in the expression for the distance. (See Appendix A.)

It remains to be noticed that the corresponding value of the Sun's diameter is fairly represented by 850,000 miles, nor is anything gained by expressing this diameter, as is so commonly done, with significant figures down to the units' place. Certainly if the true solar parallax is 8″·94000, for instance, then the exact value of the Sun's diameter is 852,908 miles. But when we see that the value of the solar parallax can only fairly be presented to the first decimal place, or as 8″·9, we cannot pretend to know the Sun's diameter with greater exactness. The limits of error in our estimate of the Sun's distance being probably four or five hundred thousand miles at the least, it must be admitted that the limits of error in our estimate of the Sun's diameter cannot be less than four thousand miles.

The surface of the Sun is equal to four times the area of a circle having this diameter of 850,000 miles, or in round numbers to 2,285,000 millions of miles. His volume is about 1,250,000 times that of the Earth.

---

Royal for Scotland, considers to be recorded in the dimensional features of the Great Pyramid. See his *Life and Work at the Great Pyramid*, which goes far to demonstrate that a number of the facts regarded as recent acquisitions of astronomy were known to the architects of that most remarkable erection.

In drawing to a close this chapter on the most difficult and important problem of astronomy—a chapter which, long as it is, has barely sufficed to convey so much of the history of investigation as seemed essential to my purpose—I would remark that there is no branch of research with which astronomers have better reason to feel satisfied. I cannot consent to speak of recent successes as though they had removed a reproach from astronomy. There is no single part of the history of this problem of which astronomers have not abundant reason to be proud. By long and patient labours they have been able to overcome difficulties which might fairly have been thought insuperable. Availing themselves at every step of the best means they could secure for approximating more and more closely to the truth, they have necessarily had during their advance to pass from ground which they had formerly occupied. But every such change of ground has been an advance towards more complete success. Sir John Herschel has said that the recent correction in the value of the Sun's parallax, corresponds to the apparent breadth of a human hair at 125 feet, or of a sovereign at eight miles off; 'and that, moreover, *the error has been detected* and *the correction applied*; and that the detection and correction have originated with the *friends and not with the enemies of science*.' But I would go even farther than this, since in place of regarding the recent change as involving the detection and correction of error, I would speak of it rather as a new approximation, more successful indeed than former ones, but not therefore changing those former successes into defeats. The astronomy of half a century since had as good reason to be proud of Encke's work as the astronomy of the present day has to rejoice at the successes of Hansen, Foucault, Leverrier, and Stone; or as the astronomy of future ages will have to boast of those labours by which the results now accepted will inevitably be improved upon.

## CHAPTER II.

### THE SUN AS RULER.

ONE of the most important of the Sun's functions is that by virtue of which he rules the motions of his family of planets. By the exercise of his mighty attractive influence he continually controls the tendency which they have to rush tangentially far out into space beyond the influence of his illuminating, heating, and actinic rays. Their swift orbital motions, combined with the relative stability of their axial *pose*, result in producing the orderly succession of the seasons. But this succession would come to an end, were it not for the stability of their orbital motion; and this stability is due to the Sun's overmastering attraction. To this it is due that the paths of the planets though undergoing continual processes of variation, yet suffer no sudden changes as respects their distance from him, or, therefore, as respects the period necessary for a complete revolution. Nay, so perfect is the whole scheme of governance that even the processes of slow change take place within limits, and those limits not very wide. Not merely can the orbits of the planets suffer no sudden change, but they can neither suffer a great change nor a permanent change.

We might dwell much farther on the importance ot the Sun's influence as the most massive portion of the scheme of which he is the centre. On the one hand we might point to the possibly even vital importance of the action which causes the terrestrial equinoxes to circuit the ecliptic in their grand precessional year of 25,868 solar years, or of the slowly-exerted influence which changes the eccentricity of the Earth's orbit. It is very far from unlikely that but for these influences the Earth would long since have been rendered unfit, through a species of exhaustion, for being the abode of living creatures. But as it is, continents become oceans, and oceans continents, one hemisphere interchanges with another the office of supplying the chief proportion of land surface; activity is followed by rest and rest by activity; and so through countless cycles this globe has been and will continue to be a fit abode for innumerable races. On the other hand, we might dwell on the influence which the Sun's mighty attractive influences exert in gathering in from all sides abundant supplies of motive energy, to recruit it may well be, his seemingly exhaustless stores of heat and light and chemical activity.

But without dwelling further in this place on themes of which some will find a place in other chapters, while others—interesting and fascinating though they be—must yet be regarded as lying outside the range of our subject, let us proceed at once to consider what the Sun's true influence is, by virtue of that principle of gravitation which causes every particle of his mass to aid in attracting all bodies within the sphere of his influence.

In one sense it may be said that the sphere of the Sun's influence is all space. According to the present conceptions of the power of gravitation, there is no particle of matter throughout the whole universe which does not feel the attractive influence of the Sun (as indeed of every particle of matter). But to all intents and purposes the Sun's reign may be regarded as limited. His influence on the stars is not merely minute so far as the amount of motion it is capable of producing in any given interval is concerned, but it is to be regarded as the influence of a peer among peers, not of a king over his subjects. The results of the mutual attractions of the stars may be, and doubtless are, of the utmost importance, but they do not belong to the history of the Sun as a ruler. On every side, then, the Sun's rule is limited—for in all directions there are stars, and the sphere over which each star rules, is as definite as that governed by the sun, so that in each direction we come upon regions where his influence is subordinate to the influence of some other orb.*

---

* It is sometimes said that a body like a comet can pass from the sway of one star to come permanently under the dominion of our Sun or of another star, and *vice versâ*; but setting aside the case of interference with such a body through the action of a planet, or by reason of atmospheric resistance near the Sun, or the like, this can never happen. For let us suppose that a comet is passing from the sphere of one star's influence to that of the Sun's. Then it cannot be moving on a closed orbit around the Sun, at this time, for if so it is already subject to the solar dominion, contrary to our supposition. It must then be travelling on a hyperbolic or parabolic orbit around the Sun and so must eventually pass

But it is desirable to know what sort of influence the Sun would exert even at distances beyond the limits of his direct control. Let us set ourselves to form adequate conceptions of the Sun's energy of gravitation.

The measure of all gravitation is that force which the Earth exerts as we know to draw bodies to her surface. We must therefore first ascertain what proportion the energy of the Sun's attraction bears to that familiar attraction exerted by the Earth.

For this purpose we may proceed either directly by comparing the amount of velocity which the Earth's attraction communicates to falling bodies, with the actual motion towards himself which the Sun causes in the case of any planet; or indirectly by comparing the motion of any planet round the Sun with the Moon's motion round the Earth. I select the second method as being the simplest.* I adopt also a way of apply-

wholly away from his dominion. It follows that, setting aside the above-mentioned influences, a comet which travels in an unclosed orbit round one star can never travel in a closed orbit round any star, but will continue to flit from star to star through all time.

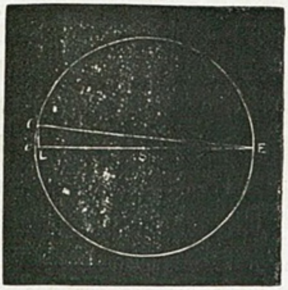
* It is worthy of notice, however, that the other method, though seldom applied, is in effect quite as available as the method in which reference is made to the Moon's motion. Thus we know that in one second a body falls about 16·1 feet towards the Earth, acquiring a velocity of twice this number of feet, or 32·2 feet per second. Now the Earth circuits the Sun at a rate of about eighteen miles per second, and supposing $e\,e'$ (fig. 19) to represent this distance on the orbit $e\,e'\,\mathrm{E}$, then $e\,\mathrm{L}$, obtained by drawing a perpendicular from $e'$ on the diameter $e\,\mathrm{S}\,\mathrm{E}$, represents the amount by which in effect the Earth has been drawn towards S, and a velocity of twice $e\,\mathrm{L}$ per second measures the Sun's gravity at the distance $\mathrm{S}\,e$. But by Euclid vi. 8, $e\,e'$ is a mean proportional between $e\,\mathrm{L}$ and $e\,\mathrm{E}$; in other words, $e\,\mathrm{L}$ is equal to $\frac{(e\,e')^2}{e\,\mathrm{E}}$; and therefore

ing it by which certain difficulties of conception are removed.

The Earth at a distance from the Moon of about 238,800 miles has power to change the direction of the Moon's motion through four right angles in 27·322 days, the Moon moving in her orbit round the Earth

the Sun's gravity at *e* is measured by a velocity of $\frac{(ee')^2}{e\,\mathrm{s}}$ per second, or in feet $\frac{(18)^2 \times 1760 \times 3}{91{,}500{,}000}$. Hence at a distance equal to the Earth's radius, the Sun (supposing all his mass collected at one point) would exert a force represented by a velocity of about $\frac{(18)^2 \times 1760 \times 3 \times 91{,}500{,}000}{(4{,}000)^2}$ feet per second, which reduces to 9,783,180 feet (or about 1,853 miles per second). Comparing this with the measure of the force of terrestrial gravity, or 32·2 feet, we see that the Sun's mass must be, according to this rough process, fully 300,000 times as great as the Earth's. As a matter of fact it is estimated at 315,000 times the Earth's, a result we should have closely approximated to had we taken the Earth's radius at 3,960 instead of 4,000.

Fig. 19.



The following general theorem is often useful. Take $r$ the Earth's radius, $\mathrm{E}$ her mass and $g$ terrestrial gravity, $\mathrm{R}$ the radius of a circular orbit described by a body of mass $m$, about a larger body of mass $\mathrm{M}$ with velocity $v$ (in feet per second). Then the attraction between the bodies, or the gravity of $m$ towards $\mathrm{M}$, is represented by the expression $\frac{v^2}{\mathrm{R}}$ and the mass $\mathrm{M} + m$ is equal to $\mathrm{E}\,.\,\frac{v^2\mathrm{R}}{r^2 g}$. If $m$ is relatively very small with a velocity which we may represent by $\frac{238,800}{27\cdot322}$.* Now the Sun at a distance from the Earth of about 91,500,000 miles has power to change the direction of her motion through four right angles in 365·256 days, the Earth moving in her orbit with a velocity which we may represent by $\frac{91,500,000}{365\cdot256}$. Now clearly, since gravity varies inversely as the square of the distance, the Sun would require (were other things equal) to have an attractive power greater than the Earth's in the ratio $\left(\frac{91,500,000}{238,800}\right)^2$ to produce the same effect on her that she produces on the Moon; and secondly, since to change the direction of a body's motion through any angle is a work which will be done at a rate proportioned to the force which operates, it is clear that the Sun's attractive power would have (were other things equal) to be less than the Earth's, in the ratio $\frac{27\cdot322}{365\cdot256}$, to accomplish in one sidereal year what the Earth accomplishes in one synodical month; while lastly, since the faster a body moves the greater the force necessary to deflect its course through a given angle, it is obvious that the Sun's attractive power would have (were other things equal) to be greater than the Earth's in the proportion of $\frac{91,500,000}{365\cdot256}$ to $\frac{238,800}{27\cdot322}$—that is, in the ratio $\frac{91,500,000\times27\cdot322}{238,800\times365\cdot256}$—to

(as in the case of the Earth compared with the Sun), and the radius of the larger mass is $\rho$, then gravity at the surface of the larger mass is equal to $\frac{v^2.\text{R}}{\rho^2}$.

* We need not trouble ourselves to determine the velocity in miles per second, or minute, or hour; because relative and not absolute velocities are in question. Hence we can represent the Moon's velocity by the radius of her orbit divided by the period, provided we represent the Earth's velocity in like manner.

produce a given change on the quickly moving Earth in the same time that the Earth produces such a change on the less swiftly moving Moon. Now we have only to combine these three proportions, which take into account every circumstance in which the Sun's action on the Earth differs from the Earth's action on the Moon, in order to deduce the relation between the Sun's real attractive energy, and the Earth's (at equal distances from the centre of either). This gives the proportion $\frac{(91{,}500{,}000)^3}{(238{,}800)^3} \times \frac{(27{\cdot}322)^2}{(365{\cdot}256)^2}$, which reduces to 314,798, in which proportion the Sun's mass exceeds the Earth's. We may take 315,000 (the value given in tables of the elements) as representing in round numbers the true proportion, which as it depends on the Sun's distance, cannot be determined so accurately that the last three figures of the number can be regarded as significant.

At equal distances, then, the Sun exerts 315,000 times as much force on any body as the Earth. So that if the Earth's mass were as great as the Sun's, her dimensions remaining unchanged, a mass which now weighs one pound, would weigh more than 14½ tons. A man now of average weight would be crushed down by a weight of more than 20,000 tons. A body, if raised but a single inch and let fall, would strike the ground with a velocity three times as great as that of the swiftest express train.

But now that we have thus ascertained the proportion which the Sun's attractive energy bears to that exerted by the Earth, and so are able to measure the

Sun's might as ruler over his system by direct comparison with the familiar force of terrestrial gravity, we must endeavour to form an estimate of the extent of force exerted by the Sun at different distances. We are to inquire what are the limits of the Sun's effective reign, not as regards distance alone, but as regards also the activity of matter,—that is, the velocity with which it is travelling.

We may begin with the Earth. We know that the Earth is completely subject to the Sun's attraction. Notwithstanding the inconceivable velocity with which she moves, and therefore the inconceivable energy of the tendency she has to travel onwards in a right line and so to free herself from the Sun's control, she is compelled to travel in a nearly circular course around him. At one time her velocity has reached its maximum and she has power for awhile to increase her distance from the Sun. But she has derived that very power from him. Anon her speed is reduced to its minimum, and then she is compelled slowly to approach the ruling centre. But throughout her course there is one constant relation from which there is no escape. The Earth's velocity and distance are the two quantities which measure the extent of the Earth's partial freedom. When one is reduced, the other is increased, and *vice versâ*, in such sort that there is absolutely no change in her condition regarded as depending on these two combined relations. The one law from which there is no escape, let distance and velocity change as they may during the Earth's circuit around the Sun, is that her period of revolution continues unchangeable,—and therefore her mean distance also.*

We may then take the Earth's mean distance as measuring her freedom from complete solar control—complete control being understood to be such an overwhelming influence on the Earth as would force her to fall directly upon the Sun. And while the Earth's mean distance thus measures her partial freedom, the shortness of the period in which she completes her circuit measures the amount of the Sun's power over her. Her mass may be regarded as having nothing to do with either relation. Increase of mass, so far as it would be effective at all, would tend to increase the strength of the bond uniting the Earth and the Sun, and to diminish the period of the Earth's circuit. But remembering that the Earth's mass is a very minute fraction of the Sun's, it may be disregarded wholly. A body no larger and heavier than a peppercorn, if projected with the same velocity and on the same course as the Earth would continue to travel in precisely the same path and period around the Sun.

We have next a law for our guidance which is of a very remarkable character, and the recognition of which will be found to throw a most important light

---

* Or we may say in preference that the one law from which there is no escape is the law connecting the Earth's velocity $v$ at any time, with her distance $R$ from the Sun at that time, and a certain fixed quantity $A$ which is her mean distance. This law is thus expressed mathematically: $v^2=\mu\left(\frac{2}{R}-\frac{1}{A}\right)$, where $\mu$ is the accelerating force of the Sun at the unit of distance.

on all the relations of the planetary scheme. It is this: Given the distance of a body from the Sun, and the velocity with which the body is travelling, then—let the course of the body be what it may (so long only as it does not bring the body into actual contact with the Sun), the period of the body's revolution is assigned.

The Earth's greatest velocity and her least correspond therefore—as truly as her mean velocity—with her period; and further, we need not trouble ourselves about the direction of her motion at any time, for if this direction were altered by the action of some external force, while yet her velocity remained unchanged, she would continue to travel in the same periodic time around the Sun, and at the same mean distance.

So that if we take the Earth's greatest velocity when she is in perihelion (18·5 miles per second), we have the velocity which is necessary in order that a body about 90,000,000 miles from the Sun may travel once in a year, or at a mean distance of 91,500,000 miles, round the sun; and if we take her least velocity (17·9 miles per second) when she is in aphelion, we have the velocity which is necessary in order that a body about 93,000,000 miles from the Sun may travel once in a year round the Sun; while the Earth's mean velocity (18·2 miles per second) at her mean distance is the velocity necessary in order that a body at that distance may have a period of one year.

We will now take only the mean distance and the mean velocity. We see that at a distance of 91,500,000 miles a body requires a velocity of 18·2 miles per second if it is to have a mean distance of the same amount. So that clearly if it were projected square to a line from the Sun it would never change its distance; for it would have exactly the right velocity and exactly the right direction for travelling in a circle around the Sun. The Earth when at this distance, though travelling with the right velocity for the required mean distance, is not travelling square to a line from the Sun, and so does not travel in a circle around him. But we have learned from her motion what is the just rate at which a body should be projected so as to travel in a circle round the Sun at a distance of 91,500,000 miles.

Now how much must this velocity be increased in order to enable a body at this distance of 91,500,000 miles to pass wholly from the control of the sun? If we can determine this we shall have determined the limits of the Sun's influence at this particular distance. Over bodies moving with a velocity below that limiting velocity he is completely master; let them travel onwards as they may, increasing their distance from him more and more, there is yet a limit to this increase. Their absolute velocity will become less and less; it will become at last such, that if their direction of motion were but changed they would thenceforth continue to describe a circle around the Sun; still it will go on diminishing, until at length they reach their extreme range of distance, after which they will be brought back through all the orders of distance they have passed through, and finally return to the place they started from, to pursue the same round for ever. But if their velocity do but equal or exceed the limit we are dealing with, they will travel onwards—with ever-diminishing velocity, it is true, but still—with ever-increasing distance, *for ever*.

It might be supposed that a very great increase of velocity would be required in order that the Earth should be thus (unfortunately for her inhabitants) released from the Sun's service and sent to wander freely through space. But in reality it would not even be necessary that her velocity should be doubled; an increase by one half would be more than sufficient to free the Earth for ever from enforced periodic revolution around the centre of our planetary scheme. The exact proportion of increase necessary to effect this is represented by the proportion in which the diagonal of a square exceeds the side,* which we know to be repre-

---

* The following simple formula conveniently expresses the relation between the mean distances $a$ and $a'$ of bodies which at a distance $r$ from the Sun are travelling with the velocities $v$ and $v'$ respectively:

$$a'(2a-r) : a(2a'-r) :: v^2 : v'^2.$$

Now supposing the velocity $v$ to be such that a circle would be described about the centre of motion, if the body travelled square to the line from that centre, then obviously $a$ is equal to $r$; so that the above relation becomes

$$a' : 2a'-r :: v^2 : v'^2;$$

and therefore

$$a' : r :: v^2 : 2v^2-v'^2.$$

Now clearly if we increase $v'$ until $v'^2$ is equal to $2v^2$, we make the fourth term gradually diminish until it vanishes; so that the third at last bears an infinitely great proportion to the fourth. But in this case the first will bear an infinitely great proportion to the second; in other words, $a'$ will be infinitely great. Hence the period of the body will have become also infinite (by Kepler's third law): or in other words the body will never return to its starting-place. Therefore, it follows that sented numerically by the proportion in which 1414 (pretty nearly) exceeds 1000.

So that if the Earth when at her mean distance had her velocity suddenly increased from 18·2 miles per second, to 25·7 miles or thereabouts, we should be carried thenceforth continually farther and farther away

---

if $v$ be the velocity with which a body will describe a circle at a distance $r$ from the Sun, the greatest possible velocity which a body can have at a distance $r$ from the Sun, so as to travel on a closed orbit around him is $v\sqrt{2}$; and if a body is observed to travel with any greater velocity at such a distance we know with certainty that that body has entered the domain of the Sun, with a velocity imparted to it by extra-solar influences.

If in the above proportion $v'^2$ is greater than $2v^2$, we see that $a'$ must have a negative value. This means that the centre of the path described by the body lies in the direction contrary to that in which the Sun lies. And knowing that the path must needs be a conic section with the Sun in a focus, it follows that the path of the body must be hyperbolic, the Sun lying at that focus which is next to the branch traversed by the body.

Of course in this case as in the former the body will never return. But there is a noteworthy distinction between the two cases. When the axis of the orbit is infinite, the body describes a parabola, and if it could be traced from the Sun as it approached from an indefinitely great distance to its nearest point and then passed away again to an infinitely great distance, the point to which it seemed to pass away would be precisely the same as that from which it seemed to come, and these coincident points would lie directly opposite the point of nearest approach. (This is obvious, because if lines be drawn from the focus of the parabola to two points of the curve equally and enormously removed from the vertex, these lines will enclose an indefinitely small angle and will approach indefinitely near to coincidence with the axis.) On the other hand, a body approaching and then passing away on a hyperbolic orbit will seem to come from one point of the heavens and to pass away to a different point, the bisection of the celestial arc between these points lying directly opposite the point at which the body makes its nearest approach; and for a given distance of this nearest point, the arc separating the two former will be the greater as the velocity of the body when at its nearest is greater; becoming equal to two right angles when this velocity is infinitely great.

from the light and life of the planetary scheme. From Mars and Jupiter, and perchance from Saturn, Uranus, and Neptune, the unhappy career of the Earth might be traced for many a long year (though years—at least terrestrial ones—would then be no more). But long before the Earth crossed the confines of those distant regions along which the outer planets pursue their career, all the higher forms of life would have vanished from her surface. She would still rotate; day and night would still succeed each other on her surface; but the orderly sequence of the seasons would be replaced by the continual diminution of solar light and heat, until a cold more intense than that of the bitterest Arctic winter would bind the world in everlasting frost.

A similar fate would befall us if the Sun's mass were suddenly reduced in the proportion of about 1,000 to 1,414; the only difference being that in this case we should have companions in our troubles, for Mars and Venus and Mercury would all forthwith start on parabolic paths, carrying them away to infinite distances from the Sun. Nor would the larger planets escape. From their distant orbits they would rush off into outer space, carrying their systems of satellites with them; so that if I have been right in regarding these orbs as acting the part of secondary suns to their satellites, the latter would be less unfortunate in their fate than the four minor planets, for these would have no sun at all, while the former would still enjoy such heat and light as their ruling centres could supply to them.

It is not without a purpose that I have thus dwelt on the general result of a sudden diminution of the Sun's mass. The consideration that all the planets would thus at once be freed from their allegiance if the Sun's mass were reduced, leads us to the consideration that each planet's velocity need but be increased in the proportion of about 1,414 to 1,000, to lead to a similar result. And thus we see that the Sun's influence at the distances of the successive planets is limited to the control of bodies moving with a velocity bearing such a relation to the velocity of the respective planets. So that we have only to draw up a table of the distances and mean velocities of the planets, and to increase the latter quantities in the proportion of about 1,414 to 1,000, in order to have a representation of the gradual diminution of the Sun's influence at greater and greater distances. The table runs thus:—

| Planet | Mean distance in miles | Mean velocity in miles per second | Velocity increased as 1,414 to 1,000 |
|---|---|---|---|
| Mercury . . . . . . | 35,392,000 | 29·3 | 41·4 |
| Venus . . . . . . | 66,134,000 | 21·4 | 30·3 |
| The Earth . . . . . | 91,430,000 | 18·3 | 25·9 |
| Mars . . . . . . . | 139,311,000 | 14·7 | 20·8 |
| The Asteroids . . . . | 250,000,000 | 11·0 | 15·5 |
| Jupiter . . . . . . | 475,692,000 | 8·0 | 11·3 |
| Saturn . . . . . . | 872,137,000 | 5·9 | 8·3 |
| Uranus . . . . . . | 1,753,869,000 | 4·2 | 5·9 |
| Neptune . . . . . | 2,745,998,000 | 3·3 | 4·7 |

We see from this table that if the three outermost planets could only have imparted to them the velocity of the minor planets, they would be freed from their allegiance to the Sun, and pass away on hyperbolic orbits. But with the exception of Uranus there is no planet which would be thus freed (absolutely) if it had imparted to it the velocity of the *next* inner one.*

But the above table has only been presented by way of introducing a more general law. What the table teaches us respecting special distances we can determine for all distances from the Sun, by a simple application of Kepler's third law and its results.

Thus, suppose we wish to determine the maximum velocity which the Sun can control at a distance half that of the planet Mercury. Then the law that the

---

* It is a rather singular circumstance that the maximum velocity which the Sun can control at the distance of any one of the four outer planets should correspond so closely as it does with the actual velocity of the planet whose orbit lies next within. We see that if Neptune could have the velocity of Uranus, he would be almost wholly freed from his allegiance; Uranus would be just freed if he had the velocity of Saturn; Saturn would be almost wholly freed if he had the velocity of Jupiter; while Jupiter would be almost wholly freed if he had the velocity of those asteroids which travel at a mean distance. And there is a tendency, though less marked, to the same relation among the remaining planets. Remembering that the velocity a planet would require for freedom is that with which a body approaching on a parabolic orbit from an infinite distance would pass the mean distance of that planet, we have throughout the solar system a tendency (very marked among the outer members) to this remarkable relation, that the velocity with which a body approaching from infinity would cross the orbit of any planet should be the same as the actual velocity of the next inner planet. It need hardly be said perhaps that this relation directly results in the law to which Bode's law approximates for the outer planets. Thus, if the outermost planet had a distance D and a velocity V, the next minor planet would have a velocity $V\sqrt{2}$ corresponding to a mean distance $\frac{D}{2}$, and so on. But in the law of the duplication of the distances outwards there is no direct physical significance, whereas it is possible to conceive that the law as presented above—that is, regarded as associated with the velocities—may be associated also with the processes by which the solar system has reached its present condition.

cubes of the distances are as the squares of the periodic times shows us that the period of a planet at such a distance would be to Mercury's as 1 to the square root of 8 (or twice the square root of 2). Since, then, the actual circuit of such a planet would be half that of Mercury, its velocity would exceed Mercury's in the proportion which the square root of 2 bears to 1,* or about 1,414 to 1,000. This would correspond to a velocity of 41·4 miles per second; and the greatest velocity the Sun could control at this distance would be obtained by increasing this velocity of 41·4 miles per second in the proportion of 1,414 to 1,000. It would therefore be 59·6. This shows how we can measure the Sun's controlling energy for any distance. But it also establishes a very important general relation. It appears that when we halve the distance, we have, in order to determine the velocity which the Sun can control, to increase the velocity at the greater distance in the proportion of about 1,414 to 1,000. And therefore when we take one-fourth of the distance, we must increase the velocity *twice* in this proportion. But this amounts to doubling the velocity, since this proportion is that of the square root of 2 to unity. Hence we have this general rule, that the velocity which the Sun can control is doubled when the distance

---

* This illustrates the general law that if two planets have mean distances $d$ and $d'$ respectively, and mean velocities $v$ and $v'$ respectively, then

$$v : v' :: d'^{\frac{1}{2}} : d^{\frac{1}{2}}.$$

Clearly this is so, since the periodic times are as $d^{\frac{3}{2}}$ to $d'^{\frac{3}{2}}$, and the velocities, therefore, as $d \div d^{\frac{3}{2}}$ to $d' \div d^{\frac{3}{2}}$.

is reduced to one-fourth, and we can see at once how enormously the velocity must increase in the Sun's immediate neighbourhood.*

Thus, at a distance of 8,848,000 miles (one-fourth that of Mercury), the velocity the Sun can control, so as to compel a body to move in a closed orbit around him, is 82·8 miles per second; at a distance of 2,212,000 miles it is 165·6 miles per second; at a distance of 553,000 miles it is 331·2 miles per second. But this brings us very close to the Sun's surface—since it is from his centre all our distances are measured—and his radius is about 426,450 miles. The actual velocity at his surface—that is, the velocity which he could just control so as to compel a body to travel in a closed orbit just touching his surface—is easily obtained from the formula given in the preceding note. It is no less than 378·9 miles per second; and this is the least velocity with which a body must be projected from the Sun in order that it may never return to his globe again.

---

* It need hardly be said that this result might have been obtained directly from a consideration of the law according to which gravity diminishes with distance. But apart from the fact that the mere dry reasoning by which the result would have been established would have had little interest to the general reader, the particular path which I have selected to follow has the advantage of introducing a number of independent relations, and of showing how the various matters dealt with bear upon each other and upon the general subject of the chapter.

The general law connecting distance with the velocity which the Sun can control is as follows:—Let $d$ represent the Earth's mean distance; $v$ her velocity at that distance in miles per second; D any other distance. Then the velocity which the Sun can control (so as to compel a body to travel in a closed orbit round him) at a distance D is

$$v\sqrt{\frac{2d}{D}}$$

Thus the Sun, which at the distance of Neptune can control a velocity of but 4·7 miles per second, can control close by his own surface a velocity eighty times as great. At a distance four times as great as Neptune's, a velocity of 2·4 miles per second would suffice to enable a body to pass away to an infinite distance from the Sun. But even this velocity is enormous and almost inconceivable. If we seek to know in what regions the Sun could barely control velocities such as we are familiar with (by which I mean such velocities as the eye can appreciate, not velocities—as of cannon balls or the like—which we speak of without appreciating) we shall find that such regions lie at enormous distances. A body moving at the rate of our swiftest express trains (say 60 miles per hour) would be compelled to travel in a closed orbit around the Sun unless its distance from him were no less than 220,507,500,000,000 miles, a distance some ten or twelve times exceeding that of the star Alpha Centauri.*

We have seen that the Sun can control the motions of a body travelling with a velocity of less than 378·9 miles per second close by his surface in such sort as to compel that body to travel in a closed orbit around him,

---

* At half this distance a planet travelling in a circle round the Sun would have a velocity equal to that of an express train; but if such a planet travelled in the plane of the ecliptic it would be unable to complete a circuit round the Sun on account of the disturbing influences of the star Alpha Centauri, which, being at only one-fifth of its distance from the Sun, would lie nearer to some parts of its circular orbit than the Sun does.

always coming close by him at each return. That is the maximum velocity a body can have under such circumstances. It is also the greatest velocity a body can acquire in approaching the Sun, under his attraction alone, from an infinite distance and starting from rest. Now, the least velocity a body can have so as to travel close by the Sun is clearly that which would allow the body to travel in a complete circle around the Sun and close to his surface. We obtain this by simply reducing 378·9 in the proportion of 1,000 to 1,414; the required velocity is therefore 268·0 miles per second.* Such are the limits between which the velocities of all bodies travelling around the Sun so as just to graze his surface must necessarily lie.† Nor can any mass reach

---

* It may perhaps be necessary to point out that in a statement of this sort, 268·0 is not strictly the same as 268. The former implies that the velocity lies between 268·05 and 267·95; the latter would imply that the velocities lie between 268·5 and 267·5.

† It may be interesting to inquire how far from the Sun a body would reach if projected vertically with a velocity which would just enable a body projected horizontally to complete the circuit of the Sun. The problem is exceedingly easy. Such a body would move as if travelling on an orbit having a mean distance equal to the solar radius. Conceiving the Sun's mass all collected at his centre, the body would, after passing to its greatest distance, return to the centre, round which it would circle with (for a moment) an infinite velocity, and so return to its aphelion. The aphelion distance, therefore, from the centre must be equal to the Sun's diameter, and the greatest distance attained from the Sun's real surface must be equal to the solar radius, or more than 425,000 miles. The rotation is not here considered. It would not, however, affect the distance attained by the body, though it would affect the real path by which that distance would be attained. We can in like manner determine the distance to which a body projected with any other velocity would reach. For we can determine by means of the formula in the note at page 84 what the mean distance corresponding to this velocity at such a distance from the Sun would be, and so (as

him from without, whether gathered directly out of space by his attraction, or reaching him after passing through any number of forms of orbital motion due to the perturbing influences of the planets, with a velocity exceeding 378·9 miles per second, unless such a mass had already had motion communicated to it before the Sun began to act upon it. In like manner, no body can reach any given distance from the Sun with a velocity greater than that deduced according to the above-discussed considerations, as the limiting velocity the Sun can control, unless that body had started on

---

before) we can determine the greatest height attained by the projectile. But there is a simple and convenient formula for this purpose, thus:— Let $V$ be the velocity of projection, $g$ the accelerating force of gravity at the Sun's surface, $R$ the Sun's radius, and $H$ the height attained by the body; then

$$V^2 = 2gH\frac{R}{R + H}.$$

If the reader prefer a formula in which the quantity $g$ does not directly appear—so as to avoid the necessity of considering the value of terrestrial gravity (from which $g$ is deduced) measured with reference to feet and seconds—we can easily get a convenient formula. Thus, we have

$$V^2 = 2gH\frac{R}{R + H}$$

and therefore from what has just been shown it follows that

$$(268\cdot 0)^2 = 2gR\frac{R}{R + R}$$

(a mile being supposed to be the unit of length in the value of $g$ and a second the unit of time). Therefore, dividing, we have

$$V^2 = 2\,(268\cdot 0)^2\frac{H}{R + H}$$

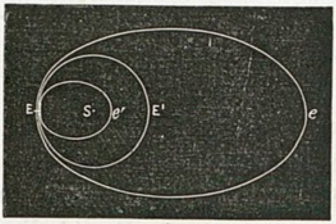
(very convenient for finding $V$ when $H$ is given); and since $2(268\cdot 0)^2$ is equal to 143,648, we have, for finding $H$ when $V$ is given

$$H = \frac{V^2 R}{143{,}648 - V^2}.$$

its journey towards the Sun with a velocity communicated beforehand, and by the influence of other attracting orbs. So that if it has been demonstrated, as many believe, that some of the meteors which reach our atmosphere travel through it with a velocity greater than 25·9 miles per second, superadded to the velocity with which the Earth meets them (which, according to the direction of impact, may have any value up to 18·5 miles per second), then that body comes to us with an intersidereal velocity, so to speak, in addition to such velocity as the Sun has communicated. For example, if a meteor penetrate our atmosphere with a velocity of 50 miles per second, coming full tilt against the Earth when she is near perihelion, then 5·6 miles per second at least of that velocity was communicated to the meteor by other orbs than our Sun.

It will naturally occur to the reader to inquire at this point what is the least velocity with which a meteor can travel when at the Earth's distance from the Sun. For clearly if meteors moving with velocities exceeding the greatest which the Sun's attractive energies can give, afford information of an extra-solar force to which such meteors have been subjected, so also meteors moving more slowly than any which could circle around the Sun would have to be regarded as indicating the action of some other force than solar attraction. Now, if we suppose a body travelling in the Earth's orbit, and as fast as the Earth, to have its velocity *increased*, we have seen that its orbit must needs have its greater axis increased, and will become parabolic if the increase of velocity be great enough. In fact, an increase of velocity makes the point where the increase takes place the perihelion of the new orbit. And obviously a decrease of velocity will make the point where the decrease takes place the aphelion of the new orbit. Thus, suppose the body is at E (fig. 20) and travelling along the Earth's orbit E E′ about the Sun at S, when it suddenly receives an impulse onwards, then its new orbit will have such a shape as E *e*, E being the perihelion. But if the body is checked when at E, the new orbit will be of such a figure as E *e*′, the aphelion being at E. And the question we have to determine is, how great the reduction of velocity can be in order that the revolution of the body in an orbit may not be prevented. Clearly there is no limit to the reduction except that resulting from the size of the Sun. If the body can but pass clear round the Sun's globe, it will return to the point E, and (neglecting perturbations or resistance as it passes through the Sun's atmosphere) it will continue to circle in this way for ever. We have to determine what velocity at E is necessary for this purpose; and obviously, as a

FIG. 20.



body moving more slowly at E would not pass clear of the Sun at S, so also, tracing its path back, it could not have arrived along a course clear of the Sun's globe; and the only interpretation of its motions would be *that it had been projected angularly from the Sun's surface with sufficient velocity to reach the Earth.* It is not difficult to calculate the least velocity with which a body could travel when at the Earth's distance on an orbit just grazing the Sun's surface.* This velocity is

---

* There are many ways of attacking the problem. The best, perhaps, for our present purpose is the following:—

It is obvious that if a body be projected tangentially to the Sun's surface with such velocity as to just reach the Earth's orbit when at its greatest distance from the Sun, it would continue to revolve in such an orbit as we are inquiring into. Now, in order to determine the velocity of projection requisite for such a result, we may apply the formula in the preceding note, remembering that if a body projected vertically upwards from the Sun would reach to a height H, a body projected horizontally with the same velocity would travel in an orbit having a mean distance equal to $\frac{1}{2}$ (H + R) where R is the Sun's radius; so that we require our body to be projected tangentially with such velocity as would be required to project a body vertically to a height equal to the Earth's mean distance D (say) from the Sun's *centre*, since the required orbit is to have a mean distance equal to $\frac{1}{2}$ (D + R). Hence, by the formula for v in the preceding note we have

$$v^2 = 2(268{\cdot}0)^2 \frac{91,500,000}{91,925,000} \text{ (approximately)};$$

whence we find the required velocity of tangential projection equal to 359 miles per second; and we note in passing that such a velocity as this would be required to project a body from the Sun to the Earth's distance. Now, this is the perihelion velocity of our projectile, when at a distance of 425,000 miles from the Sun's centre. In aphelion, its distance is 91,500,000 miles (approximately); hence from Kepler's first law its velocity there will be

$$359 \frac{425,000}{91,500,000} \text{ miles per second,}$$

or about 1·85 miles per second, which is almost exactly one-tenth of the Earth's velocity in her orbit, but considerably exceeds the Moon's.

about one-tenth of that with which the Earth moves in her orbit, or 1·85 miles per second; and no body can possibly reach the Earth with a smaller real velocity than this, unless actually projected from the Sun. A smaller *relative* velocity might very well be observed however. For example, a meteor travelling very little faster than the Earth might overtake her, and so seem to enter her atmosphere very slowly. And observations seeming to indicate that the real velocity of a meteor was less than 1·85 miles per second, would need to be very strongly confirmed before they would be accepted by astronomers; since to reach the Earth *a body must be projected from the Sun with a velocity of about* 360 *miles per second.*

Here, having already passed the limits I had proposed to myself in dealing with the subject of this chapter, I draw it somewhat regretfully to a close. It would have been easy to extend the chapter so as to have occupied a volume twice the size of the present, and yet to have discussed no well-worn facts. The whole subject of the Sun's rule over the space surround-

This velocity would, however, be somewhat increased by the Earth's attraction. It is to be noticed, that the Moon's velocity in her orbit being about $\frac{3}{5}$ths of a mile per second, the greatest velocity the Earth can control at the Moon's distance is but about $\frac{17}{20}$ths of a mile per second; so that no body moving to the Earth's neighbourhood under the influence of the Sun's attraction can by any possibility be compelled by the Earth to travel in an orbit around her unless it comes much nearer than the Moon. The maximum velocity which the Earth can control close by her surface is, however, about seven miles per second; so that bodies having the aphelion of their orbit nearly at the Earth's distance and the perihelion close to the Sun's surface, could, if they happened to come close by the Earth, be compelled to circuit in an orbit round her.

ing him has remained in a sense almost untouched, until lately its significance began to be recognised by Mayer, Thomson, Waterston, and others. Even these, however, have dealt rather with the limits of activity possessed by bodies close to the Sun's surface than with the velocities which measure his influence at greater distances. For my own part, I find a wonderful interest in the ideas suggested by the Sun's activity throughout the whole range of his wide domain. The velocities to which I have referred in this chapter as those which the Sun can control are also those which he can generate. We have to think of him, therefore, as capable of drawing towards himself all such cosmical matter as comes under his exclusive attraction either by leaving the domain of some other star, or on account of his own motion through space. In so drawing cosmical materials towards himself, he imparts to them velocities such as we have been considering in the present chapter. The vaster the distances from which they come, the greater the velocities he imparts to them. As they sweep onward in their course they are subject to the influences of the planets—the patrols of the solar system—and under such disturbing influences large numbers must be compelled to follow either temporarily or permanently true orbits (that is, closed curves) around the Sun. But the majority of such visitants, whether comets or meteors, must return to the sidereal depths after once paying their respects —in the full rush of their perihelion swoop—to the giant ruler of our system. It seems probable that in this continual rush of matter, this continual interchange of attendants on suns and stars, we may recognise the progress of processes exercising a most important influence on the welfare of planetary systems. It is still more probable that the bodies which are finally drawn into the solar domain perform highly important functions in the economy of our own particular planetary scheme. But unless I mistake, the real significance of the considerations we have dealt with in the present chapter lies in their bearing on the past history of the solar system. The rush of matter which we now recognise affords perhaps but the faintest indication of the amazing conflicts in which our system had its birth. Tracing back the history of that system, we seem to recognise a time when the Sun's supremacy was still incomplete, when the planets struggled with him for the continually inrushing materials from which his substance as well as theirs was to be recruited. We can see him by the mighty energy of his attraction clearing a wide space around him of all save such relatively tiny orbs as Venus and the Earth, Mars, Mercury, and the asteroids. With more distant planets the struggle was less unequal. The masses which flowed in towards the centre of the scheme swept with comparatively slow motion past its outer bounds, so that the subordinate centres there forming were able to grasp a goodly proportion of material to increase their own mass or to form subordinate systems around them. And so the giant planets, Jupiter and Saturn, Uranus and distant Neptune, grew to their present dimensions; and became records at once of the Sun's might as a ruler—for without his overruling attraction the material which formed these planets would never have approached the system —and of the richness of the chaos of matter from which his bulk and theirs were alike evolved. Nor is the consideration without a mysterious attraction that in thus looking back at the past history of our system we have passed after all but a step towards that primal state whence the conflict of matter arose. We are looking into a vast abysm, and as we look we fancy we recognise strange movements, and signs as if the depths were shaping themselves into definite forms. But in truth those movements show only the vastness of the abysm, those depths speak to us of far mightier depths within which they are taking shape. 'Lo! these are but a portion of His ways; they utter but a whisper of His glory.'

## CHAPTER III.

### ANALYSING SUNLIGHT.

THE researches of telescopists have revealed many important facts respecting the Sun's constitution. Studied thoughtfully, these researches enable us to answer many questions which at first sight seem to require more powerful modes of inquiry. But, undoubtedly, the science of solar physics is too vast and too difficult to be satisfactorily treated by telescopic research alone. The condition of the Sun is so different from that of any bodies we can experiment on, that his mere aspect —and the telescope can show us nothing more—is insufficient to tell us what his constitution may be. The picture of the Sun presented by the most powerful telescope resembles a book, full of meaning indeed, but written in an unknown language. The spectroscope is the means by which that unknown language has been in part interpreted.

Let us consider what spectroscopic analysis really is. It is scarcely possible to treat of any astronomical subject in the present day without describing the most powerful of all instruments of astronomical research; but in the case of the Sun it would be hopeless to attempt such a course. Spectroscopic analysis forms the very basis of all our ideas respecting solar physics. We must thoroughly understand the mode in which it teaches, the meaning of its teaching, and the extent to which its teaching may be relied on, otherwise our views will be vague and unsatisfactory, depending rather on the statements of others than on any clear apprehension of their truth on our own part.

For this reason I shall spare no pains to make the explanation of spectroscopic analysis which follows as simple, and, at the same time, as complete as possible. There is, in reality, nothing difficult in the subject; but it cannot be denied that, considering its simplicity, it is not nearly so well or so widely understood as it might be,—a circumstance the more to be regretted because the whole history of recent scientific researches is a sealed book to those who do not clearly recognise the nature of the instrument by which those researches have been effected.

Newton was the first who showed that white light is a compound of light of many different colours.* He proved this by his investigation of an experiment of Grimaldi's—illustrated in fig. 21. Here A B represents the course of a pencil of solar light† passing through

---

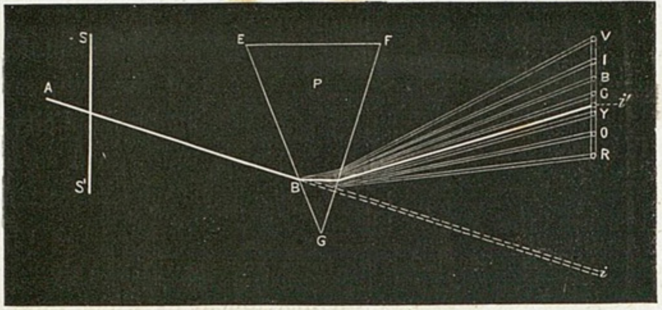
* Grimaldi was, however, the first who discovered the effect of passing sun-light through a prism. (See his *Physico-mathesis de lumine*, prop. xxx. et seq.)

† I have purposely modified Newton's figure, because many misunderstand a figure in which the pencil A B is shown with its proper divergency. They confuse the dispersing effect of the prism with the optical effects produced on a diverging pencil of *pure* light. It need hardly be said that it would be quite as great a mistake to neglect the

a circular aperture in a screen S S′. The prism P is so placed as to intercept the light. It will be well to consider the prism as placed with the base E F uppermost and horizontal.*

Now, if the prism were removed, the light would fall at *i* and make a small elliptical image there. And if the solar light were simple instead of being composed of rays of many different colours, it would follow such a course as is indicated by the bent bright line, and form a small elliptical image at *i′*—this image being white like that at *i*, and resembling the latter image in shape.†

Fig. 21.



---

effect of divergency altogether. In fact, many important optical considerations are associated with the divergency (however small this may be made) of the pencil analysed by the prism. But the two matters are best kept apart, at least in the case of the beginner. In the above description the object has been to direct the reader's special attention to the dispersive action of the prism, and therefore no attention is paid to the divergency of the pencil.

* E G is supposed equal to G F, so that these lines are equally inclined to the vertical.

† It would indeed be absolutely identical in shape with the image at *i* if the exact course indicated in the figure were followed; but if the

But instead of this, Newton found, as Grimaldi had found before him, that a streak of light was formed as V R (V R is exaggerated in length), the streak being violet at the highest point and thence changing through indigo, blue, green, yellow, and orange, to red at the lowest point. Neither above nor below was the streak well defined, but passed gradually into darkness. At the sides, however, the streak was well defined, and in breadth equal to the horizontal breadth of the figure at *i*. It thus formed a rainbow-tinted streak or ribbon.

It appears from this experiment that light consists of rays of all the colours of the rainbow, that the violet rays are the most bent by the action of a prism, the red rays least, the others in the order named above. This happens with prisms of all refracting angles and of whatever substance. Hence the rays forming the violet part of the spectrum are often called—without further description—the most refrangible rays, while the red rays are called in the same way the least refrangible rays. This mode of speaking, and the expressions rising out of it, should be carefully noted.

Now, the streak of light seen by Newton showed no breach of continuity. Newton appears to have suspected the possibility that by a change in the conditions of his experiment the streak would show gaps. In other words, he suspected that light of all degrees of

---

single image fell at V or R this would not be the case. It need hardly be remarked that for pure light the course of the beam would depend on the refracting angle G of the prism.

refrangibility, between the light which forms the extreme violet and the extreme red of the streak, may not be present in the solar beam. But he did not succeed in proving this, though he employed apertures of different shape whereby to admit the light. It is clear that if there were simple violet light, and simple indigo light, and so on in the solar beam, a succession of small coloured images would be formed as shown in the figure at V, I, &c., and between these images dark spaces would be seen. Newton's experiments led him to the conclusion that an infinite number of images, shifting by indefinite gradations from V to R, exist along the streak, and so cause the colour to vary insensibly from violet to red as observed in his first experiment.

Wollaston was the first who succeeded in showing that there are gaps in the spectral streak.

It is clear that the circular aperture in the above experiment is not suitable for determining whether rays of all degrees of refrangibility are included in a beam of solar light. In fig. 21 an image of the aperture is represented at V, another at I, another at B, and so on; though of course the spectrum in Newton's experiment showed no such separate images. Now, it is perfectly obvious that if instead of the seven images represented in the figure there were twenty or thirty along the spectrum V R, there would be no means of knowing that the spectrum was made up of these twenty or thirty distinct images, for they would overlap, and so show a continuous streak of light. Wollaston found that when, instead of a circular, triangular, or oblong aperture, a very narrow slit is employed, light of certain degrees of refrangibility is absent from the solar beam. He admitted the light through a narrow slit (parallel to the refracting angle of the prism). With this arrangement the spectrum seen by Wollaston was not continuous, but crossed by two dark lines parallel to the slit—or, in other words, at right angles to the length of the spectrum.

These two lines—two *gaps* in the solar spectrum—proved that light of two definite orders of refrangibility is absent from the solar beam. Fig. 22 shows how the light, after passing through the prism, had become divided into three parts, with spaces between them along which no light travelled. It is quite obvious that the existence of these gaps can be recognised without allowing a spectrum to be formed on a screen, as in Newton's experiment,—simply by placing the eye as shown in fig. 22. It was in this way that Wollaston observed the two gaps. It may be remarked in passing that this mode of viewing the spectrum bears the same relation to Newton's plan that observation of the Sun with the naked eye bears to observation of the Sun's image received upon a screen.

Fig. 22.

Dr. Wollaston did not pursue the inquiry further. Nor need we greatly wonder at this, if we rightly consider the matter. We now know, indeed, that in the two dark spaces on the spectrum of Wollaston there lay the germ of the most wonderful discoveries man has yet made. We know that had he persisted in the inquiry his name would have been associated through all time, as that of Fraünhofer will undoubtedly be, with the very language of the new analysis. But it must be admitted that Wollaston had little reason for expecting any very remarkable results from the study of a peculiarity which seemed quite as likely to depend on the nature of the glass of which the prism was made as upon any inherent property of solar light. And even supposing that the gaps were due to some peculiarity of solar light, who could suspect that that peculiarity, when traced to its source, would be so full of meaning as to reveal to us the very constitution of the solar orb?

It is to the unwearying patience with which Fraünhofer—like so many others of his countrymen—was willing to work day after day at what seemed a most unpromising subject, that the world owes the first complete recognition of the characteristic peculiarities of the solar spectrum. Wollaston had observed the spectrum directly, with the unaided eye. Fraünhofer improved on this plan by employing a telescope.*

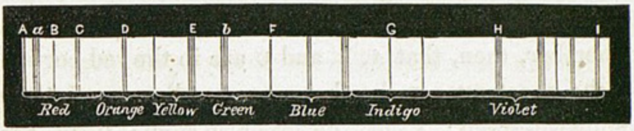
---

* It is well to notice that all the modes of viewing the Sun are available for viewing the solar spectrum. A prismatic spectrum is simply a series of images of a luminous object formed by rays of different refrangibilities. Where the luminous object is a line of light—as where light is received through a fine slit—the solar spectrum is in reality made up of an infinite number of lines of light at right angles to its length. It is because light of certain definite orders of refrangibility is wanting that images of the line are wanting at certain definite parts of

Dr. Wollaston had seen but two gaps in the solar spectrum; Fraünhofer, in 1814, saw and mapped no less than 576 lines. The positions of the chief lines seen by Fraünhofer are indicated in fig. 23, and as reference is continually made to the lettered lines, it is well that the student should carefully study their sequence and position.

A is a well-marked line close to the limits of visibility at the red end of the spectrum. B is a well-defined red line of sensible breadth.* Between A and B is a band of several lines called *a*. C is a dark and very well-marked line. Between B and C Fraünhofer counted nine fine lines; between C and D about thirty. D consists of two strong lines close together. Between D and E Fraünhofer counted eighty-four lines. E is a band of several lines, the middle line of the set being stronger than the rest. At *b* are three strong lines,

Fig. 23.



---

the spectrum,—that, in other words, dark lines are seen. The clear recognition of this fact will prevent much misapprehension.

* It will be understood that I am here describing the lines as seen by Fraünhofer, and as they would appear with similar spectroscopic power to that employed by him. With greater power many single lines are resolved into several, and many new lines make their appearance. For example, D is described above as consisting of two strong lines with an extremely small interval. With a powerful spectroscope numerous lines are seen between these two strong lines.

the two farthest from E being close together. Between E and *b* Fraünhofer counted twenty-four lines, and between *b* and F more than fifty. F, G, and H are strong lines. Between F and G, and between G and H, Fraünhofer counted 185 and 190 lines respectively; and he found many lines also between H and I—the violet end of the spectrum.

Fig. 23 shows the colours of those parts of the spectrum in which the several lines occur. The reader will do well to bear in mind the position of the several lines, as thus, by an easily remembered relation, he will find himself enabled to interpret readily the accounts of spectroscopic researches, whether into astronomical or chemical subjects of inquiry. Let him remember, then, that A, B, and C are in the red portion of the spectrum; D in the orange-yellow; E in the yellowish-green; F in the greenish-blue; G in the indigo; and H in the violet.

Fraünhofer's contributions to the science of spectroscopic analysis did not conclude, however, with the recognition and mapping of these lines. Having first convinced himself that the same lines were seen in the solar spectrum, of whatever substance the prism was formed, he proceeded to study the spectra formed by light from other sources.

He first examined solar light received indirectly by reflection or otherwise—as from the clouds, the sky, the Moon or planets, and so on; and he found in the spectrum of such light the same lines which he had seen in the spectrum of direct solar light. He studied the spectrum of the Sun when that orb is near the horizon, and he found that under such circumstances the violet end of the spectrum disappears and a number of lines make their appearance in the remainder of the spectrum.

Fraünhofer found that the spectra of the fixed stars exhibit dark lines resembling those in the Sun; but none of the stars whose light he examined had a spectrum exactly the same as the Sun's. Some lines of the solar spectrum he found wanting in star spectra, while other lines were not seen with the same relative distinctness as in the spectrum of the Sun. On the other hand, he found several new lines in star spectra. No two stars appeared to have the same spectrum.

An important conclusion followed, as Fraünhofer pointed out, from this observation. If the dark lines in the solar spectrum were caused by an absorptive action exercised by our own atmosphere, it would follow that the same lines ought to be seen in the spectra of the fixed stars. The contrary being the case, Fraünhofer held it to be a demonstrated fact that the dark lines are due to some property inherent in the light itself which the Sun and the fixed stars severally emit.

One more observation of Fraünhofer's, and I pass on to later researches. He found that when the flame of a candle or lamp is the source of light, the spectrum is not crossed by dark lines, but a bright double line is seen in the exact place occupied by the double dark line D of the solar spectrum. To prevent misconception, it is necessary, however, to mention that light from an incandescent substance—and the flame of a candle is such light—exhibits a continuous spectrum. The double bright line seen by Fraünhofer was due to the presence of the almost ubiquitous element, sodium, in the flame, as will be explained further on.

If we carefully weigh the results obtained by Fraünhofer, it will appear that spectroscopic analysis owes very much to his researches. It may be questioned, indeed, whether, but for his patience and perseverance, the attention of scientific men might not have been turned away, at least for many years, from a subject of inquiry which seemed when he began his labours to be rather curious than important.

Let us next inquire into the spectra given by different terrestrial sources of light.

An incandescent solid or fluid—or, to speak more correctly,* a solid or fluid *glowing* with intensity of heat—gives a continuous spectrum. But the nature of the spectrum varies with the heat of the source. If a piece of metal, for instance, be gradually heated till it is at a white heat, only the red part of the spectrum will at first be visible; then the orange part will show,

---

* The term incandescent is not properly applicable to any source of light which is not actually white. Many spectroscopists indeed go farther, and say that no luminous body ought to be described as incandescent unless its spectrum extends without dark lines or gaps of any sort from the extreme limit of visibility at the red end to the extreme limit of visibility at the violet end of the solar spectrum. Without insisting on this limitation, it certainly does seem well to point out that the term incandescent is not properly applicable to solids or fluids glowing with light belonging to the red end of the spectrum, nor to vapours glowing with light of a well-marked colour.

then the yellow, and so on, until at length the whole range of the spectrum will be seen, from the extreme red to the extreme violet.

But the question will at once suggest itself at this point,—What are the limits of the spectrum? When we were considering the solar spectrum, we might have inferred that the limits of visibility are the true limits of the space to which the solar action extends. But now that we have seen a spectrum growing with the growing heat of the source of light, we are naturally led to inquire whether there are limits to this growth. And, again, the spectrum of a gradually heated body begins at the red end so far as our vision is concerned; but is that its true beginning?

To both these questions an answer has been given. It occurred to several physicists during the latter half of the last century to inquire whether the heat which undoubtedly accompanies the light forming the spectrum corresponds in reality with the light, in so far that where the light is strongest the heat is strongest, and *vice versâ,* while where light fails totally there heat also fails. They found that no such correspondence exists. So far as the visible spectrum is concerned, the greatest heat is not received where the spectrum is brightest, but at the red end. Sir W. Herschel, however, at the beginning of the present century, found that the maximum of heat comes from *beyond* the red end of the spectrum; while, yet farther beyond the red end, heat continues to be received for a distance whose extreme limit has not yet been determined.

Heat, however, is not received beyond the violet end of the spectrum, nor even completely up to the limits of visibility in that direction.

Yet we now know that in this direction, also, the limits of visibility are not to be regarded as the limits of the solar action. Besides the heat and light which are transmitted through the prism, there is another form of action whose effects are as distinct from heat and light as heat and light are from each other. We know that the solar rays, besides illuminating and heating substances on which they fall, produce changes in the appearance and constitution of many substances. To take a familiar example: the solar rays falling on the skin not only warm it, and so affect the sense of touch; not only illuminate it, and so affect the sense of sight; but *tan* it,—an effect which is not directly cognisable by any sense that we possess,* though in-

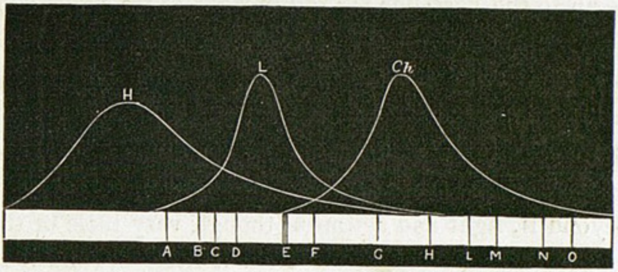
---

* It need hardly be said, perhaps, that the reference here to the senses is not introduced as bearing in any way on the subject of spectroscopic analysis, but as affording a distinction of a popular kind between the three forms of solar action. It is worthy of notice, however, that we have a sense by which the action of the longer light-waves corresponding to the red end and the parts beyond the red end of the spectrum is recognised by us, and another sense enabling us to recognise the action of the medium waves corresponding to the yellow part of the spectrum and in gradually diminishing degree the waves corresponding to parts up to the red end on one side and the violet end on the other side: but we have no sense enabling us to recognise directly the action of the shorter waves corresponding to parts of the spectrum beyond the violet end. Is it not conceivable that some creatures, even among terrestrial beings, may possess a sense enabling them to recognise the action of these short waves, and that such a sense may give them powers as distinct from the powers we possess in virtue of the senses of touch and of sight, as the sense of sight is distinct from the sense of touch? A man born blind may not be more incapable of conceiving the nature of the sense

directly sensible both to sense and touch. The real nature of this action is not altogether understood (which may be remarked also respecting heat and light), but the observed result is a modification of the chemical condition of the substances acted upon.

Now this particular mode of action—actinic action, as it has been called*—is not exerted most powerfully where the spectrum is brightest, but near the violet extremity—between the lines G and H. Nor is this mode of action limited to the visible spectrum; but precisely as heat falls beyond the red extremity of the spectrum, so actinic effect is produced beyond the violet extremity.

FIG. 24.



In fig. 24 the relation between the several kinds of action is exhibited by the curves marked H, L, and C*h* respectively, the height of these curves vertically above

---

of sight and of the powers it confers upon those who possess it, than those who have all the five senses are of the powers which may be actually possessed by creatures having organs suited to appreciate the action of the shorter light-waves.

* From a Greek word signifying a ray. Surely the term is ill-chosen.

the spectrum indicating the relative intensity of the heat, light, and chemical activity of the portion of the spectrum immediately below. Thus, beyond the red end, the heat-curve is seen to acquire its greatest height, indicating that the maximum heating effect is exerted there. The light-curve reaches its greatest height above the part of the spectrum between D and E,—in the yellow portion, that is, of the spectrum. The heat-curve reaches the base-line close by G, while the light-curve extends somewhat beyond H. At E the actinic curve rises above the base-line, reaching its greatest height above G and H, and thence passing down to the base-line far to the right of the violet end.

It appears, then, that from the extreme heat end of the spectrum to near A there is heat only; from near A to about E we have heat and light combined; from E to G, all three forms of action—light, heat and actinism—are present.* From about G to a little beyond H, light and actinism (though very little of the former) are exerted, while from a little beyond H to the extreme actinic end of the spectrum there is actinism alone.

It may be well, however, to warn the reader against the error sometimes made, of assuming that where two forms of action are present they are separable (or can be conceived to be separated) from each other. For example, it would be incorrect to say that near D a small proportion of heat rays, and a larger proportion

---

* It is worthy of notice how small the amount of all three forms of action becomes near the F line of the spectrum.

of light rays, fall on the spectrum. The correct statement is that those rays (or rather light-waves) which reach this part of the spectrum are capable of exciting heat to a certain extent, and light to a somewhat greater extent.

We see, then, that in addition to what spectroscopic analysis has already taught us, we must add this interesting fact, that the three forms of action which sun-light is capable of exerting are associated with different parts of the spectrum—heat with the red end and parts beyond; chemical action with the violet end and parts beyond that; and, finally, light more particularly with the yellow part of the spectrum, though of course, as the very term visible spectrum implies, more or less light is received from all parts of the coloured spectrum.

And the intimate real association which exists between the three forms of action is shown by nothing more distinctly than by this, that when a solid or fluid body is gradually raised to a white heat, all the forms of action are generated:—first, heat; then heat and light; then heat, and light, and actinism—as the formation of the continuous spectrum of such bodies serves abundantly to prove.

Only one solid substance, the earth erbia, gives a non-continuous spectrum when heated. So that it has come to be regarded, as a characteristic peculiarity of incandescent solids and fluids, that they present a rainbow-tinted streak of light crossed by no dark lines or gaps.

With glowing vapours the case is altogether different. Although there are exceptions to the rule, it may be stated as a general characteristic of the spectra of such vapours that they consist of coloured lines or bands. Sir David Brewster, Sir John Herschel, and Talbot, were among the first who examined such spectra. In 1822 Sir John Herschel called attention to the importance of the study of the lines and bands seen in the spectra of the vapours of different elements. 'The pure earths,' he said, 'when violently heated, yield from their surfaces lights of extraordinary splendour, which when examined by prismatic analysis are found to possess the peculiar definite rays in excess which characterise the tints of the flames coloured by them; so that there can be no doubt that these tints arise from the molecules of the colouring matter reduced to vapour, and held in a state of violent ignition.'

It would be interesting to trace the history of those laborious researches by which men of science—Herschel, Brewster, Tyndall, the Millers, Huggins, Gladstone, Frankland, Plücker, Hittorf, and many others—have determined the spectra of gases and vapours by various methods and under various conditions. But as in this treatise I feel bound to deal only with those parts of the history of spectroscopic analysis which are associated with the study of the Sun, or which serve to elucidate solar physics, I must here content myself with indicating results without describing the processes by which they were obtained, or assigning to each of the eminent men above-named, and to their fellowworkers their exact share in the noble series of labours referred to.

At an early stage of the inquiry, though at the time the phenomenon was not correctly interpreted, a means was found of obtaining the spectra of the vapours of elements which cannot be vaporised by ordinary methods. It was noticed that the electric spark has a spectrum consisting of bright lines. But, unlike all the sources of light whose spectra have hitherto been considered, the electric spark yields a variable spectrum. When the electric discharge takes place between conductors of the same nature, and through any given and unchanged medium, the same spectrum is always seen; but when either condition is departed from, a different spectrum is obtained.

It was presently recognised that the spectrum obtained from the electric spark is twofold in its nature. It includes the spectrum of the gas or vapour through which the discharge takes place, and also the spectrum of the vaporised substance of the conductors.

Here, then, was a ready means of determining the spectra of the metallic and other elements* not easily volatilised in other ways, as also of such gases as nitrogen and oxygen; while conditions of pressure, combination, and the like, could be introduced, which would be wholly impracticable if the method of vaporising elements in the flame of an ordinary lamp had alone been available.

---

* It seems unnecessary to speak of the spectra *of the vapours* of such, and such elements, since in reality it is only as vapours that iron, sodium, and the rest have characteristic spectra at all.

In this way a large number of elements had their spectra assigned to them, while in several instances physicists began to recognise peculiarities in the spectrum of the same element when examined under different conditions.

But an important series of researches yet remains to be considered. Hitherto we have dealt with the actual spectra of luminous objects; we have now to inquire into the effects produced on these spectra, when the light which forms them is allowed to pass through absorbing media. We owe chiefly to Sir David Brewster the initiation of this branch of research,—though in this as in so many other departments of spectroscopic inquiry, we find a host of distinguished physicists joining in the work. Brewster found that when ordinary solar light is transmitted through the thick vapour of nitrous gas, a number of new dark lines are seen, parallel to the Fraüнhofer lines, and congregated in a remarkable degree towards the violet end of the spectrum. He further proved that these lines were seen whatever the source of light might be. Professors Miller and Daniel made further researches into the effects of vapours in causing dark lines to appear in the spectrum of solar or white light. Some of Professor Miller's results are worth quoting, because they show how closely a physicist may approach a great discovery without actually effecting it. 'First,' he says, 'colourless gases in no case give additional lines, or lines differing from those of Fraünhofer. Secondly. The mere presence of colour is not a security that new lines will be produced; for instance, of two vapours undistinguishable by the eye, one, *bromine*, gives a great number of new lines, while the other, *chlorine of tungsten*, exhibits none. Thirdly. The position of the new lines has no connection with the colour of the gas; with *green perchlorine of manganese*, the new lines abound in the green of the spectrum; with *red nitrous acid* they increase in number and density as we approach the spectrum's blue extremity.' Some of these results, rightly understood, contain the germ of the great discovery afterwards effected by Kirchhoff; since in some of the cases actually experimented on by Professor Miller, the absence of *new* lines meant simply that the absorption lines, corresponding to the element he was dealing with, were coincident with some of the Fraünhofer lines. Others, however, approached the solution of the great problem even more nearly, since they actually touched on the principle of the reversal of the spectral lines, which affords the explanation of the coincidences detected but unnoticed by Professor Miller. 'None of these distinguished men,' says Professor Tyndall, 'betrayed the least knowledge of the connection between the bright bands of the metals and the dark lines of the solar spectrum. The man who came nearest to the philosophy of the subject was Ängstrom. In a paper translated from Poggendorff's 'Annalen' by myself, and published in the 'Philosophical Magazine' for 1855, he indicates that the rays which a body receives are precisely those which it can emit when rendered luminous. In another place he speaks of one of his spectra giving the general impression of reversal of the solar spectrum. Foucault, Stokes, Thomson, and Stewart, have all been very close to the discovery; and for my own part the examination of the radiation and absorption of heat by gases and vapours would have led me in 1859 to the law on which all Kirchhoff's speculations are founded, had not an accident withdrawn me from the investigation.'

At the very moment, however, when the great secret was about to be revealed, an eminent physicist wrote thus: 'In quitting the mere phenomena of luminous spectra, and rising to the inquiry as to their causes, we enter a more arduous course. The phenomena defying, as we have seen, all attempts hitherto to reduce them within empirical laws, no complete explanation or theory of them is possible. All that theory can be expected to do is this—it may explain how dark lines of any sort may arise within the spectrum.'

But theory succeeded in doing very much more than was thus anticipated from her.

It had been noticed by Fraünhofer that the two orange-coloured lines close together which form the spectrum of the glowing vapour of sodium, coincide exactly in position with two dark lines in the orange-coloured part of the solar spectrum—the double D line in fig. 23. Kirchhoff, having a spectrosocope of great dispersive power,* determined to inquire whether this coincidence

---

* Its prismatic battery contained four flint-glass prisms, not fixed or clamped in any way, but standing freely on four little pedestals. The

was exact. 'In order to test in the most direct manner possible,' he says, 'the frequently asserted fact of the coincidence of the sodium lines with the lines D, I obtained a tolerably bright solar spectrum, and brought a flame coloured by sodium vapour in front of the slit. I then saw the dark lines D change into bright ones. The flame of a Bunsen's lamp threw the bright sodium lines upon the solar spectrum with unexpected brilliancy. In order to find out the extent to which the intensity of the solar spectrum could be increased without impairing the distinctness of the sodium lines, I allowed the full sunlight to shine through the sodium flame, and, to my astonishment, I saw that the dark lines D appeared with an extraordinary degree of clearness.'

Let the full force of this result be recognised before we proceed to consider the method by which Kirchhoff certified himself as to its exactness. He had shining in through the slit two kinds of light—sunlight and the light of the sodium flame. The sunlight alone would have given the ordinary solar spectrum with the D lines of a certain degree of darkness. The sodium flame alone would have given two bright lines just where the dark D lines of the solar spectrum appear. What he expected was, naturally, that the bright lines of the sodium spectrum would at least partially reduce the

---

whole arrangement seems singularly ineffective in comparison with the spectroscopes now in use; for example, if we compare Kirchhoff's battery with the battery of prisms in the fine spectroscope made by Browning for Mr. Gassiot, the former seems almost ridiculous in its imperfectness. 'A cough or a sneeze,' it has been well remarked, 'would set that whole battery in disarray, with which, nevertheless, Kirchhoff solved the secret of the solar spectrum.'

darkness of the coresponding D lines, even if they did not altogether cause these dark lines to disappear, or to be replaced by bright lines. But these lines actually appeared darker. It was precisely as though an experimenter were to cast a beam of light exactly upon a shadow, and to see the shadow actually intensified instead of the reverse.

Kirchhoff proceeded to test this astonishing result. 'I exchanged the sunlight for the Drummond's or oxy-hydrogen lime-light, which, like that of all incandescent solid or liquid bodies, gives a spectrum containing no dark lines. When this light was allowed to fall through a suitable flame coloured by common salt, dark lines were seen in the spectrum in the position of the sodium lines. The same phenomenon was observed if, instead of the incandescent lime, a platinum wire was used, which, being heated in a flame, was brought to a temperature near its melting point by passing an electric current through it.'

These experiments were, if possible, even more striking than the former; for now Kirchhoff had two lights which seemed to produce darkness. The electric light alone covered with its continuous spectrum the place where the sodium lines appear in the solar spectrum; the sodium light alone lit up this very part of the spectrum. When the two lights were both shining one would have expected the result to be an increased brightness in this part; instead of which there actually resulted darkness.

The observed fact is in itself important: its interpretation involves one of the most important facts ever discovered by man. It is well to distinguish between the two.

The observed fact is that the sodium flame, which emits rays of a certain order of refrangibility—that is, rays which, if passed through a prism, will follow a certain path—has the power of absorbing rays of precisely the same order. The interpretation of the fact is founded on the existence of a law which is thus worded by Professor Roscoe—'Every substance which emits *at a given temperature* certain kinds of light, must possess the power *at that same temperature* of absorbing the same kinds of light.'*

---

* This is merely a corollary from a more general law, according to which the same relation holds between the powers which substances possess of emitting and absorbing heat-waves as well as light-waves and actinic-waves. The law called the theory of exchanges was first enunciated for heat by Prevost of Geneva, and has since been established for heat and light by the researches of Prevostaye, Dessains, Stewart, and Kirchhoff. So far as the application of the theory to light is concerned, it must be admitted that there are still many difficulties in the way of its complete acceptance. These difficulties somewhat importantly affect our conclusions where we are considering the application of the theory in mode and measure to spectroscopic researches, though, so far as the general results here chiefly considered are concerned, they need not greatly trouble us.

I may refer my reader to Dr. Stewart's *Elementary Treatise on Heat* for a very interesting examination of the subject and the demonstration of the fundamental principles involved in the law. But I must caution the reader against one point in the course of Dr. Stewart's reasoning which is very likely to mislead, and involves an error in an optical subject of considerable importance. It is necessary for the demonstration that the course of rays (light-rays or heat-rays) not falling quite perpendicularly, or rather not strictly parallel to each other, should be considered, and Dr. Stewart removes the difficulty for small beams of ight or heat, by considering the size of the source of light. He says, 'Just as a line is in reality always part of the boundary of a solid, so

Kirchhoff experimented on other elements. He found that a flame coloured by potassium causes dark lines to appear on the continuous spectrum of the lime-light, and that these lines appear precisely where bright lines are seen when the spectrum of that coloured flame is viewed alone. The same was proved by Kirchhoff and Bunsen for the lines of lithium, calcium, strontium, and barium; while it has been shown to hold for other elements by Dr. Miller and others.

But now that the general law was established, important results respecting solar physics were established along with it. Since Kirchhoff had proved that when the electric light shines through a sodium flame, the sodium

a ray is always in reality part of the boundary of a beam or pencil of light. We may satisfy ourselves that this is the case in nature by considering the light which reaches the eye from a star or other object apparently very small; this would seem to be the nearest approach to a geometrical line of light, whereas since a star has a certain real, though very minute, angular diameter, the light from it is in reality a converging pencil, although no doubt the angle of convergence is very small.' On account of the importance of the optical considerations in question, I may be permitted to correct what is undoubtedly an erroneous statement. The difficulty must be got over by considering the size of the object on which light or heat rays fall, not by considering the size of the source of light. There is no such thing in nature as a converging pencil of light proceeding directly from a luminous object. All real pencils, whether of light or of heat, are originally diverging; they diverge from every point of the self-luminous or heat-giving body, and the angle of their divergence is real—however minute it may be—so long as the object which receives light or heat has real dimensions, however minute. This consideration does not affect the conclusions deduced by Dr. Stewart, since undoubtedly the diverging pencils of light or heat, from a source of considerable dimensions, form a converging beam, when we consider them with reference to a smaller object on which they fall.

lines appear as dark lines across the continuous spectrum of the electric light, what was more obvious than the conclusion that sunlight also, which shows these same dark lines, must, before reaching us, have passed through the vapour of sodium. Either then, in our own atmosphere, or in the atmosphere of the Sun, this familiar metal sodium, the basis of such commonplace substances as salt and soda, must exist in quantities sufficient to cause the observed absorption-lines.

But more than this, those countless other lines which cross the solar spectrum must each indicate some process of absorption exerted by vapours in our atmosphere or his. Since the presence of sodium has thus been demonstrated, why may not the presence of other elements be in like manner rendered apparent?

Kirchhoff did not long delay the inquiry thus suggested. He compared the spectrum of iron (or, to use the accepted mode of expression, the spectrum of the luminous vapour of iron) with the solar spectrum. Now, the spectrum of iron as known to Kirchhoff contained some sixty bright lines.* There was a great difference, then, between this spectrum and that of sodium. Yet, to his astonishment, Kirchhoff found that precisely as the two sodium lines agree with the D lines of the sodium spectrum, so every one of the iron lines had its counterpart in the solar spectrum.† Line for line, and not only so, but strong

---

* Many more have since been discovered; in fact, there are now more than 450 recognised iron lines.

† It is hardly necessary to note, perhaps, that Kirchhoff could not

line for strong line, and faint line for faint line, every line of the iron spectrum appears as a dark line in the spectrum of the Sun!

Kirchhoff in all probability never questioned for a moment the conclusiveness of this relation. He must have felt its significance intuitively. It was probably, therefore, only to satisfy others that he presented a strong argument from the theory of probabilities in favour of the view that the observed relation implied a real association between the two sets of lines. Taking as ½ the chance that a bright line in the iron spectrum would *seem* to have a counterpart in the richly 'lined' solar spectrum if accident were alone in question, he calculates that the chance of the observed relation (leaving altogether out of question the relative strength of the lines) is less than

$$\frac{1}{1{,}000{,}000{,}000{,}000{,}000{,}000}.*$$

'Hence,' remarks Kirchhoff, 'this coincidence must

---

establish this result in the same way as the former, because he could not cause sunlight to shine through the glowing vapour of iron. He employed a method quite as satisfactory however, causing the light from iron vaporised by the electric spark to form a spectrum side by side with the solar spectrum, the solar light being admitted directly to the battery of prisms; that of the iron being admitted after reflection through a small prism near the slit.

* To this I may add that the chance of the observed relation, now that 450 lines of iron are recognised, is less than a fraction whose numerator is unity, and whose denominator consists of no less than 136 figures (the first four being 2907). This chance is not very unequal to that which I have shown to correspond to the probability that one of the less marked peculiarities of stellar arrangement detected by me while constructing my large star-atlas, is due to mere chance-distribution.

be produced by some cause, and a cause can be assigned which affords a perfect explanation of the phenomenon. The observed phenomenon may be explained by the supposition that the rays of light which form the solar spectrum have passed through the vapour of iron, and have thus suffered the absorption which the vapour of iron must exert. As this is the only assignable cause of the coincidence, the supposition appears to be a necessary one. These iron vapours might be contained either in the atmosphere of the Sun or in that of the Earth. But it is not easy to understand how our atmosphere can contain such a quantity of iron vapour as would produce the very distinct absorption-lines which we see in the solar spectrum, and this supposition is rendered still less probable by the fact that these lines do not appreciably alter when the Sun approaches the horizon. It does not, on the other hand, seem at all unlikely, owing to the high temperature which we must suppose the Sun's atmosphere to possess, that such vapours should be present in it. Hence the observations of the solar spectrum appear to me to prove the presence of iron vapour in the solar atmosphere with as great a degree of certainty as we can attain in any question of natural science.'

Thus cautiously did Kirchhoff proceed in establishing the great principle on which spectroscopic researches into solar physics were to depend. Nothing can perhaps be required to strengthen the confidence of the reader in the justice of this principle. Yet it may be added that the effect of our own atmosphere on the solar spectrum is now thoroughly recognised and found to correspond with the density of the layers through which the Sun's rays penetrate at different hours of the day. The reader may further be reminded of what has been already said respecting the evidence afforded by the character of stellar spectra. All those spectra would undoubtedly be similar to each other and to the solar spectrum if our atmosphere contained the source of all the spectral dark lines. It is to be noted also that while the coincidences in the case of iron serve to confirm any evidence we may derive from other coincidences, they are also *confirmed by* such evidence; because as one after another the terrestrial elements are found to have spectral lines corresponding with the solar dark lines, the general fact becomes more and more firmly established that the Sun is constituted of those elements with which we are familiar.

Kirchhoff found that calcium, magnesium, and chromium exist in the solar atmosphere. The presence of nickel, also, and cobalt was indicated by the agreement of the most conspicuous of their lines with solar dark lines. All the lines of these metals, however, could not be recognised, nor were the coincidences in the case of cobalt sufficient to satisfy Kirchhoff. 'I consider myself entitled,' he says, 'to conclude that nickel is visible in the solar atmosphere; but I do not yet express an opinion as to the presence of cobalt. Barium, copper, and zinc,' he proceeds, 'appear to be present in the solar atmosphere, but only in small quantities; the brightest of the lines of these metals correspond to distinct lines in the solar spectrum, but the weaker lines are not noticeable. The remaining metals which I have examined, viz. gold, silver, mercury, aluminium, cadmium, tin, lead, antimony, arsenic, strontium, and lithium, are, according, to my observations, not visible in the solar atmosphere.'

I have thus far quoted Kirchhoff, not because the account of his investigations exhibits the actual state of our knowledge at the present time, but because they are so associated with the discovery of the great principle on which spectroscopic analysis depends as to have an interest wholly distinct from that—great as it is—which they derive from their intrinsic importance. It would occupy much more space than is here at my disposal to exhibit the progress of research by which our knowledge of the solar spectrum has reached its present position. For the full history of the subject I would refer my readers to Dr. Schellen's admirable 'Spectralanalyse,'* and to the excellent treatise on spectroscopic analysis by Professor Roscoe. So far as my purpose in this place is concerned it is only necessary for me to sum up the results of the search for coincidences between solar dark lines and the bright lines of terrestrial elements—in other words, to exhibit the elements which exist, so far as is yet known, in the

---

* A translation of this work by the daughters of Mr. Lassell, F.R.A.S., and edited by Dr. Huggins, the great English master of the subject, will probably be published (by Messrs. Longmans) before this work appears. It will contain many important additions as compared with the first German edition, and cannot fail to be a work of extreme value.

substance of the great central luminary of our system. The following table, drawn up by M. Ängström, exhibits the number of lines belonging to the several elements enumerated, which have been found to correspond with dark lines of the solar spectrum:—

| | | | |
|---|---|---|---|
| Hydrogen | 4 | Manganese | 57 |
| Sodium | 9 | Chromium | 18 |
| Barium | 11 | Cobalt | 19 |
| Calcium | 75 | Nickel | 33 |
| Magnesium | 4 + (3 ?) | Zinc | 2 |
| Aluminium | 2 ? | Copper | 7 |
| Iron | 450 | Titanium | 200 |

It will be noticed that the solar spectrum shows no traces of the existence of the nobler metals, gold and silver, nor of the heavy metals, platinum, lead, and mercury. On the other hand, it is significant that the lines of nitrogen and oxygen are absent, though these gases can scarcely be supposed to be actually wanting. We must remember, in forming an opinion as to the *absence* of these elements (as also of such elements as carbon, boron, silicon, and sulphur), that while the presence of certain lines in the solar system may prove abundantly that the terrestrial element which has corresponding bright lines, exists in the Sun's substance, it by no means follows with equal certainty that because all the lines of an element are wanting in the solar spectrum, therefore the Sun does not contain those elements. This will appear when we consider the various circumstances which may cause an element really existing in the Sun's substance to afford no trace of its presence. In the first place, the vapour of that element may be of a density causing it to lie always at a very low level, and therefore perhaps altogether beneath that level whence proceeds the white light of the Sun—that is, the light which gives the continuous spectrum across which the dark lines lie. Or, again, the element may exist in the Sun's substance at such a temperature, or at such a pressure, as to produce—not well defined absorption lines, but —only broad faint bands, which no optical means we possess can render sensible as such. Or, again, the element may be in a condition enabling it to radiate as much light as it absorbs, or else very little more or very little less—so that it either wholly obliterates all signs of its existence, whether in the form of dark lines or bright lines, or else gives lines so little brighter or darker than the surrounding parts of the spectrum that we can detect no trace of their existence. In these, and in yet other ways, elements may really exist (or rather undoubtedly do exist in the Sun) of whose presence we can obtain no trace whatever.

In other chapters of this work the evidence afforded by the spectroscope respecting the condition of various parts or appendages of the Sun, as the spots, faculæ, pores, prominences, corona, and so on, will be considered at length, as also the various theories to which such evidence has given rise. I conclude this chapter by enunciating the general rules of spectral analysis, and by explaining the rationale of certain recent applications of the spectroscope which have deservedly attracted great interest, but are not perhaps very generally understood.

The general principles of spectroscopic analysis are as follows:—

1. An incandescent solid or liquid gives a continuous spectrum.

2. A glowing vapour gives a spectrum of bright lines, each vapour having its own set of bright lines, so that from the appearance of a bright line spectrum we can infer the nature of the vapour or vapours whose light forms the spectrum.

3. An incandescent solid or liquid shining through absorbent vapours gives a rainbow-tinted spectrum crossed by dark lines, these dark lines having the same position as the bright lines belonging to the spectra of the vapours.

4. Light reflected from any opaque body gives the same spectrum as it would have given before reflection.

5. But if the opaque body be surrounded by vapours, the dark lines corresponding to these vapours appear in the spectrum with a distinctness proportioned to the extent to which the light has penetrated these vapours before being reflected.

6. If the reflecting body is itself luminous, the spectrum belonging to it is superadded to the spectrum belonging to the reflected light.

7. Glowing vapours surrounding an incandescent body will cause bright lines or dark lines to appear in the spectrum according as they are at a higher or lower

temperature than the body; if they are at the same temperature, they will emit just so much light as to compensate for that which they absorb, in which case there will remain no trace of their presence.

8. The electric spark presents a bright-line spectrum, compounded of the spectra belonging to those vapours between which and of those through which the discharge takes place. According to the nature of these vapours, and of the discharge itself, the relative intensity of the component parts of the spectrum will vary.

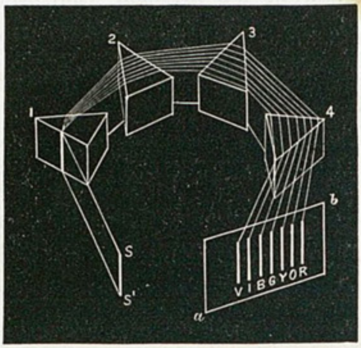
It will be seen, as we proceed, that all these principles bear more or less directly on the application of spectroscopic analysis to the interpretation of solar phenomena.

In all branches of spectroscopic research certain difficulties have to be specially dealt with, while certain circumstances avail to help the observer. We are now to consider,—first, the means which are available to the astronomer for the special purpose of advancing our knowledge of solar physics; and, secondly, the peculiar difficulties which he has to overcome.

In the experiment illustrated in fig. 21 we see the action of a single prism on the solar light. But the time has long since passed when the action of a single prism could teach us anything new respecting the Sun. The observer has to work nowadays with a battery of prisms. A portion of light which has been already dispersed by one prism is received on another, and is thus yet more dispersed; a portion of this doubly dispersed light falls again on another prism and emerges yet more dispersed; a portion of this trebly dispersed light falls on a fourth prism, and so on—the number of prisms actually employed depending on the special branch of inquiry which is being pursued, for some require more dispersive power than is necessary for others.

Space compels me to limit my description specially to the action of the prismatic battery, and I therefore neither describe nor illustrate the means adopted for causing a suitably-shaped beam of solar light to fall on the first face of the first prism. I therefore simply state the fact that, in the study of the solar spectrum, a beam whose cross section is as S S′ (fig. 25), the shape of the slit through which light is admitted, falls, as shown in fig. 25, upon the first prism of the battery, or prism 1 in the figure. If a screen were placed to intercept this beam anywhere between S S′ and prism 1, there would be seen upon the screen a bright bar of light shaped like S S′. Prism 1 disperses this beam in the manner already described, and the beam falls thus dispersed on

Fig. 25.



prism 2. If a screen were placed to intercept the beam anywhere between prism 1 and prism 2, a short solar spectrum would be seen on it, the violet end lying towards the bases of the prisms; and, assuming the battery of prisms to lie on a horizontal plane, the length of the spectrum—that is, its extension measured from red to violet—would be horizontal. Then this beam passes to 3; and if a screen were placed between 2 and 3 a somewhat longer spectrum would be seen. Between 3 and 4 the spectrum would be still longer. And, lastly, a screen placed beyond the last prism, as *a b*, would show the solar spectrum much longer and proportionately fainter than in Newton's experiment. And although no such screen is used by spectroscopists, who receive the emergent rays into a telescope with which they examine the spectrum, it will be convenient for our purpose to refer at present to a spectrum supposed to be received on a screen, as in fig. 25.

Now, on *a b* I have shown a violet image of the slit V, an indigo one I, and so on, to a red image at R. But there are an infinite number of images ranged from the extreme violet end to the extreme red end. In places, however, no images are formed. The spaces thus left without light are the dark lines.

It is not difficult, then, to see on what conditions the visibility of the dark lines will necessarily depend. If we could have a slit which was a true mathematical line, every dark space would be present in the screen, even though the dispersive power were small. But, as a matter of fact, the slit has a definite breadth, however narrow we may make it. Now, suppose that A B C (fig. 26) represents a small part of the solar spectrum as shown on the screen in fig. 25, but that the *true* nature of this part of the solar spectrum* is shown in the narrow band *a b c*, so that in reality sunlight has no rays whose refrangibility corresponds to the band at *b*. Suppose, further, that the aperture of the slit is equal in width to this band *b*. Then the light corresponding to the extreme limits of the light in *a b c*, will form the two images of the slit shown at B; these images will meet, and no absolutely black line will be seen. There will, however, be a dusky band, darkest down the middle, and twice as broad as the true band *b*. But now consider the effects of an increase of dispersive power. Say we double the length of the spectrum, or rather of the particular part shown in figs. 26 and 27. Then the parts *a b c* of the real solar spectrum will all be doubled in length, and in the observed solar spectrum the light corresponding to the extreme limits of the band *b* will produce the two images of the slit

FIG. 26.

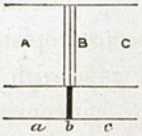


FIG. 27.

---

* The true nature of the solar spectrum may be regarded as that corresponding to the imaginary case of a slit, which should be a true mathematical line, and of a battery of prisms which should cause no optical faults whatever, insomuch that the image of the slit for rays of any order of refrangibility would also be a mathematical line.

shown at B and B′, but these will be no wider than before, and will be separated by a really black band, half as wide as *b*. This band will be bordered by a penumbral fringe whose boundaries are indicated by the dotted lines, the whole breadth from dotted line to dotted line being half as great again as that of the band *b*.

The reader will, therefore, at once see the importance of increasing the dispersive power of our battery of prisms; since in this way very fine lines which might otherwise escape detection can be rendered visible. Also, it is obvious that two lines very close together would be shown as one with a certain amount of dispersive power, while with more dispersive power they would be clearly separated. Yet once more, a line in the solar spectrum which *seemed* to coincide, without being really coincident, with the bright line of some metallic spectrum (brought into comparison with the Sun's in the manner referred to above) might, by increase of dispersive power, be removed appreciably from its supposed counterpart.

So far, then, as the direct examination of the solar spectrum is concerned, we must aim specially at the means of increasing dispersion. It will be seen, also, further on, that in two highly important applications of the analysis—viz. to the examination of the prominences, and to the recognition of motions of approach or recess due to solar cyclones—great dispersion is the chief point to be secured by the spectroscopist.

25 But fig. ~~24~~ shows that certain difficulties have to be encountered in securing great dispersion. Only the

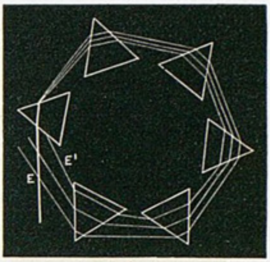
part of the spectrum near G has been formed (in the case illustrated in that figure) by rays which have gone symmetrically through the battery of prisms. The rays forming the part near V have gone through the middle of prism 1, and come out near the base of prism 4; while those forming the part R passing also through the middle of prism 1, come out near the vertex of prism 4. Now, optical considerations render it essential, or at least extremely important as far as the clear definition of the lines is concerned, that each part of the spectrum should be formed by rays which have gone symmetrically round such a battery as is shown in fig. ~~24~~.* Furthermore, 25 when the dispersion is very great only a portion of the spectrum will be formed at all (some at either extremity falling outside the last prism—beyond apex and base) when the prisms occupy a fixed position. Then, again, when the light has been bent round a nearly

---

* The point to be secured is that the rays forming any part of the spectrum under examination should pass into and out of each prism at equal angles (as the light forming the part G of the spectrum in fig. 25 does). It may be shown that in this case those special rays pass with least possible deviation through each prism, so that the condition is generally called that of *minimum deviation*. But minimum deviation *per se* has no advantages, and, as a matter of fact, the real condition secured by this arrangement is that the primary and secondary foci of emergent pencils are as nearly as possible coincident; so that, though the image of the slit formed by those special rays is not formed by absolute points of light, it is formed by circles (technically called 'circles of least confusion') having the smallest possible diameter. In a paper in the *Monthly Notices of the Astronomical Society*, vol. xxx., I have shown that the circle of least confusion has in this case a definite, though exceedingly minute diameter, even in the case of a single prism. The mathematical expression for the radius of this circle is given in that paper, but is somewhat too complex to be repeated with advantage in these pages.

complete circle of prisms, as in fig. 28, the emergent light E E′ will be intercepted by the first prism of the battery; and this circumstance limits the dispersion which can be given in this manner.

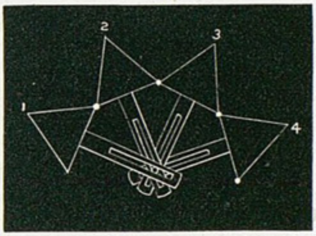
FIG. 28.



Let us consider how these difficulties have been or may be encountered.

We owe to Mr. Browning the invention of a most ingenious plan by which, whatever part of the solar spectrum is studied, the battery of prisms will be

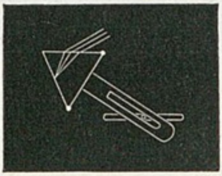
FIG. 29.



properly adjusted. In his automatic spectroscope he attaches to each prism a slotted bar, as shown in fig. 29. All the slots pass over a central pivot, and the prisms are attached at their angles as shown. Hence they must necessarily be at all times symmetrically placed; so that if 1 be fixed and motion be communicated to 4, then 2 and 3 will move, each in its proper degree, and all four will preserve their proper relative positions. In Browning's spectroscope there are six such prisms, and the light emerging from the sixth passes just clear of 1.

I have suggested a modification, by which only a corner of the first prism is fixed, as at fig. 30, and this prism is automatically adjusted like the rest,—a fixed slot *s s′* square to the path of the incident light guiding the motion of this prism according to the required law. In like manner I have suggested that the viewing telescope should be guided on the same principle of automatic motion.*

Fig. 30.



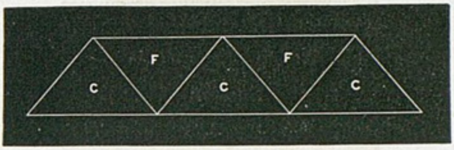
---

* This arrangement must be regarded as only one out of several modifications of which Mr. Browning's ingenious plan admits. It is doubtless this modification which alone satisfies the conditions aimed at; and this circumstance renders it all the more evident that it should be regarded as implicitly contained in Mr. Browning's plan. Nothing is easier than to devise modifications, and even (as in this instance) improvements on the plans of others. But no separate merit can fairly be claimed under such circumstances. In this special instance, for example, I should not have turned my thoughts towards the problem of securing the conditions of minimum deviation for all rays, but for the interest I took in Mr. Browning's plan. So soon as he had read the paper describing his plan, before the Royal Astronomical

So far as clearness of definition and the satisfactory study of the whole length of the spectrum are concerned, this arrangement leaves nothing to be desired. But for a yet greater increase of the spectrum's length more is needed. We have reached the limits of dispersion which one circular battery can give; but it would be desirable to obtain a yet greater dispersion.

One way in which this can be done is by the use of what are called direct-vision prisms. In these, two flint-glass prisms (F F) are combined, as shown in fig. 31, with three crown-glass prisms. The prisms C cause deviation and dispersion in one direction; the prisms F cause equal deviation and more dispersion in the contrary direction. Hence results a balance of dispersion without deviation; and if we add such a prism to any battery of prisms, we get all the advantage of the dispersion without increasing the deviation which had been our difficulty.

FIG. 31.

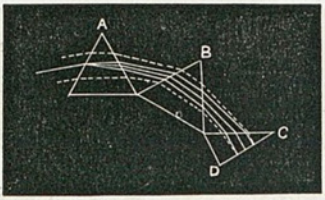


Society, I felt that his method involved the complete solution of the difficulty. He is now constructing an automatic spectroscope, in which the modifications I have suggested have been introduced; and as complete success depends only on the exactness with which the mechanical and optical relations are fulfilled, there can be no doubt of the result, for rigid accuracy is a characteristic feature of all Mr. Browning's work. Let it be understood clearly that if I have any merit in the matter at all, it consists in pointing out two unnoticed good qualities of Mr. Browning's own plan. Modified or not, the plan is altogether his.

Yet there is one important disadvantage in direct-vision prisms, more especially when they are employed in researches requiring very neat and exact definition: it is, of course, wholly impossible to employ any method for securing minimum deviation.

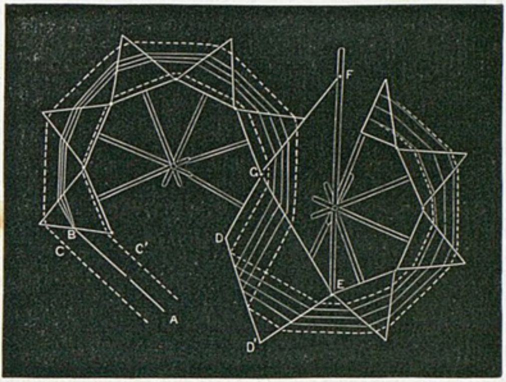
A plan by which the dispersion in a battery of prisms may be doubled, falls next to be considered. Supposing A and B (fig. 32) to be two of the ordinary triangular prisms and C a right-angled prism half the size of the others, then the rays which fall on C D are reflected back again through the battery. I need not here describe the contrivance by which the emergent pencil can be viewed in such a case by a telescope in which there is a totally reflecting prism sending the light out at right angles to the axis of the tube. According to another arrangement, in place of such a prism as C, one is employed which by two total reflections raises the rays to a higher level, so that (the prisms of the battery being twice as high as usual) the light returns by a separate course.

Fig. 32.



I have devised a plan by which a much greater dispersion than has ever yet been gained may be combined with a perfectly true automatic adjustment for all parts of the spectrum. In this plan Mr. Browning's automatic method is extended to a second battery, while the plan for returning the rays, illustrated in fig. 33, is employed in such sort as to double the dispersive power of the double battery. Fig. 33 shows the arrangement of the two batteries. A B is the light incident on the first battery, and the course of the light can be traced by the triple set of lines through the double set of prisms, the dotted return lines showing the course of the return rays to emergence at C C′. It will be seen that the large intermediate prism D E may be regarded as belonging to both batteries. There is no loss of light in passing from one battery to the other, since the reflection at D D′ is total. The close double lines show the direction of the slotted bars, E F being a long slotted bar kept square to the rays leaving the face D′ E by means of the equality of G F and G E.

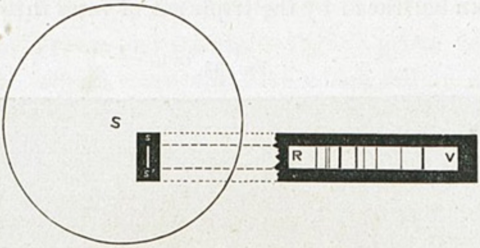
FIG. 33.



(It pivots round E and slots at F.)* The figure shows the arrangement of a battery having an effective dispersing power equal to that of nineteen equilateral prisms of heavy flint-glass.†

It must be remembered that what the spectroscope really does is to give a range of pictures of whatever luminous object or part of an object would be visible through the slit if the spectroscope were removed.

FIG. 34.



---

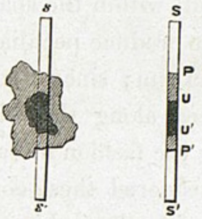
[*] It is not improbable that before these lines are read an instrument on this plan will have been constructed by Mr. Browning, to whom I have submitted the proposal. I cannot but think that if the mechanical and optical difficulties which the plan involves can be overcome (and if he cannot overcome them I know not who can), such an instrument will prove of considerable service in researches into solar physics. It will be seen that the plan is only available where very strong light is to be analysed. But in dealing with the Sun this consideration introduces no new difficulty.

[†] The double battery is represented in about the position corresponding to minimum deviation for the line G in the indigo; and as the half prism can be made to pass over the slot at F, the automatic motion can be carried on till the visible extremity of the spectrum towards the violet is reached. Of course, in examining the red rays, the batteries open out, and there is no limit (except the length of the slots) to motion in that direction, so that the red extremity of the spectrum can be overpassed if need be. Motion is communicated to the intermediate prism D D′, by which means the range of the automatic action is divided, and greater truth of working secured.

Now, supposing such a portion of the solar photosphere is observed as is shown in the space *s s′*, fig. 34 (S being the solar disc), the images of this portion will give us the spectrum R V, showing the dark lines due to the absence of certain images as explained above; and *no other portion of the disc produces any effect whatever.* It is very essential to remember this. We are in fact analysing under such circumstances the part *s s′* of the disc, and no other part.

If a spot or a facula be crossed by *s s′*, then the spectrum we get is no longer that of a uniformly, or almost uniformly, bright part of the solar disc. If *s s′* (fig. 35) represent an enlarged view of the spot and the space included by the slit, then this last, seen separately, will be as S S′; and the spectrum will consist of a number of images of S S′ ranged side by side, so as to form a strip, as R V in fig. 34. Hence at the top and bottom of this compound spectrum there will be two narrow solar spectra corresponding to the parts S P and S′ P′; next to these will be two narrow spectra of the penumbral parts P U and P′ U′; and about the middle a narrow spectrum corresponding to the umbral

FIG. 35.



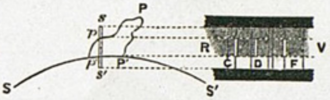
part U U′, all these spectra forming one compound spectrum, whose red end is towards the left (assuming the dispersion to be as in the case illustrated in fig. 34) and its violet end towards the right. The nature of the penumbral and umbral spectra will be stated further on. It is by comparing these spectra with the adjacent solar spectra that the spectroscopist is enabled to form an opinion as to the nature of the spots, and to make inferences as to the general physical constitution of the Sun.

Similar remarks apply to the case where a portion of facula, or pores, or mottlings, or any other features of the solar disc, fall within the space *s s′*. All such peculiarities tend to produce peculiarities in the resulting compound spectrum; since the image of the portion *s s′* is repeated along the whole length of the spectrum R V after the fashion already described.

But, having considered these comparatively simple cases, let us deal with the subject which has of late attracted so much attention—the visibility of the spectrum of the prominences when the Sun is not eclipsed. Further on the exact nature of the prominence spectrum will be considered; but in this place I note only respecting it that it consists of bright lines.

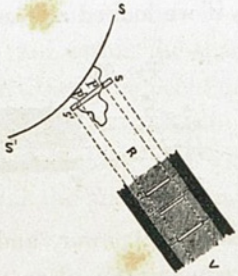
Now, suppose that P P′ is a prominence, S S′ the edge of the Sun, and *s s′* the space included by the slit. Then *p′ s′*, as in the former cases, produces a solar spectrum (which, however, commonly presents certain peculiarities when belonging to the edge of the Sun's disc); the part *p p′* includes a portion of the prominence, and gives a prominence-spectrum which we may suppose to be represented by the bright lines at C and F and near D. But it will also give a solar spectrum, for the light of our own illuminated air comes from the space included within the slit $s\,s'$; and as our air is illuminated by solar light, it produces (according to rule 4, page 128) a solar spectrum. Also the part $s\,p$ will give a solar spectrum due to the illuminated air. Now, the prominence P P′ is absolutely obliterated from view by the illuminated air, which extends all round (and over, be it remembered) the place of the Sun. Since, then, if we looked at the space $s\,s'$ alone, with whatever telescopic power, and with whatever contrivances for reducing the glare of light, the portion $p\,p'$ of the prominence P P′ would be wholly invisible to us, *why*, it may be asked, should the lines C, F, and the one near D—which are in truth but coloured images of the part $p\,p'$—be visible, although the spectrum of the illuminated air falling within $s\,p'$ is spread over these lines precisely as the illuminated air is itself spread over the prominence? The answer is easy. The whole of the light of the illuminated air within the small space $s\,p'$ is spread over the large space shaded with cross lines in fig. 36, and is reduced in intrinsic brightness in corresponding proportion.

Fig. 36.



On the other hand, the light of the prominence-matter within $p\ p'$ is spread only over the three lines shown in the figure (and a few fainter ones), and is therefore proportionately but very little reduced. Hence if we only have enough dispersive power, we can make sure of rendering the prominence-lines visible, for we get the same luminosity for them whatever the length of the spectrum, the only effect of an increase of length being to throw the bright lines farther apart; whereas the atmospheric spectrum which forms the background will obviously be so much the fainter as we spread its light over a longer range.

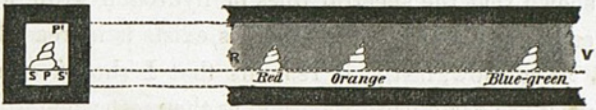
Fig. 37.



By this plan we get a certain number of images of a portion of a prominence—a mere strip so to speak; and we can get any number of such portions, and in any direction as compared with the Sun's limb. For example, if S S' (fig. 37) be the Sun's limb, P P' a prominence, we can get from such a strip as $s\ s'$ the spectrum R V. And obviously since the length of the bright lines tell us the length of the part $p\ p'$ in figs. 36 and 37, we can, by combining a number of such parallel strips as S S′, learn what is the true shape of the prominence P P′.

But the plan can be applied to show the whole of a prominence. For let us suppose that in place of a narrow strip as *s s′*, in figs. 36 and 37, we have a space such as is shown in fig. 38, through which, but for the intense brightness of the illuminated air, the prominence P P′ would be visible. Then the part S S′ of the Sun will produce a solar spectrum—altogether impure, of course, on account of the great width of S S′, and brighter than the solar spectrum produced by *s′ p′* in the case illustrated by fig. 36 in precisely the proportion that S S′ in fig. 38 is greater than *s′ p′* in fig. 36.

FIG. 38.



All the remainder of the space, including the prominence P P′, will give an impure solar spectrum due to the illuminated air, and very much brighter than in the cases illustrated in figs. 36 and 37, because so much more of this light is admitted through the open slit. Three coloured images will be formed of the prominences (other fainter ones need not be considered), one red at C, one orange near D, the other greenish-blue near F. These images will be as bright (neglecting variations in the intrinsic brilliancy of the prominence) as the corresponding lines in the cases illustrated by figs. 36 and 37; but they will of course not be so well seen, since the background, as I have said, will be very much brighter than in those cases. They can be made as conspicuous only by an increase of dispersive power; hence the importance of constructing prismatic batteries of great dispersive power.

In connection with this portion of my subject it is necessary to remark that the bright lines seen in the prominence-spectrum are not uniformly wide throughout, but commonly are wider close to the Sun's limb. This circumstance will be referred to more at length further on; but it is proper to state in this place, that this increase of width is held to indicate an increase of pressure, because the researches of Plucker, Hittorf, Huggins, and Frankland have shown that the spectral lines of hydrogen grow wider as the pressure at which the gas exists is increased.

And now, lastly, it remains that I should explain what has been justly regarded as the most wonderful of all applications of the powers of spectroscopic analysis —the measurement of the velocity of recess or approach of stars, or other self-luminous objects moving with very great rapidity.

Reverting to fig. 21, the reader will see that the violet rays are most affected by their passage through the prism, the red rays least. Now, it has been demonstrated by the careful mathematical analysis * of the

---

* I refer here to the investigations of Cauchy, and Baden Powell, and others (see specially Cauchy's *Mémoire sur la Dispersion*), not, of course, to the proof that when the differences of velocity are admitted, differences of refrangibility are accounted for. The latter may be regarded in fact as self-evident.

motions of light-waves that this difference of refrangibility is due to the different velocities with which the longer light-waves forming red light, and the shorter light-waves forming violet light, travel (respectively) through material media.* The shorter waves travel more slowly than the long ones, and the difference is the greater according to the density (or approach to opacity) of the medium. So that, in fine, the part of the spectrum formed by light of any order depends on the wave-length of that light; and if under any circumstances the wave-length could be altered, then the light of that order would no longer occupy the same portion of the spectrum, but would pass nearer to the violet end if the waves were shortened, and nearer to the red end if they were lengthened.

Now, so far as we know, it never happens that light-waves of a certain length are really modified. Precisely as waves of a certain breadth propagated along a canal are not found to change their breadth as they proceed, or, again, precisely as a sound of a certain tone does not change in tone as it travels onwards, so light-waves of a certain length or order do not as they travel through ether, or through material media, become changed into light-waves of some other order.†

---

* In the ether of space they travel of course with appreciably equal velocities; otherwise the satellites of Jupiter, after emerging from eclipse, would show the same changes of colour that we see in a metal heated from a red to a white heat.

† I have sometimes been inclined to suspect, however, that under certain circumstances of excessive agitation within the substance of the source of light, the wave-length might be altered, precisely as waves travelling along a canal might be modified in length by the action of

But there is a circumstance which may cause the light-waves to *appear* to change in length. Supposing the source of light is approaching or receding at a very rapid rate—at a rate which bears an appreciable proportion to that of light—then the length of the light-waves must needs appear modified—shortened when the source of light is approaching, lengthened when it is receding. The same will also hold if the observer be carried very rapidly towards or from the source of light. To see that this is so, it is only necessary to consider that more light-waves must necessarily reach the observer in a given time when the source of light is approaching, than when it is at rest (with respect to him), and fewer when it is receding. They must then in one case succeed each other more rapidly, and so seem to be separated by shorter intervals, while in the other they must succeed each other more slowly, and so seem to be separated by longer intervals.[*]

---

the cause which gave them birth. When we know that the C line of the prominences has been observed to be tranquil, while the F line has been broken, the idea is certainly suggested that those molecular motions within the substance of the hydrogen of the prominences, which produce that part of the light corresponding to the F line, may by some violent action be so far modified that the observed disturbance of the wave-length corresponding to that particular line may be brought about. It seems difficult to understand how, under any other circumstances, one line of the hydrogen should be undistorted, while the other is, to use Professor Young's description, 'absolutely shattered.'

[*] The principle on which this brief but sufficient explanation depends admits of several illustrations. I do not know of any which more clearly exhibits the true nature of the principle than one which I employed in the first matter I ever wrote for publication, a paper on 'The Colours of the Double Stars,' which appeared in the *Cornhill* for December, 1863. I quote the portion referred to:—'Let the reader imagine

Now, Doppler, who first called attention to this circumstance, supposed that an alteration of colour would

> himself on the bank of a canal, observing a series of waves uniformly propagated along the stream. . . . A very simple method will suffice to determine the breadth of the waves with any required degree of accuracy. Let the observer, fixing his eye on a certain wave, walk any measured distance (say 100 yards) at the same rate as the wave is moving. Suppose he accomplishes this distance in 65 seconds. He knows then that the velocity of transmission of the waves is 100 yards in 65 seconds. Let him now, standing still for 65 seconds, count the number of crests that pass him in that time. Suppose 360 pass him. Then, from his first observation, he knows that the first which passed him has travelled 100 yards from him. Within that distance all the 360 waves are uniformly distributed. Thus the breadth of each is 1-360th part of 100 yards, or ten inches. This result is perfectly reliable if, during his second observation, his position on the bank has been unchanged. But let us imagine that he has made this observation from a truck—on rails by the canal's edge—and that, unnoticed by him, the truck has glided uniformly along the rails. First, suppose that this motion has taken place in a direction contrary to that of the waves, and that while he is counting the passing crests the truck glides a distance of 20 yards. It is evident that when the last wave passes him, the first is 120 yards, instead of 100, from him. Thus the 360 waves are distributed over 120 yards, and the true breadth of each is 1-360th part of 120 yards, or 12 inches. If, on the other hand, the truck had moved over 20 yards in the same direction as the waves, it is equally obvious that the 360 waves would be distributed over only 80 yards, and the breadth of each would be only 8 inches. Similarly, at whatever rate the truck moves, it is evident that the observer can no longer depend on the result of his observations. If it moves in a direction opposite to that in which the waves travel, they appear narrower; if it travel with them they appear broader than they really are. Indeed, it is not difficult to conceive the truck to move in the same direction and at the same rate as the waves travel, in which case (if we could suppose the observer to remain unconscious of that motion) all undulation would appear to him to have ceased, and the water to have a waved but unmoving surface.'

This account illustrates in a very direct (and I think effective) manner the effect of the approach or recess of a source of light. We see that the effect depends on the ratio which the velocity of approach or recess bears to the velocity with which the waves travel. It will be seen at once that equal velocities of approach and recess produce equal but not result; and this would, in fact, be the case (though the alteration would under any conceivable conditions, be wholly inappreciable) if the source of light emitted rays of a certain order only. But in the case of such a source of light as a star—or the Sun—no change of colour could be produced, because though—to take the case of approach—the red light would be shifted towards the orange, while a portion of the violet would disappear, yet heat rays from beyond the red end would become visible as red rays through being shortened, and so the spectrum would be complete as before. A similar result would follow in the case of recession.

But the presence of dark lines in a spectrum gives the observer a far more effective means than mere change of colour would supply of determining the approach or recession of a source of light. If some

---

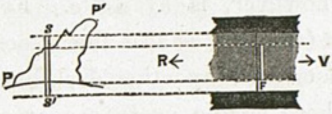
*corresponding* effects. For, by the supposed velocity of 20 yards in 65 seconds, the length of the waves is increased or diminished by two inches, but the increase is only in the ratio of 6 to 5, while the decrease is in the greater ratio of 5 to 4. Corresponding to this, we have the fact that the approach of a star at a given rate produces a greater relative effect on wave-lengths of any order than the recess of the star at the same rate would produce.

Of course, in the interesting case of stellar approach or recess, the problem is somewhat complicated by the Earth's own motion, which does not take place in the direction of the star, nor necessarily in the same plane. The successful measurement of the velocity with which Sirius is receding from us is a problem of such interest that I may be permitted to note a very small contribution of mine to the work, in the determination of the formula for eliminating the effects of the Earth's motion ($v$) viz.—Earth's motion towards star $= v \cos \lambda \sin (l - l')$ where $l$ and $l'$ are the respective longitudes of the star and the Earth, and $\lambda$ is the star's latitude. The contribution has no particular value, but, such as it is, it chanced that I made it.

recognised dark line in the spectrum of a star—say, for instance, the F line—is found not to agree exactly in position with the corresponding line in the spectrum of a fixed source of light—as a hydrogen flame, for instance—then the difference of position must be ascribed to a motion of recess or approach on the part of the star, and the rate of such motion may be determined by noticing the amount by which the line is displaced.

Now, this method admits of being applied under exceptionally favourable conditions to the examination of solar cyclonic motions—if only these motions are sufficiently rapid to fall within the province of this special mode of research. For we have in the lines appertaining to parts of the Sun which are relatively at rest the means of determining very surely, and measuring somewhat exactly, the displacement due to such motions as we are considering.

Fig. 39.



Let us take as an instance the case represented in fig. 39. Here *s s′*, as before, represents the portion of the prominence P P′ which is under examination. Only a small part of the spectrum is shown, that namely, near the F line; and the bright line of the prominence which under normal conditions coincides with the dark line F of the solar spectrum is seen to be displaced towards the violet. It thus appears that owing to a motion of approach affecting the portion of the prominence included within the narrow space *s s′*, the light-waves producing the F line seem shortened. The general fact of a motion of approach is thus ascertained. But the rate of approach can also be measured; for we know the length of the light-waves corresponding to the line F, Van der Willingen and Ängström having independently determined the wave-length corresponding to the principal lines in the solar spectrum. So that if we measured in any way the distance of the bright F line of the prominence from the dark F line of the solar spectrum, we should be able to calculate the amount of the apparent change. A better plan, however, is available. For the bright solar spectrum (fig. 39), as also the atmospheric spectrum above, is crossed by other dark lines besides the F line, and these enable us to see at once how far the bright line has shifted. Suppose *l*, for example, to be another dark line of the solar spectrum, and that the bright prominence-line has moved half-way from its proper place towards *l*, then we know that its wave-length is changed to a value midway between the wave-length corresponding to the lines F and *l*.* The change of value thus indicated gives us at once the

---

* This is true for such small displacements as are here considered. For greater differences of refrangibility, no such simple proportions exist, partly because the actual change of wave-length (for given differences of refrangibility) diminishes towards the violet end, and partly on account of the irrationality of dispersion for all known media, when the spectra they give is compared with what Ängström has called the *normal* solar spectrum.

rate of the motion of approach which affects the portion of the prominence-matter corresponding to the space *s s′*[*]—because, though the wave-length corresponding to the line *l* will not be indicated in the tables of Ängström or of Van der Willingen (which only include the principal lines) yet it is readily determinable, and indeed may be regarded as a known quantity. And, in a similar way, if the line is shifted towards the red end, the velocity of recession of the prominence-matter can readily be determined.

But as a general rule the whole line would not be shifted bodily as in fig. 39; since indeed this would imply that the *whole* of that portion of the prominence which is seen within the space *s s′* was travelling bodily towards the observer; whereas obviously such a motion could very seldom be expected to occur. Ordinarily, then, we may expect to find a configuration of the bright line indicating varieties of motion. The same holds also in the case of portions of the solar photosphere, or spots, lying near the edge of the Sun's disc (so that ordinary cyclonic motions within them may be capable of being recognised by the method we are considering), or in the case of more central portions of the Sun's disc where ascending and descending

---

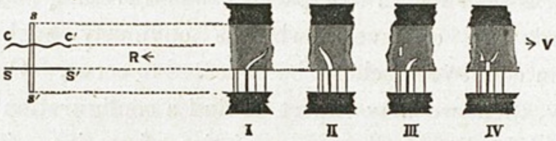
[*] The general rules on which the calculation proceeds are sufficiently simple. Suppose the wave-length corresponding to the line *l* to be 485·89 millionths of a millimeter, that corresponding to the line F being 486·39 such millionths. Then, since the prominence F-line appears half-way between F and *l*, the wave-length has been reduced to 486·19 millionths of a millimeter, or diminished by 0·2 such millionths. Hence the velocity of approach of the prominence matter is $\frac{20}{48639}$ths of the velocity of light, or some 80 miles per second.

motions are taking place, resulting in motions of recess or approach with reference to the terrestrial observer. In all such cases we may expect to find peculiarities in the affected lines, corresponding to varieties in the motion or rates of motion of the parts examined.

I give a few examples, illustrating the way in which such peculiarities are to be interpreted. I consider, for convenience, motions taking place in that coloured envelope (whence the solar prominences seem to spring) which has been called the chromosphere :—

Suppose *s s′* to represent the portion of the Sun and chromosphere under examination, S S′ being the Sun's limb, C C′ the (invisible) outline of the chromosphere. Now, if the F line appeared as in I., we should conclude that the hydrogen in the part of the chromosphere under examination was quiescent near the Sun's surface, as far as motions of approach or recess are concerned (though it might be moving very rapidly in a direction square to the line of sight), but that at some distance from the Sun's surface it was moving very rapidly towards the eye, the rate of motion increasing with the vertical height above the Sun. If, on the other hand, the spectrum appeared as at II. (fig. 40), we should come to a similar conclusion, substituting only a motion

FIG. 40.



of recession for one of approach. If the spectrum appeared as at III. we should conclude that to a certain level above the Sun's limb there was a gradually increasing motion of approach, but that at and above that level there was a motion of recession tolerably uniform in rate to a considerable height. The case would resemble those instances in our own atmosphere where an upper air current blows in a different direction than the air nearer the sea-level. If, lastly, the spectrum appeared as at IV., we should conclude that to a considerable height above the Sun's surface there was no motion of recess or approach; but that in higher regions of the chromosphere there were masses (within the long range of chromospheric matter really included in the direction of the visual line) moving both from and towards the eye at an enormously rapid rate. The greater or less width of different parts of the bright line would indicate the greater or less pressure at which the hydrogen existed at the corresponding levels during the time of observation. Hence, a bulb on any part of the bright line would indicate a corresponding layer of relatively compressed hydrogen, while a marked narrowing would indicate a layer of hydrogen existing for the time at relatively low pressure.

Similar considerations apply to the spectroscopic analysis of solar spots, or of faculous regions of the Sun's surface, or generally of any regions where disturbances may produce solar atmospheric currents of approach or recess. Combining the observed shifting of portions of a spectral line with its observed thickness and also with its relative brightness or darkness (as indicative of greater or less temperature), we have a means of studying those conditions of motion, pressure, and temperature, respecting which the telescope alone can give us no information whatever.

It is wonderful, indeed, to consider that that analysis of the dark lines of the solar spectrum which seemed half a century ago so unmeaning, those speculations of Doppler which but a quarter of a century ago were rejected by many as wholly fanciful, and those inquiries as to the almost evanescent wave-lengths of light which from the days of Newton downwards had been ridiculed as a complete waste of time and thought, should have resulted, under the labours of Bunsen and Kirchhoff, of Plucker, Huggins, and Frankland, and, finally, of Ängström and Van der Willingen, in a means of dealing with problems so recondite and seemingly so hopeless. By an observation not occupying many seconds any clear-sighted observer, armed by our opticians with adequate spectroscopic power, can measure the swiftness of the solar windstorm, can gauge the pressure of the solar atmosphere, and can estimate the relative temperature of spot and faculæ, of photosphere and chromosphere, and, lastly, of the higher regions to which eruptions cast those masses of glowing vapour which form the solar prominences.

## CHAPTER IV.

### *STUDY OF THE SUN'S SURFACE.*

We may regard the discovery of the spots on the Sun as the commencement of that long series of telescopic researches to which we owe our present knowledge of the solar orb. It is highly probable, indeed, that spots on the Sun had been seen and even watched for long intervals, when as yet astronomers were not aided by the powers of the telescope. But there is no reason to believe that the nature of the spots so seen, or even the fact that they are true solar phenomena, had ever been suspected by astronomers.* Whatever opinion we

---

* Kepler supposed that in lines 441 and 454 of Virgil's first *Georgic*, the solar spots were referred to. For 'if any one,' he reasons, 'should refuse to see anything else than an allusion to our clouds in the words

'Ille ubi nascentem maculis variaverit ortum,'

I shall oppose to the interpretation this other verse:

'Sin maculæ incipient rutilo immiscerier igni.'

But the latter verse is quite as applicable to clouds as the former. As regards the occasional recognition of spots by the ancients, however, there seems less room for doubt. We learn from Father Mailla that the Chinese recorded the appearance of spots on the Sun in the year 321 A.D., and Acosta tells us that the natives of Peru told the Spanish invaders that the Sun's face had in former times been marked with spots. In the year 807, a large spot was seen on the Sun for eight may form of ancient records of solar obscurations, we must turn to the telescopic discovery of the spots for the real commencement of astronomical researches into the Sun's physical condition.

I do not propose to enter here into the discussion which has been raised respecting the astronomer to whom the credit of having first seen the solar spots is to be assigned. This discussion has been pursued by grave authorities with an earnestness which would really seem to imply that they have regarded the matter as of serious importance. Let us simply recognise the fact that the credit of the discovery is not worth contending about,* and proceed to consider

---

successive days, and was supposed by those who were little familiar with the laws of planetary motion to be the planet Mercury. Arago is of opinion, also, that the transits of Mercury said to have been witnessed by Averrhoes, Scaliger, and Kepler himself (May 28, 1607) were only observations of Sun spots. It is worthy of notice that the ancients could very well have observed Sun spots, and have even traced the progress of these spots across the solar disc, had they employed the method which Gassenius adopted in observing the transit of Mercury in 1631. He admitted the Sun's rays into a darkened chamber through a small aperture in a shutter, and thus obtained an inverted image of the Sun, on which, when the transit had begun, he could perceive the disc of Mercury. Although no spots are ever seen which, even in the nucleus, are so dark as Mercury, yet many (or rather all the noteworthy spots) present a much more conspicuous appearance than Mercury in transit. Fabricius, indeed, as we shall presently see, did actually apply this method.

* It has been justly remarked by an eminent astronomer of our own time, that the discovery of the spots was a necessary sequel of the invention of the telescope; and whether Galileo, or Fabricius, or Scheiner, or Harriot, first set eyes on these objects, is a matter which can in no way increase the reputation of any one of these astronomers. To discuss the question of priority seems therefore to be simply a waste of time.

what the first observers actually saw — a matter of more moment.

From a work by Fabricius,* we learn that in the commencement of the year 1611, while observing the Sun just after sunrise with a telescope of inconsiderable power, he noticed a black spot upon its disc which he supposed to be a terrestrial cloud. He found, however, that the object, whatever it was, belonged to the Sun. As the Sun rose he had to discontinue his observations, for he possessed no means of mitigating the brilliancy of the Sun's light. 'My father and I,' he says, 'passed the rest of the day and the whole night in great impatience, trying to think what this spot might be. "If it is in the Sun," I said, "we shall no doubt see it again; if it is not, its motion will have carried it away from the Sun's disc, and so we shall be unable to see it." On the next morning, however, to my delight, I saw the spot again. But it was not in the same place—a peculiarity which increased our perplexity. We determined to obtain an image of the Sun on a sheet of paper by permitting his rays to pass through a small hole in a darkened chamber, and in this way we saw the spot quite clearly in the form of an elongated cloud. For three days we were prevented by bad weather from continuing our observations; but at the end of that period we again saw the spot, which had crossed obliquely towards the western side of the Sun's disc. Another smaller one

---

* Entitled *De Maculis in Sole observatis*, &c., and published at Wittemberg in 1611.

had made its appearance near the eastern edge, and in a few days this second spot reached the middle of the disc. Lastly, a third spot appeared. The three spots vanished in the order of their appearance. I was hopeful that they would be seen again, but yet perplexed by doubts and fears; however, ten days afterwards the first re-appeared on the eastern side of the disc. I knew then that it had revolved completely round (the Sun), and since then I have convinced myself that this is really the case.' Fabricius studied the import of his observations, and came to the conclusion that the spots are probably upon the body of the Sun itself. 'We invite the students of science,' he says, 'to profit by our description; they will doubtless conclude that the Sun has a motion of rotation, as Jordanus Bruno has asserted, and, more lately, Kepler. Indeed, I do not know what we could make of these spots on any other supposition.'

Galileo, at Florence, and Father Scheiner, a German Jesuit, besides independently discovering the spots, investigated the laws which regulate the motion of these objects. Scheiner was at first disposed to regard the spots as due to the existence of planets travelling round the Sun close to its surface, and indeed for a while these imagined planets were admitted as true members of the solar family under the title of the Borbonian stars.* But Galileo having pointed out

---

* They are referred to under this name in Burton's *Anatomy of Melancholy* (a very short time after their discovery), in that singularly interesting chapter which he entitles, quaintly enough, a

that the motions of the spots do not correspond to this hypothesis, but point clearly to the conclusion that the Sun rotates on his axis in about a month, Scheiner re-examined his hypothesis, and presently admitted that Galileo was in the right. He then made a long and elaborate series of observations in order to deter-

*Digression of Ayre.* The following passage from this chapter is sufficiently *apropos*, and will, I doubt not, interest the curious reader :—

'In the meantime, the world is tossed in a blanket amongst them; they hoyse the earth up and down like a ball, make it stand and goe at their pleasures. One saith the sun stands; another, he moves; a third comes in, taking them all at rebound; and, lest there should any paradox be wanting, he findes certain spots and cloudes in the sun, by the help of glasses, which multiply (saith Keplerus) a thing seen a thousand times bigger *in plano*, and make it come 32 times neerer to the eye of the beholder: but see the demonstration of this glass in Tarde, by means of which, the sun must turn round upon his own center, or they about the sun. Fabritius puts onely three, and those in the sun: Apelles, 15, and those without the sun, floating like the Cyanean isles in the Euxine sea. Tarde the Frenchman hath observed 33, and those neither spots nor clouds, as Galileus (*Epist. ad Velserum*) supposeth, but planets concentrick with the sun, and not far from him, with regular motions. Christopher Schemer [Scheiner] a German Suisser Jesuit, Ursica Rosa [Qy., in his *Rosa Ursina*], divides them *in maculas et faculas*, and will have them to be fixed *in solis superficie*, and to absolve their periodicall and regular motion in 27 or 28 dayes; holding withall the rotation of the sun upon his center; and are all so confident, that they have made skemes and tables of their motions. The Hollander, in his *dissertatiuncula cum Apelle*, censures all; and thus they disagree amongst themselves, old and new, irreconcileable in their opinions; thus Aristarchus, thus Hipparchus, thus Ptolomæus, thus Albateginus, thus Alfraganus, thus Tycho, thus Ramerus, thus Rœslinus, thus Fracastorius, thus Copernicus and his adherents, thus Clavius and Maginus, &c., with their followers, vary and determine of these celestiall orbs and bodies; and so, whilest these men contend about the sun and moon, like the philosophers in Lucian, it is to be feared the sun and moon will hide themselves, and be as much offended as shee was with those, and send another message to Jupiter, by some new fangled Icaromenippus, to make an end of all those curious controversies, and scatter them abroad.'

mine the true period of rotation and the actual position of the solar axis of rotation. In his 'Rosa Ursina' (a most monstrous volume)* he published the results of his labours. He assigned to the Sun a rotation period of between twenty-six and twenty-seven days. He also stated that the plane of the Sun's equator is inclined between 6° and 8° to the plane of the ecliptic,—a very creditable result considering his means of observation, since the best modern measures assign $7\frac{1}{3}°$ as the value of this angle.

Scheiner, Galileo, and Hevelius would seem to have independently recognised the fact that the solar spots are not of uniform brightness, but commonly surrounded by a fringe less dark than the central part. The last-named astronomer also recognised the existence of certain bright streaks in the neighbourhood of the spots. He called these the faculæ.

---

* Yet not meriting the disparaging comments of Delambre, who says, 'There are few books so diffuse and so void of facts. It contains 784 pages; there is not matter in it for fifty.' The prolixity, however, belongs to the age in which Scheiner lived, and is by no means peculiar to him. By a similar mode of judging, we should be entitled to hold in derision the works of Kepler. Lalande thus writes respecting Scheiner. 'Quoi qu'il en puisse être de celui à qui le hasard a fait voir les taches pour la première fois, il est sûr que personne ne les observa aussi bien et n'en donna la théorie d'une manière aussi complète que Scheiner. Son ouvrage a 774 pages sur cette matière, et cela suffit pour faire voir avec quelle assiduité il s'en occupa, et combien il donna d'étendue à ses recherches. Hévélius le cite avec le plus grand éloge: "Incomparabilis et omnigenæ eruditionis . . . . ut in hac materia omnibus palmam quasi præripuisse dici posset."' So much it has seemed fitting to say respecting a most laborious and ingenious investigator, whose valuable researches into solar physics have not received the credit which they deserve.

A long series of observations of Sun-spots now began, and many hypotheses of more or less ingenuity were put forward to account for the phenomena which they present. For some time, indeed, the possibility of their existence was earnestly denied by the students of Aristotelian philosophy. It is impossible, they gravely urged, that the Eye of the Universe should suffer from ophthalmia; and it is related that when Scheiner communicated his discovery of the solar spots to the provincial of his order, the latter, who was an earnest Aristotelian, answered, 'I have read Aristotle's writings from beginning to end many times, and I can assure you I have nowhere found in them anything similar to what you mention; go, therefore, my son; tranquillise yourself; be assured that what you take for spots in the Sun are the faults of your glasses or your eyes.'

Despite the defenders of Aristotle's infallibility, however, the progress of solar research went on. Galileo continued his labours, until, from viewing the Sun so often without the dark glasses now commonly employed, he lost his eyesight. Scheiner, Hevelius, and other observers, added largely to the store of known facts; and gradually the observation of solar spots began to be recognised as a regular part of the astronomer's work.

I do not propose, however, to give a detailed account of the observations made in those earlier years of telescopic observation of the Sun. Indeed, to give a full history of those observations, and to extend the same fulness of narrative to later researches, would require twenty or thirty such volumes as the present.

I shall content myself with selecting certain illustrative instances of solar observation by the earlier astronomers, choosing those observations specially which tend to introduce the results more completely educed by recent researches.

Cassini writes thus of a spot observed by him in August 1671.* 'It is now about twenty years since, that astronomers have not seen any considerable spots on the Sun, though before that time, since the invention of the telescope, they have from time to time observed them. The Sun appeared all that while with an entire brightness, and I saw him so on the ninth of the month of August. But on the eleventh of the same, about six a clock at night, being furnisht only with a three-foot glass, I remarked in the Sun's disque two spots very dark, distant from his apparent centre about the third part of his semi-diameter. . . . The first of these spots, being look'd upon with a telescope of seventeen foot long, appeared of a somewhat oval figure; the other was oblong and a little curved, like the Hebrew letter Jod; and both together were surrounded by a *corolla* or coronet made up of little dark points' (the penumbra) 'which conformed itself to the figure of the spots, considered as they were joined together. . . . The twelfth of August, 1671, I ob-

---

* *New Observations of Spots in the Sun; made at the Royal Academy of Paris on August* 11, 12, and 13, 1671; *and English't out of the French. Phil. Trans.* vol. vi. p. 2,251.

served them from the time of sun-rising, and perceived that they were nearer his centre. . . . The first was composed of two others almost round and conjoyn'd. The second represented the shape of a scorpion. The third was round' (this is the first intimation we have of the triplicity of the group), 'and they were all three environed with a coronet, which was composed, as said above, of abundance of little obscure pricks. This coronet appeared to be clearer than the rest of the Sun when looked upon with the short glass, and darker when seen with the long. Without it there were other points, but very black ones, viz. five near the round spot on the south side, and another near the scorpion's tail on the north side. At eight a clock and forty-eight minuts, the figure of the scorpion was seen divided into several pieces, as if his tail and arms had been cut off. The northern point remained no more, there remaining none but those seen on the south side; and the length of the enclosure of all the spots, comprehended between the extremities, was of one minut and fifteen seconds, and the breadth of thirty seconds.' On August 12 Cassini found no great change had taken place, but the black points outside the spot were now spread in a straight row. On the 13th 'the edge of the coronet was turned to a point on the south side.' The spot had indeed changed in that strange way which all observers of the Sun must be familiar with, the following extremity of the penumbra drawn out to a point which was so curled round as to be directed towards the preceding end of the spot.

Cassini afterwards traced the progress of this spot to the Sun's limb. He remarks, 'The apparent velocity nigh the centre was such that if it had continued the same, the spots would have arrived almost in four days to the limb of the disque; but in the hypothesis'—that they are either attached to the Sun's surface or not far from it—'this apparent velocity was to lessen according as the spots should remove from the centre; as hath come to pass in effect. The diminution in the length of the misty crown' (a strange name for the penumbra) 'was in a manner proportionat to the diminution of the apparent velocity; since that when this crown was in the midle, and in a scituation wherein its true figure could be best seen, it appeared oblong and of the form of an human ear, its greatest diameter respecting east and west; but being nigh the limb, this same diameter seemed to shorten; and having appeared greatest in its first scituation, it appeared least in this, because it was almost in a circle that passed through the centre of the Sun, whose equal arches are by so much the more oblique by how much they approach more to the limb of his disque, and consequently appear less according to the rules of opticks; meantime the diameter that was turned from north to south apparently kept the same bigness it had near the centre, because it was in a circle almost parallel to the horizon of the Sun, which formed the representation of its limb, and whose equal arches' (by the same optical reasons) 'do not appear contracted.' It will be seen by this reasoning—which, being interpreted, signifies that the effect of foreshortening was to make the spot seem longest in a direction square to a line from the Sun's centre—that Cassini had on this occasion come very near to the discovery afterwards made by Dr. Wilson, that the nucleus of a spot is at a lower level than the solar photosphere. For had he but noticed an excess of breadth in the penumbra nearest to the Sun's limb, the same just reasoning which he applied to the figure of the spot would have enabled him to pronounce at once respecting the relative level of the nucleus, the penumbra, and the photosphere. He gives a picture of the spot close by the Sun's limb, in which the penumbra is of equal width on the side next the centre and on that next the limb. Most probably no such peculiarity as Dr. Wilson detected, existed in the case of this particular spot. In fact, so far as my own experience of the aspect of Sun-spots is concerned, spots such as the one observed by Cassini seldom exhibit the peculiarity in question in a manner which would enable an observer to theorise safely respecting the level at which nucleus, penumbra, and photosphere actually lie.

In November 1769, Dr. Wilson, of Glasgow, began the careful study of a large spot (visible to the naked eye). The results which he deduced from its changes of appearance are of great interest and importance. When he first examined the spot (November 22) it was situated not very far from the western edge of the Sun's disc. On the next day he found that the spot had changed in appearance. ‘The penumbra which on the previous day was equally broad on all sides of the nucleus * was now very much contracted on the side which lay towards the centre of the disc, while the other parts retained nearly their former dimensions. On the 24th he again observed the spot. The distance from the limb was now only twenty-four seconds, and the contracted side of the penumbra had entirely vanished. The breadth of the nucleus on the same side also appeared to be more suddenly impaired than it ought to have been by the motion of the Sun across the disc.’

Dr. Wilson showed that these results correspond with those which would follow if the spot was a vast cavernous opening, having the nucleus at the bottom, and the penumbra forming its sloping sides. It only remained to be seen whether a corresponding succession of changes occurred when the spot re-appeared on the eastern edge, and thence passed across the solar disc. This actually happened. ‘On December 11, the spot appeared on the opposite side of the disc. It was then distant about a minute and a half from the edge. The side of the penumbra next the edge, which formerly vanished, was now wholly visible, while that turned towards the centre of the disc appeared to be wanting. On December 12 it came into view, and he saw it distinctly, though narrower than the other side. He did not see the spot again until December 17, when it

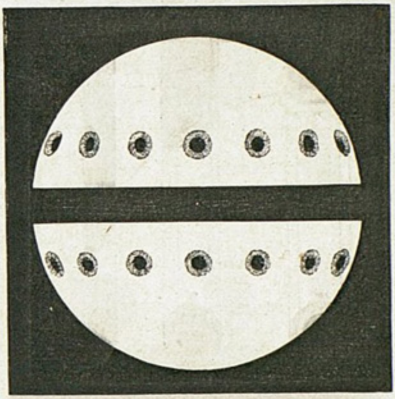
---

* From Professor Grant's abstract of the original narrative, in his excellent *History of Physical Astronomy*.

had passed the centre of the disc, and the penumbra now appeared to surround the nucleus equally on all sides.

In fig. 41 the upper row of spots represents the succession of changes actually presented by this spot, while the lower shows what would occur as a spot traversed the Sun's disc, if the spot were simply a surface stain with a penumbral fringe. It will be seen that the difference between the appearances depicted in the two rows is of a sufficiently marked character. It will also be noticed that even without any definite explanation of the peculiarities shown in the upper row, the mind at once recognises the fact that we have here to do with a cavity or depression.

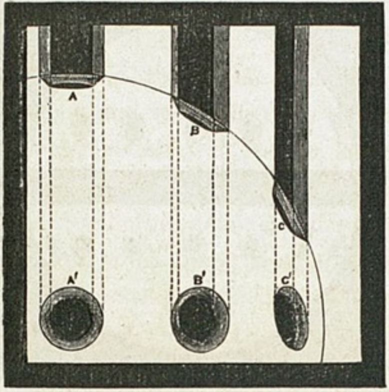
Fig. 41.



Let us examine, however, the line of reasoning by which Dr. Wilson demonstrated this.

Let A, B, C (fig. 42)* be supposed to represent perspective views of a saucer-shaped depression on the surface of a sphere—the depressions being all of like dimensions (the sloping sides are assumed in *these three* views to be transparent). Then it is obvious that to an eye, supposed to view them from above, the relative breadth of the black base and of the shaded sides would be indicated by the breadth of the dark and shaded spaces carried vertically upwards from the spots. Now, suppose the sphere to be rotated on a horizontal axis so that the spots are brought (their edges sliding as it were along the dotted lines) down to the positions A′, B′, C.′ Then the relative breadths of the penumbra and nucleus will be indicated by the

FIG. 42.



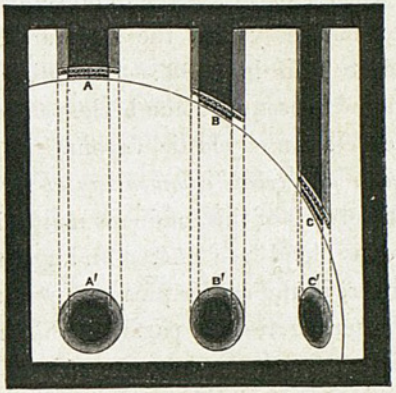
---

* This figure in essentials closely resembles a figure in Fr. Secchi's recently published work on the Sun. It was drawn by me, however, and employed in illustrating lectures, nearly a year before Secchi's book appeared.

distances separating these dotted lines, and must needs therefore be such as are shown in the figure. The shape of the depression would also be as shown. We see, then, that to an eye watching a depression of this sort as it rotated from the position A′ to the position C′, changes of shape would be shown which correspond exactly with those recognised by Dr. Wilson.

This is the proper place to point out, however, that Dr. Wilson's observations were insufficient to demonstrate that a spot is a region which lies *as a whole* beneath the solar surface. They show only that the nucleus of the spot he observed lay at a lower level than the penumbra. It is evident from fig. 43 that appearances precisely corresponding to those observed by Wilson would be seen if spots were caused by a double layer of clouds in the solar atmosphere, the lower opaque, the upper semi-transparent and ex-

Fig. 43.



tending on all sides beyond the limits covered by the lower. Such an interpretation has indeed been put forward in recent times by Kirchhoff, and is still maintained, I believe, by Spörer.

Wilson's physical interpretation of the occurrence of these solar depressions need not detain us here. They have not been regarded as so successful as his geometrical analysis of the observed phenomena. It is only just to add that he himself did not attach equal weight to them; for in answer to objections urged by Lalande to his theory that the spots are depressions, Wilson wrote thus in 1783:—'Whether their first production and subsequent numberless changes depend upon the eructation of elastic vapours from below, or *upon eddies or whirlpools commencing at the surface*, or upon the dissolving of this luminous matter in the solar atmosphere, as clouds are melted and again given out by our air, or, if the reader pleases, upon the annihilation and reproduction of parts of this resplendent covering, is left for theory to guess at.'

Passing also over the theories of Bode, which differed in no important respect from those of Wilson, let us turn now to the observations and theories of the greatest observational astronomer the world has ever known—Sir William Herschel. I propose to treat at considerable length what he has advanced upon the subject of the Sun, partly because of the great value of his work, but partly also to reclaim for him many discoveries which have been assigned to later observers. What these are I need not in every instance particularise; but it seems to me essential that the original observation of facts of so much interest and importance, only determinable by long and patient scrutiny of the Sun, should be assigned to their proper place.

Let me begin by quoting the fine passage in which Sir William Herschel speaks of the central luminary of our system.

'Among the celestial bodies,' he says, 'the Sun is certainly the first which should attract our notice. It is a fountain of light that illuminates the world! it is the cause of that heat which maintains the productive power of nature, and makes the Earth a fit habitation for man! it is the central body of the planetary system; and what renders a knowledge of its nature still more interesting to us, is that the numberless stars which compose the universe, appear by the strictest analogy to be similar bodies. Their innate light is so intense, that it reaches the eye of the observer from the remotest region of space, and forcibly claims his notice.'

Next let us hear his summary of the theories which had been put forward respecting the physical constitution of the Sun. I may note in passing that the opening remarks are as applicable in the present day as when Herschel wrote them:—'I should not wonder,' says the great astronomer, 'if we were induced to think that nothing remained to complete our knowledge; and yet it will not be difficult to show that we are still very ignorant, at least with regard to the internal constitution of the Sun. The various conjectures which have been formed on this subject are evident marks of the uncertainty under which we have hitherto laboured. The dark spots in the Sun, for instance, have been supposed to be solid bodies revolving very near its surface. They have been conjectured to be the smoke of volcanoes, or the scum floating upon an ocean of fluid matter. They have also been taken for clouds. They were explained to be opaque masses, swimming in the fluid matter of the Sun; dipping down occasionally. It has been supposed that a fiery liquid surrounded the Sun, and that, by its ebbing and flowing, the highest parts of it were occasionally uncovered, and appeared under the shape of dark spots; and that by the return of this fiery liquid they were again covered, and in that manner successively assumed different phases. The Sun itself has been called a globe of fire, though perhaps metaphorically. The waste it would undergo by a gradual consumption, on the supposition of its being ignited, has been ingeniously calculated. And in the same point of view its immense power of heating the bodies of such comets as draw very near to it has been assigned.' 'In supporting,' he proceeds, 'the ideas I shall propose in this paper with regard to the physical constitution of the Sun, I have availed myself of the labours of all these astronomers, but have been induced thereto only by my own actual observation of the solar phenomena, which, besides verifying those particulars that had been already observed, gave me such views of the solar regions as led to the foundation of a very rational system. For having the advantage of former observations, my latest reviews of the body of the Sun were immediately directed to the most essential points; and the work was by this means facilitated and contracted into a pretty narrow compass.'

'In the year 1779,' he begins, 'there was a spot on the Sun which was large enough to be seen with the naked eye. By a view of it with a 7-feet reflector, charged with a very high power, it appeared to be divided into two parts. The largest of the two on April 19 measured 1′ 8″·06 in diameter, which is equal in length to more than 31,000 miles. Both together must certainly have exceeded 50,000. The idea of its being occasioned by a volcanic explosion, violently driving away a fiery fluid, which on its return would gradually fill up the vacancy, and thus restore the Sun, in that place, to its former splendour, ought to be rejected on many accounts. To mention only one, the great extent of the spot is very unfavourable to that supposition. Indeed, a much less violent and pernicious cause may be assigned, to account for all the appearances of the spot.

'The Earth is surrounded by an atmosphere composed of various elastic fluids. The Sun, also, has its atmosphere, and if some of the fluids which enter into its composition should be of a shining brilliancy while others are merely transparent, any temporary cause which may remove the lucid fluid will permit us to see the body of the Sun through the transparent ones. If an observer were placed on the Moon, he would see the solid body of our Earth only in those places where the transparent fluids of the atmosphere would permit him. In others the opaque vapours would reflect the light of the Sun without permitting his view to penetrate to the surface of our globe. He would probably also find that our planet had occasionally some shining fluids in its atmosphere; as, not unlikely, some of our northern lights might attract his notice, if they happened in the unenlightened part of the Earth, and were seen by him in his long dark night. Nay, we have pretty good reason to believe, that probably all the planets emit light in some degree; for the illumination which remains on the Moon in a total eclipse cannot be entirely ascribed to the light which may reach it by the refraction of the Earth's atmosphere.* . . In the instance of one large spot on the Sun, I concluded, from appearances, that I viewed the real solid body of the Sun itself, of which we rarely see more than its shining atmosphere.'

'In the year 1783,' he proceeds, 'I observed a fine large spot, and followed it up to the edge of the Sun's limb. Here I took notice that the spot was plainly depressed below the surface of the Sun; and that it had very broad shelving sides. I also suspected some

---

* There is, however, an important flaw in the reasoning on which Sir William Herschel bases this opinion. He takes the horizontal refraction due to a ray of solar light so entering the Earth's atmosphere as to reach the surface of the Earth tangentially, without noticing that the same amount of refraction will affect the ray as it passes out of the terrestrial atmosphere. This error is repeated in Ferguson's *Astronomy*, and has since been often quoted without correction.

A PORTION OF THE SUNS DISC

as seen in May 1870, by Mr. Browning F.R.A.S.

WITH A 12¼ INCH BROWNING_WITH REFLECTOR.

M&N. HANHART LITH.

part at least of the shelving sides to be elevated above the surface of the Sun; and observed that, contrary to what usually happens, the margin of that side of the spot which was farthest from the limb was the broadest.' It will be noticed that in the picture of the Sun presented in Plate I., Mr. Browning delineates in the case of one spot of the group a precisely analogous appearance.

Sir William Herschel's explanation of these peculiar appearances need not be quoted, as it has been disposed of by recent researches.

'In the year 1791,' he proceeds, 'I examined a large spot in the Sun, and found it evidently depressed below the level of the surface; about the third part was a broad margin or plane of considerable extent, less bright than the Sun, and also lower than its surface. This plane seemed to rise, with shelving sides, up to the place where it joined the level of the surface.'*

'How very ill,' proceeds Herschel, 'would this

---

* Herschel reasons that there could have been no deception in this appearance, because the Sun *looked* convex, whereas he had noticed that on those occasions when the Moon's mountains and valleys were apparently reversed, the Moon herself always looked concave, the illusion disappearing when the mind was directed to the fact of the Moon being in truth convex. It may be questioned, however, whether this reasoning can fairly be applied to a self-luminous body. The peculiarity affecting the apparent concavity or convexity of the lunar mountains or craters, depends entirely on the ideas present in the mind at the moment of observation, respecting the direction in which the source of illumination lies—precisely as in the analogous experiment with a seal or coin, discussed in Brewster's *Natural Magic*. Such considerations cannot affect our views respecting a self-luminous body.

A PORTION OF THE SUNS DISC

as seen in May 1870, by Mr Browning F.R.A.S.

WITH A 12½ INCH BROWNING-WITH REFLECTOR.

part at least of the shelving sides to be elevated above the surface of the Sun; and observed that, contrary to what usually happens, the margin of that side of the spot which was farthest from the limb was the broadest.' It will be noticed that in the picture of the Sun presented in Plate I., Mr. Browning delineates in the case of one spot of the group a precisely analogous appearance.

Sir William Herschel's explanation of these peculiar appearances need not be quoted, as it has been disposed of by recent researches.

'In the year 1791,' he proceeds, 'I examined a large spot in the Sun, and found it evidently depressed below the level of the surface; about the third part was a broad margin or plane of considerable extent, less bright than the Sun, and also lower than its surface. This plane seemed to rise, with shelving sides, up to the place where it joined the level of the surface.'*

'How very ill,' proceeds Herschel, 'would this

---

* Herschel reasons that there could have been no deception in this appearance, because the Sun *looked* convex, whereas he had noticed that on those occasions when the Moon's mountains and valleys were apparently reversed, the Moon herself always looked concave, the illusion disappearing when the mind was directed to the fact of the Moon being in truth convex. It may be questioned, however, whether this reasoning can fairly be applied to a self-luminous body. The peculiarity affecting the apparent concavity or convexity of the lunar mountains or craters, depends entirely on the ideas present in the mind at the moment of observation, respecting the direction in which the source of illumination lies—precisely as in the analogous experiment with a seal or coin, discussed in Brewster's *Natural Magic.* Such considerations cannot affect our views respecting a self-luminous body.

observation agree with the ideas of solid bodies bobbing up and down in a fiery liquid—with the smoke of volcanoes, or scum upon an ocean; and how easily is it explained upon our foregoing theory. The removal of the shining atmosphere, which permits us to see the Sun, must naturally be attended with a gradual diminution on its borders; an instance of a similar kind we have daily before us, when through the opening of a cloud we see the sky, which generally is attended by a surrounding haziness of some short extent, and seldom transits from a perfect clearness to its greatest obscurity.'

On August 26, 1792, Herschel examined the Sun with several powers, from 90 to 500. 'It appears evidently' he remarks, 'that the black spots are the opaque ground or body of the Sun; and that the luminous part is an atmosphere, which, being interrupted or broken, gives us a transient glimpse of the Sun itself.' He presently suggests that possibly even where there are no spots, the real surface of the Sun may now and then be perceived—'as we see the shape of the wick of a candle through its flame, or the contents of a furnace in the midst of the brightest glare of it; but this, I should suppose, will only happen where the lucid matter of the Sun is not very accumulated.'

A few days later he studied some well-marked faculæ. In the neighbourhood of a dark spot pretty near the edge, 'I saw,' he says, 'a great number of elevated bright places, making various figures. I shall call them *faculæ*, with Hevelius; but without assigning to this term any other meaning than what it will hereafter appear ought to be given to it. I see these faculæ extended on the preceding side, over about one-sixth part of the Sun; but so far from resembling torches, they appear to me like the shrivelled elevations upon a dried apple, extended in length; and most of them are joined together, making waves, or waving lines. By some good views in the afternoon, I find that the rest of the surface of the Sun does not contain any faculæ, except a few on the following and equatorial part of the Sun. Towards the north and south I see no faculæ; there is all over the Sun a great unevenness in the surface, which has the appearance of a mixture of small points of an unequal light; but

FIG. 44.

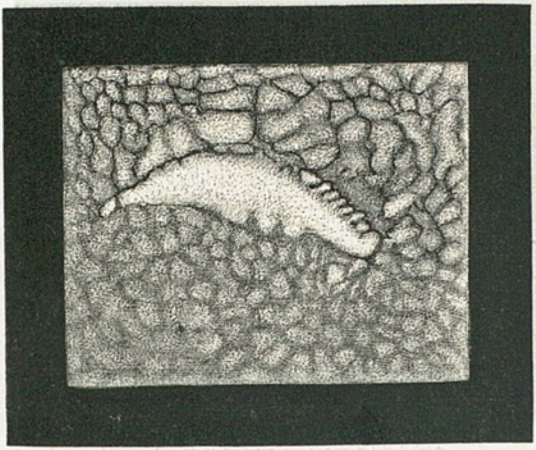
The Sun's corrugated surface.—*Secchi.*

they are evidently an unevenness or roughness of high and low parts.'

The accompanying views (figs. 44 and 45) of portions of the Sun's surface, as delineated by Secchi with the fine telescope of the Roman observatory, correspond exactly with this description. Note also the drawing by Chacornac (fig. 55) farther on.

After a week's observations of the faculæ, Herschel thus reasons about them:—'The faculæ being elevations, very satisfactorily explains the reason why they disappear towards the middle of the Sun, and reappear on the other margin; for, about the place where we lose them they begin to be edgeways to our view; and if between the faculæ should lie dark spots, they will most frequently break out in the middle of the Sun,

Fig. 45.



The Sun's surface, showing facula.—*Secchi.*

because they are no longer covered by the side views of these faculæ.'*

On September 22, 1792, Herschel observed few faculæ on the Sun and few spots, but the whole disc very much marked with roughness, like an orange, and some of the lowest parts of the inequalities blackish. On the following day he thus associates this roughness with the faculæ :—'The faculæ are ridges of elevation above the rough surface.'†

On February 23, 1794, Herschel noticed an appearance which is very well illustrated in Mr. Browning's drawing of the Sun, Plate I. 'One of the black spots on the preceding margin, which was greatly below the margin of the Sun, had, next to it, a protuberant lump of shining matter, a little brighter than the rest of the Sun. About all the spots,' he adds, 'the shining matter seems to have been disturbed; and is even lumpy and zig-zagged in an irregular manner. I call the spots black, not that they are entirely so, but merely to distinguish them; for there is not one of them to-day which is not partly or entirely covered with whitish and unequally bright nebulosity or cloudiness. This in many of them comes

---

[*] Perhaps the most convincing proof we have of the fact that faculæ are elevations, is that supplied by Mr. Dawes, who actually saw a facula projecting from the edge of the Sun's disc.

[†] The observation next following the above in Herschel's paper (*Phil. Trans.* vol. lxxxv.) is worth citing, though the fact involved is now well-known to all observers of the Sun. It runs thus:—'Feb. 23, 1794. By an experiment I have just now tried, I find it confirmed that the Sun cannot be so distinctly viewed with a small aperture and faint darkening glasses as with a large aperture and stronger ones; this latter is the method I always use.'

near to an extinction of the spot, and in others, seems to bring on a subdivision.'

On September 28, 1794, Sir William Herschel observed a spot similar in its general characteristics to that on the observation of which Wilson had based his hypothesis. ' There is a dark spot in the Sun on the following side. It is certainly depressed below the shining atmosphere, and has shelving sides of shining matter which rise up higher than the general surface, and are brightest at the top. The preceding shelving side is rendered almost invisible by the overhanging of the preceding elevations; while the following is very well exposed; the spot being apparently such in figure as denotes a circular form viewed in an oblique direction. Near the following margin are many bright elevations close to visible depressions. The depressed parts are less bright than the common surface. The penumbra, as it is called, about this spot is a considerable plane, of less brightness than the common surface, and seems to be as much depressed below that surface as the spot is below the plane. Hence, if the brightness of the Sun is occasioned by the lucid atmosphere, the intensity of the brightness must be less where it is depressed; for light being transparent, must be the more intense the more it is deep.'

Having thus described the most striking of his first series of observations, Herschel now proceeds to enunciate his theory respecting the solar constitution.

He remarks, in the first place, that it cannot be doubted but that the Sun has a very extensive atmosphere; 'and that this atmosphere,' he proceeds, 'consists of various elastic fluids, that are more or less lucid and transparent, and of which the lucid one is that which furnishes us with light, seems also to be fully established by all the phenomena of its spots, of the faculæ, and of the lucid surface itself. There is no kind of variety in these appearances but what may be accounted for with the greatest facility, from the continued agitation which we may easily conceive must take place in the regions of such elastic fluids.'

After dwelling on certain illustrations drawn from the clouds in our own atmosphere, which Herschel (strangely enough) regards as 'probably *decompositions* of some of the elastic fluids of the atmosphere itself,' Herschel points out that the analogy of our own atmosphere will not be less to his purpose to whatever cause the clouds may owe their origin. 'The lucid clouds of the Sun, so to call them, plainly exist, because we see them; the manner of their being generated may remain an hypothesis, and mine, till a better can be proposed, may stand good; but whether it does or not, the consequences I am going to draw from what has been said will not be affected by it.' *

---

* It is a peculiarity of Sir William Herschel's reasoning, that it is nearly always divided very definitely into two portions, which yet many who study his writings are apt to confound. We find certain conclusions on which Sir William Herschel insists, and certain hypotheses which he simply enunciates. Respecting these last, he has perhaps as often been in the wrong as in the right, and it indicates his surprising acumen, that so even a proportion should be observed in the case of mere hypotheses. Respecting the former, I cannot recall one instance in which he has been proved to have been in error. Owing to his singular clearness of mental vision, and also in part to the extreme lucidity of

Herschel then states that he regards the spots as regions where the atmosphere is free from lucid clouds, the faculæ as regions where such clouds are more numerous than elsewhere. The penumbra being generally depressed about half-way between the level of the nucleus and that of the photosphere, must of course be fainter than other parts. 'No spot favourable for taking measures having lately been on the Sun,' he adds, 'I can only judge from former appearances that the regions in which the luminous solar clouds are formed, adding thereto the elevation of the faculæ, cannot be less than 1,843, nor much more than 2,765 miles in depth. It is true that in our atmosphere the extent of the clouds is limited to a very narrow compass; but we ought rather to compare the solar ones to the luminous decompositions which take place in our aurora borealis, or luminous arches, which extend much farther than the cloudy regions. The density of the luminous solar clouds, though very great, may not be exceedingly more so than that of our aurora borealis. For if we consider what would be the brilliancy of a space two or three thousand miles deep filled with such corruscations as we see now and then in our atmosphere, their apparent intensity, when viewed at the distance of the Sun, might not be much inferior to that of the lucid solar fluid.'

his descriptions, one is very apt to forget, when he is describing mere hypotheses, that he is not discussing established conclusions; and to this probably is due the fact that some of his warmest admirers do him the injustice of insisting as earnestly on views which he put forward simply as hypotheses, as though they had been enunciated by him as legitimate deductions from observed facts.

From the luminous atmosphere of the Sun, Herschel proceeds to the opaque body, which he surmises to be of great solidity, on account of the power it exerts upon the planets. From the phenomena of those dark spots which have been repeatedly seen in the same place, 'and otherwise denote inequalities in their level,' he suggests that the Sun's surface 'is diversified with mountains and valleys.'

Then follows that remarkable passage which every student of astronomy knows by heart; but which yet (even though we may not accept—as I confess I do not—the opinions suggested in it) will well bear repetition:—

'The Sun, viewed in this light, appears to be nothing else than a very eminent, large, and lucid planet, evidently the first, or, in strictness of speaking, the only primary one of our system, all others being truly secondary to it. Its similarity to the other globes of the solar system with regard to its solidity, its atmosphere, and its diversified surface; the rotation upon its axis, and the fall of heavy bodies, lead us on to suppose that it is most probably also inhabited, like the rest of the planets, by beings whose organs are adapted to the peculiar circumstances of that vast globe. Whatever fanciful poets might say in making the Sun the abode of blessed spirits, or angry moralists devise in pointing it out as a fit place for the punishment of the wicked, it does not appear that they had any other foundations than mere opinion and vague surmise; but now I think myself authorised, *upon* *astronomical principles*, to propose the Sun as an inhabitable world, and am persuaded that the foregoing observations, with the conclusions I have drawn from them, are fully sufficient to answer every objection that may be made against it.'

Herschel proceeds to consider the objection founded on the great heat which here at a distance of so many millions of miles we receive from the Sun, and the tremendous nature of the heat which consequently (one would suppose) must affect the imagined inhabitants of the Sun. Our admiration for the greatest astronomer of modern times must not cause us to lose sight of the fact that the reasoning at this stage of the inquiry is founded on inexact notions of the nature and laws of heat—though not such as in his day could have been unfavourably commented upon by most physicists. He remarks that the Sun's rays are 'the cause of the production of heat by uniting with the matter of fire which is contained in the substances that are heated.' He then instances the snow-covered summits of lofty mountains, and the cold experienced by aeronauts; and he concludes, 'that we have only to admit that on the Sun itself, the elastic fluids composing its atmosphere, and the matter on its surface, are of such a nature as not to be capable of any excessive affection from its own rays, which seems indeed to be proved by their copious emission.'*

---

* After noting other possible objections, Sir William Herschel—who did not disdain at times to be as imaginative and fanciful in theorising as he was exact and scrupulous in observing—proceeds to consider the possibility that the inhabitants of the Moon and of the satellites of

In a later paper (communicated to the Royal Society in 1801) Sir William Herschel records the results of further observations. He draws special attention to certain characteristic features of the Sun's surface. These are, first, *corrugations,* which he regards as elevations and depressions causing the mottled appearance of the Sun; secondly, *nodules,* or smaller elevations in the corrugations themselves over which they are distributed as bright spots; *punctulations,* or dark spaces between the nodules; and *pores,* or 'darker-coloured places in the punctulations. He also enters into many particulars as to the behaviour of spots, pores, corrugations, nodules, and so on. To this valuable paper, as to the other, from which I have quoted, I would invite the attention of every reader

---

Jupiter, Saturn, and Uranus, regard the primary orbs round which they travel as mere attractive centres, to keep together their orbits, to direct their revolution round the Sun, and to supply them with reflected light in the absence of direct illumination. 'Ought we not,' he asks, 'to condemn their ignorance, as proceeding from want of attention and proper reflection? It is very true that the Earth and those other planets that have satellites about them, perform all the offices that have been named for the inhabitants of these little globes; but to us, who live upon one of these planets, their reasonings cannot but appear very defective, when we see what a magnificent dwelling-place the Earth affords to numberless intelligent beings. These considerations ought to make the inhabitants of the planets wiser than we have supposed those of their satellites to be. We surely ought not, like them, to say, 'The Sun (that immense globe, whose body would much more than fill the whole orbit of the Moon) is merely an attractive centre to us. From experience we can affirm that the performance of the most salutary offices to inferior planets is not inconsistent with the dignity of superior purposes; and in consequence of such analogical reasonings, assisted by telescopic views, which plainly favour the same opinion, we need not hesitate to admit that the Sun is richly stored with inhabitants.'

interested in solar physics. Here I shall only quote two observations bearing on the periodicity of the disturbances which affect the Sun's surface. We have seen that in 1671 Cassini had for a long time noticed the absence of Sun-spots. But on July 5, 1795, Sir William Herschel remarked that the Sun presented an appearance far more remarkable, and such, he remarks, as differed wholly 'from what he had ever seen before. There was not a single opening in the whole disc; there were no ridges or nodules, and no corrugations.' On December 9, 1798, he noticed that a similar state of things prevailed.

We may sum up as follows the views of Sir William Herschel as to the general constitution of the solar globe and surface:—He supposed the Sun to be an opaque globe surrounded by a luminous envelope. He considered that this envelope is neither fluid nor gaseous, but consists rather of luminous clouds floating in a transparent atmosphere. Beneath this layer or envelope of luminous clouds he conceived that there floats in the same atmosphere a layer of opaque clouds, rendered luminous on the outside by the light which they receive from the outer layer. These opaque clouds protect, according to this theory, the solid and relatively unilluminated nucleus of the Sun. When openings are formed in the same region in both layers of clouds, we see the body of the Sun as a dark spot. If the apertures are equally large, the spot will be uniformly dark; but if, as more commonly happens, the outer aperture is the greater, the dark nucleus of the spot will seem to be surrounded by a dusky border. If the upper layer alone is perforated, a dusky spot without any dark central portion makes its appearance. Herschel supposed that those spots in which both layers are broken through, are caused by an uprush of some highly elastic gas breaking its way through the lower layer, and then, after expansion, removing the upper self-luminous clouds.

We shall see that while all the facts observed by Herschel have been confirmed, and while his reasoning, so far as it relates to observed facts, has been abundantly justified, some of his hypotheses have been disproved by recent observations.

I pass on next to the researches of Sir John Herschel, recorded in that store-house of valuable facts, the 'Results of Astronomical Observations at the South Cape.'

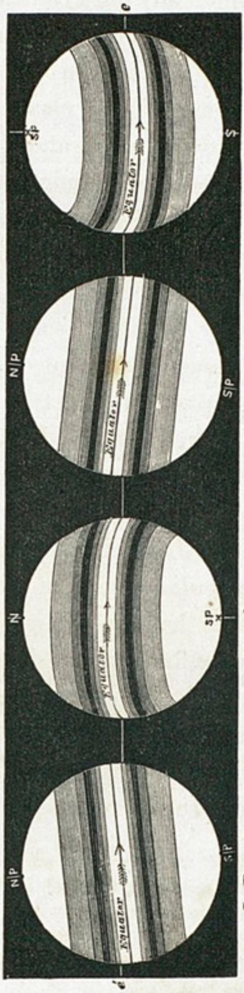
Sir John Herschel's observations led him to pay particular attention to a feature of the solar surface which had been first noted by Galileo. The spots are confined to two definite zones, extending about 35°* on each side of the equator; an intermediate zone to a distance of some 8° on either side of the solar equator being ordinarily free from spots. Fig. 46 serves to indicate the regions where spots occur, and also (where

---

[*] If we may trust an observation of La Hire's (which, however, Mr. Carrington, than whom no higher authority can be cited, is disposed to reject), a spot has been seen as far as 70° from the Sun's equator. In 1846, Dr. Peters, of Altona, saw a spot 50° 55′ from the equator, while Carrington and Capocci have each seen spots about 45° from that circle.

the darkest zones are shown) those regions in which spots occur most frequently and attain the greatest dimensions. The Sun is so placed in the four views as to show the way in which the spot-belts are actually presented—I. early in December; II. early in March; III. early in June; and IV. early in September. The actual dates are those indicated under the several figures. I shall have to discuss further on those researches of Carrington and others into the laws of the Sun's rotation and the position of the solar equator * on which these

Fig. 46.



I. Dec. 6.

II. March 6.

III. June 5.

IV. Sept. 5.

---

* It is surprising that in Lockyer's *Elementary Lessons of Astronomy*, not only are pictures admitted in which the effects of the Sun's inclination are altogether exaggerated, but the author actually states that in September and March (corresponding to figs. 3 and 4) the paths of the spots are observed to be *sharply* curved. This is the more astonishing, because when that book was published, Mr. Lockyer had long been an observer of solar phenomena; and the slight nature of the curvature is a peculiarity which can hardly escape notice, even though observations are continued only for a few days in September or in March.

dates and the presentations shown in fig. 46 are founded.

It was to the explanation of this peculiarity that Sir John Herschel directed his chief attention. He remarks that the very existence of these zones 'at once refers the cause of spots to fluid circulations, modified, if not produced, by the Sun's rotation, by reasoning of the very same kind whereby we connect our own system of trade and anti-trade winds with the Earth's rotation. Having given any exciting cause for the circulation of atmospheric fluids from the poles to the equator and back again, or *vice versâ*, the effect of rotation will necessarily be to modify those currents as our trade-winds and monsoons are modified, and to dispose all those meteorological phenomena (on a great scale) which accompany them as their visible manifestations, in zones parallel to the equator with a calm equatorial zone interposed.' Thus far, be it observed, Sir John Herschel is dealing with observed facts, and pointing to almost inevitable conclusions. He passes on (following in this respect, as in so many others, the procedure of his father) to hypothetical considerations, in dealing with the question—Whether any cause of atmospheric circulation can be found in the economy of the Sun, 'so far as we know and can comprehend it?'

He is thus led to the inquiry, whether a transparent atmosphere extends beyond the luminous surface of the Sun. He mentions the deficiency of light at the borders of the visible disc of the Sun, remarking that this feature is so obvious that he is surprised it should ever have been controverted. He mentions in corroboration 'the extraordinary phenomenon of the rose-coloured solar clouds witnessed during the total eclipse of July 8, 1842, which must have floated in, and been sustained by, an exterior transparent atmosphere.'* And he suggests that this atmosphere must extend to some distance beyond the visible disc, because the darkening of the solar disc is not limited to the immediate neighbourhood of the edge but extends some distance within the disc.†

Assuming the existence of such an atmosphere, the rotation of the Sun would cause the outer surface of the atmosphere to take up an oblately spheroidal figure, the least axis of which would correspond with the polar axis of the Sun. 'Consequently, the equatorial portions of this envelope must be of a thickness different from that of the polar, *density for density*, so that a different obstacle must be thereby opposed to the escape of heat from the equatorial and the polar regions of the Sun. The former therefore ought,

---

* When Sir John Herschel thus wrote, less thoughtful astronomers were questioning whether these prominences belong to the Sun, whether they may not be lunar mirages, or phenomena of the Earth's atmosphere, or finally, whether they have any existence at all.

† This argument, however, is not strictly sound. The extension of the darkening over the disc indicates shallowness rather than depth. This is easily seen, if we consider that were the atmosphere indefinitely deep, the luminosity of the disc would be uniform. In the actual case, of course, the atmosphere is not uniformly dense; but still the reasoning is analogous, and the extension of the darkening over the disc implies shallowness rather than the reverse; though not the same degree of shallowness as would follow in the case of an atmosphere of uniform density.

according to this reasoning, to be habitually maintained at a different temperature from the latter.' 'The spots,' adds Sir John Herschel, 'would come, on this view of the subject, to be assimilated to those regions on the Earth's surface in which, for the moment, hurricanes and tornadoes prevail—the upper stratum being temporarily carried downwards, displacing by its impetus the two strata of luminous matter beneath (which may be conceived as forming an habitually tranquil limit between the opposite upper and under currents), the upper of course to a greater extent than the lower, and thus wholly or partially denuding the opaque surface of the Sun below. Such processes cannot be unaccompanied by vorticose motions, which, left to themselves, die away by degrees and dissipate—with this peculiarity, that their lower portions come to rest more speedily than their upper, by reason of the greater resistance below, as well as the remoteness from the point of action, which lies in a higher region, so that their centre (as seen in our water-spouts, which are nothing but small tornadoes) appears to retreat upwards. Now, this agrees perfectly with what is observed during the obliteration of the solar spots, which appear as if filled in by the collapse of their sides, the penumbra closing in upon the spot, and disappearing after it.'

With all deference to one who is as high an authority in meteorological and thermological questions (which are both involved in this matter) as in astronomical matters, I must venture to point out what appears to me a flaw in the reasoning by which an excess of heat is assigned to the solar equator. If we assume that the depth of the solar atmosphere is really greater at the solar equator, then we cannot but admit as an inevitable sequel that this atmosphere checking, as it does, (and as the theory itself requires) the radiation of heat from the solar equatorial regions to a greater extent than that from the polar regions, would cause the former regions as observed by us to appear deficient in heat-rays (and presumably in light-rays also). The deficiency must certainly become observable if the amount of heat retained in this way were adequate to produce the effects described by Sir John Herschel.

I am not endeavouring, be it understood, to negative the general conclusion to which Herschel has been led, that a difference of condition really prevails between the equatorial and polar regions to a degree sufficing to account for the spot zone as a zone of solar cyclones. Very probably this difference of temperature subsists, and almost certainly, whatever the cause may be, the spot zone is a zone of solar tornadoes. But the imagined action of a deep atmospheric layer over the solar equator seems incompatible with the observed appearance of the solar disc.

We come next to a most important series of observations directed by Schwabe, of Dessau, to the determination of the laws according to which the number and size of the solar spots may vary from time to time. Such at least was the character which Schwabe's researches eventually assumed. At first they were directed to less important objects, though from the beginning the observation of the Sun on every day when its face can be seen formed a part of Schwabe's plan.*

His observations began in the year 1826, during which many spots were visible from time to time upon the Sun's face; indeed, on the 277 days during which the weather permitted Schwabe to observe the Sun, there were but twenty-two on which no spots could be seen. In the next year there were even more spots, and only two days when none were seen. In the next two years the Sun's face was not on any day seen without spots. In 1830 only one day occurred on which no spots could be seen; in 1831, only three. But in 1832 there were no less than forty-nine days (out of 270 on which observations were made) during which no spots were seen. In 1833 there were 139 such days out of 267; in 1834, 120 out of 273; in 1835, 18 out of 244; and then followed four years during which not a single observing day occurred on which no spots were visible.

Schwabe recognised even at this early stage of the inquiry (only twelve years having as yet elapsed since he began his researches) that a certain periodicity marks the occurrence of Sun-spots, or, rather, the recurrence of years rich and poor in such phenomena. It is true that as he had noted but one full period, it might seem that he had absolutely no evidence on which to ground such a view. But in the observation of periodic variations, there are other features besides

---

* 'I went out like Saul,' he afterwards said, 'to seek my father's asses, and lo! I found a kingdom.'

the periodic return of maxima or minima to guide the experienced observer. The progression towards and from the maximum or minimum is as instructive when carefully watched as the recurrence of many maxima. The observer requires only to assure himself that this progression possesses certain characteristics, to feel assured that he is dealing with no accidental relations, but with true periodic changes. These characteristics are, chiefly, a steady (not uniform) progression from maximum to minimum, and *vice versâ*, a rapidity of change midway between maximum and minimum as compared with the rate of change near either of these stages, and a certain uniformity of character in the progression towards each stage (though the progression from minimum to maximum may not resemble in character the return towards the minimum). These characteristics Schwabe noted, and he felt satisfied that the numerical relations of the solar spots vary in a truly periodic manner.

But he felt that further observations were necessary to convince the scientific world—always slow to recognise new truths, or to accept results not rendered palpable, so to to speak, by an accumulation of evidence. He laboured on therefore for twenty more years, tracing the gradual increase and diminution of spots in frequency and in their general dimensions, until he had completed the observation of no less than three complete oscillations from maximum frequency through minimum back to maximum again. It began to be felt that Schwabe was establishing his case, and accordingly the scientific societies of Europe (somewhat tardily, it must be admitted) accepted the result which Schwabe had demonstrated,—viz., that the solar spots increase and diminish in frequency and size in a period of about ten years. The following remarks, addressed by the Rev. Mr. Main, then President of the Royal Astronomical Society, in awarding to Schwabe the Society's Gold Medal for 1857, justly express the importance of Schwabe's researches:—

'What the Council wish most emphatically to express is their admiration of the indomitable zeal and untiring energy which Herr Schwabe has displayed in bringing that research to a successful issue. Twelve years he spent to satisfy himself; six more years to satisfy, and still thirteen more to convince mankind. For thirty years never has the Sun exhibited his disc above the horizon of Dessau without being confronted by Schwabe's imperturbable telescope, and that appears to have happened on an average about 300 days a year. So that supposing he observed but once a day, he has made 9,000 observations, in the course of which he discovered 4,700 groups. This is, I believe, an instance of devoted persistence (if the word were not equivocal, I should say pertinacity) unsurpassed in the annals of astronomy. The energy of one man has revealed a phenomenon which had eluded even the suspicion of astronomers for 200 years.'

But even after these thirty-one years of labour, Schwabe continued his observations. With the exception of a few weeks during which he was unwell, he has watched the Sun as pertinaciously as ever until the present time. The results of his labours (so far as the question of the periodicity of spot-frequency is concerned) are included in the table on the next page.

The careful study of Schwabe's results (combined with such scattered records as the observations of former astronomers supply) has led Professor Wolf, of Zurich, to the conclusion that a period of 11·11 years (or the ninth part of a century) is indicated, rather than a ten-yearly period. He also recognises the existence of minor periods. 'He finds,' says Sir John Herschel, 'that a perceptibly greater degree of apparent activity prevails annually, on the average of months of September to January, than in the other months of the year; and, again, by projecting all the results in a continuous curve, he finds a series of small undulations succeeding each other at an average interval of 7·65 months, or 0·637 of a year. Now, the periodic time of Venus ($225^d$), reduced to a fraction of a year, is 0·616,—a coincidence certainly near enough to warrant some considerable suspicion of a physical connection.' A long period, estimated at about fifty-six years, has also been suspected.

The most cursory examination of the numbers in the table given in the next page, suffices to indicate the peculiarity that the progression from minimum to maximum is more rapid than the progression from maximum to minimum. In other words, if we regard the periodic changes of spot-frequency in the light of a series of waves—the maxima corresponding to the

| Year | Days of observation | Days without spots | New groups |
|---|---|---|---|
| 1826 | 277 | 22 | 118 |
| 1827 | 273 | 2 | 161 |
| 1828 | 282 | 0 | 225 |
| 1829 | 244 | 0 | 199 |
| 1830 | 217 | 1 | 190 |
| 1831 | 239 | 3 | 149 |
| 1832 | 270 | 49 | 84 |
| 1833 | 267 | 139 | 33 |
| 1834 | 273 | 120 | 51 |
| 1835 | 244 | 18 | 173 |
| 1836 | 200 | 0 | 272 |
| 1837 | 168 | 0 | 333 |
| 1838 | 202 | 0 | 282 |
| 1839 | 205 | 0 | 162 |
| 1840 | 263 | 3 | 152 |
| 1841 | 283 | 15 | 102 |
| 1842 | 307 | 64 | 68 |
| 1843 | 312 | 149 | 34 |
| 1844 | 321 | 111 | 52 |
| 1845 | 332 | 29 | 114 |
| 1846 | 314 | 1 | 157 |
| 1847 | 276 | 0 | 257 |
| 1848 | 278 | 0 | 330 |
| 1849 | 285 | 0 | 238 |
| 1850 | 308 | 2 | 186 |
| 1851 | 308 | 0 | 141 |
| 1852 | 337 | 2 | 125 |
| 1853 | 299 | 4 | 91 |
| 1854 | 334 | 65 | 67 |
| 1855 | 313 | 146 | 28 |
| 1856 | 321 | 193 | 34 |
| 1857 | 324 | 52 | 98 |
| 1858 | 335 | 0 | 202 |
| 1859 | 343 | 0 | 205 |
| 1860 | 332 | 0 | 211 |
| 1861 | 322 | 0 | 204 |
| 1862 | 317 | 3 | 160 |
| 1863 | 330 | 2 | 124 |
| 1864 | 325 | 4 | 130 |
| 1865 | 307 | 26 | 93 |
| 1866 | 349 | 76 | 45 |
| 1867 | 312 | 195 | 25 |
| 1868 | 301 | 12 | 101 |
| 1869* | 196 | 0 | 224 |

---

* This line is from the records of the Kew Observatory.

crests of the waves and the minima to the 'troughs'—the front slope of each wave is more abrupt than the rear slope.

One of the most remarkable of the results following Schwabe's noble discovery was the recognition of an association between the Sun-spot period and magnetic disturbances on the Earth.

In every part of the Earth the magnetic needle has at any given epoch a certain definite position about which, under normal conditions, it would oscillate during the day. Both as regards inclination and direction with respect to the compass-points (called the magnetic declination), this position may be regarded as determinate, at least for every fixed observatory; and, further, the intensity of the needle's directive power, that is, the energy with which, if slightly disturbed, it seeks to recover its position of rest, may also be regarded as determinate. From year to year all these magnetic elements undergo change; but with these changes we are not here concerned.* Changes of a much more

---

* I may note here my belief that the recognition of the laws affecting the secular variation of the Earth's magnetism would be simplified if attention were primarily directed to the magnetic lines determined by the inclination of the needle, instead of to those which depend on the intensity of the directive action. Fully admitting the weight of General Sabine's arguments as to the importance of the intensity, and as to its being the more essentially magnetic element, as it were (north and south, or vertical and horizontal, having no direct relation to the Earth's magnetic forces), I yet cannot but regard the inclination as affording the more trustworthy means of determining the geographical features (so to speak) of terrestrial magnetism. As the matter is of some importance, I will exhibit the reasons on which I found this opinion, leaving others to judge whether they are or are not valid. The intensity, as well as the inclination and declination (which together form one magnetic feature,—the position of rest), is doubtless affected

minute character, and the changes affecting these changes are what we have at present to deal with. Each day the needle oscillates gently about its position of rest, the oscillation corresponding to a very slight tendency on the part of that end of the needle which lies nearest to the Sun to direct itself towards his

---

by local circumstances; so that, superposed, as it were, on the normal intensity, there is an intensity depending on local conditions; and so in like manner, as to the inclination and declination. Now, the question arises, which of these two features is likely to be most *significantly* affected by these doubtless slight peculiarities? In determining, for example, the position of the magnetic equator and poles, with reference to intensity or inclination, would the error due to some small increment or decrement of intensity cause a greater or less divergence from the true position than a correspondingly small increment or decrement of inclination? The answer is obvious. The intensity-equator is the line of minimum intensity, and the intensity poles are points of maximum intensity. Near a minimum or a maximum, quantities change very slowly, and thus a very minute increment or decrement would largely shift the estimated place of minimum or maximum intensity. But a corresponding increment or decrement of inclination would have no such effect, because the inclination changes as quickly near the inclination-equator and poles as on any inclination-latitude.

The case may fairly be compared to the determination of the geographical equator and poles. Undoubtedly gravity is a far more essentially terrestrial element than the elevation of the pole star, or of the true pole of the heavens; and also, undoubtedly, the Earth's equator is the region where gravity is least, while the poles are regions where gravity is greatest. Yet these reasons are not considered sufficient to induce us to take the force of gravity as the most satisfactory indication of latitude, or to lead us to mark down as the true equator of the Earth that line along which careful observation shows that gravity has its minimum value. We know, in fact, that, however excellent the observations might be, the deduced line would differ very importantly from the true equator.

A similar objection may be urged on like grounds against the stress laid on the position of the line of no declination; since from the very nature of this line minute local peculiarities must cause enormous irregularities, and (when coupled with secular variations) the most rapid and remarkable changes of figure.

place. The daily oscillation is itself variable in a systematic manner, not only with the progress of the year, but with that of the lunar month. The daily oscillation also varies at times in a sudden and irregular manner. The needle has been exhibiting for several weeks the most perfect uniformity of oscillation. Day after day the careful observation of the needle's progress has revealed a steady swaying to and fro, such as may be seen in the masts of a stately ship at anchor on the scarce-heaving breast of ocean. Suddenly a change is noted; irregular movements become perceptible which are totally distinct from the regular periodic oscillations. A magnetic storm is in progress, and its progress does not affect only the place of observation, but widely-extended regions of the Earth; and in some well-authenticated instances, these magnetic vibrations thrill in one moment the whole frame of the Earth.

Lamont, of Munich, was the first to announce that these magnetic disturbances attain a maximum of frequency in periods of about ten years. This was in 1850. Two years later General Sabine and (independently) Professors Wolf and Gautier noted the coincidence of this period with that of the solar spots. Of course, mere coincidence in duration was not the sole circumstance on which they based this conclusion. It was the coincidence of maximum of spot-frequency with maximum of magnetic disturbance, and of minimum with minimum, which enabled them to assert the true correspondence of the two periods.

Wolf subsequently proved that the period of magnetic disturbances has the length of 11·11 years, which he had assigned as the correct value of the solar spot period.

A relation so strange might well excite grave doubts. Coincidences have so often misled men of science, and indeed it is so certain in the very nature of things that misleading coincidences must occur, that physicists were justified in requiring further evidence. Such evidence fortunately was not wanting. Independently of the continuance of observation and the close correspondence which has been observed during the past score of years between Sun-spots and magnetic phenomena, an occurrence of a very interesting nature served in 1859 to place beyond all possibility of question the influence which solar action exerts upon the Earth's magnetism. I have so often described the occurrence in my own words that I think it well in the present instance to give the words of the two telescopists by whom it was independently observed, especially as the details of the observation have recently been called in question.

On September 1, 1859, Mr. Carrington was engaged in taking his customary observation of the forms and positions of the solar spots. 'I had secured diagrams,' he says, 'of all the groups and detached spots, and was engaged at the time in counting from a chronometer and recording the contacts of the spots with the cross-wires used in the observation, when within the area of the great north group (the size of which had previously excited general remark) two patches of intensely bright and white light broke out' in the middle of the group. 'My first impression was,' he adds, 'that by some chance a ray of light had penetrated a hole in the screen attached to the object glass, by which the general image is thrown into shade, for the brilliancy was fully equal to that of direct sunlight; but by at once interrupting the current observation, and causing the image to move by turning the right ascension handle, I saw I was an unprepared witness of a very different affair. I thereupon noted down the time by the chronometer, and seeing the outbreak to be very rapidly on the increase, and being somewhat flurried by the surprise, I hastily ran to call some one to witness the exhibition with me, and on returning within 60 seconds, was mortified to find that it was already much changed and enfeebled. Very shortly afterwards the last trace was gone, and although I maintained a strict watch for nearly an hour no recurrence took place. The spots had travelled considerably from their first position, and vanished as two rapidly fading dots of white light. The instant of the first outburst was not fifteen seconds different from 11h. 18m. Greenwich mean time, and 11h. 23m. was taken for the time of disappearance. In this interval of five minutes, the two spots traversed a space of about 35,000 miles. It was impossible on first witnessing an appearance so similar to a sudden conflagration, not to expect a considerable result in the way of alteration of the details of the group in which it occurred; and I was certainly surprised, on referring to the sketch which I had carefully and satisfactorily (and I may add, fortunately) finished before the occurrence, at finding myself unable to recognise any change whatever as having taken place. The impression left upon me is that the phenomenon took place at an elevation considerably above the general surface of the Sun, and accordingly altogether above and over the great group in which it was seen projected. Both in figure and position the patches of light seemed entirely independent of the configuration of the great spot, and of its parts, whether nucleus or umbra.'

Mr. Hodgson's account (written before he had 'exchanged any information' with Mr. Carrington) runs as follows:—'While observing a group of spots on September 1, I was suddenly surprised at the appearance of a very brilliant star of light, much brighter than the Sun's surface, most dazzling to the protected eye, illuminating the upper edges of the adjacent spots and streaks, not unlike in effect the edging of the clouds at sunset; the rays extended in all directions; and the centre might be compared to the dazzling brilliancy of the bright star Alpha Lyræ when seen in a large telescope with low power. It lasted for some five minutes, and disappeared instantaneously, about 11h. 25m. A.M.' It seems probable that whereas two spots were seen by Mr. Carrington, who observed the solar image projected on a screen, these were blended, owing to their extreme brilliancy, into the semblance of a single spot when observed in the telescope itself by Mr. Hodgson.

At the moment when the Sun was thus disturbed, the magnetic instruments at Kew exhibited those signs which indicate the occurrence of a magnetic storm. 'It was found,' says Dr. Balfour Stewart, 'that a magnetic disturbance had broken out at the very moment when this singular appearance had been observed.' But this was not all. A magnetic storm never rages without accompanying signs of disturbance. Auroras in both hemispheres, and the interruption of magnetic communication all over the Earth, are the signs of a great magnetic storm. Both these evidences of great disturbance were afforded during the hours which followed the solar outbreak witnessed by Carrington and Hodgson. Vivid auroras were seen not only in both hemispheres, but in latitudes where auroras are very seldom witnessed. Even in Cuba the sky was illuminated by the auroral radiance. Strong earth-currents were observed along telegraphic lines, and these currents continually changed their direction, while all the time the magnetic needles in fixed observatories were kept markedly on one side of their normal position. 'By degrees,' says Sir John Herschel,* 'accounts began to pour in of great auroras seen

---

* I have been careful, it will be noticed, to quote the words of many authorities in dealing with this matter. The account I gave in my *Other Worlds than Ours* has been cavilled at by the Cavilian Professor of Astronomy as if it had been based on imagination; so that I have thought it well to re-examine the records and statements from which that account was really taken (though by the aid of memory instead of

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 September 2, 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 telegraphic wires struck work. They had too many private messages of their own to convey. At Washington and Philadelphia, in America, the telegraphic signal-men 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 writes down the message upon chemically-prepared paper).'

It is demonstrated then that some association exists between the disturbance of the solar photosphere and the phenomena of terrestrial magnetism. What the nature of the association may be is not so clearly apparent. We have seen that the solar spot period has been supposed to be associated by Wolf with the motions of the planets, and we shall presently see that the phenomena of spots,—as their change of form and of size, the regions in which they appear, and so on,—have been

---

transcription). Although several years had elapsed since I read those statements, I do not find that my account required correction in a single essential particular.

conceived to depend on the planetary motions. If these views be correct, we should have to account for laws of association between the planetary motions, terrestrial magnetism, and solar spots. But which of these three orders of phenomena should be regarded as the cause of the others; or whether the association be of the nature of cause and effect, and not rather due to some as yet unknown common cause of the two latter classes of phenomena; or whether, finally, the planets' motions, without being the direct cause of the other phenomena, yet indirectly brings them about, remains to be determined.*

Let us next turn to Mr. Carrington's researches, intimately associated with Schwabe's, and among the most important of all the contributions which have

---

* I would point out, however, that the consideration of the association here discussed led General Sabine, Professor Challis, and Dr. Stewart to inquire whether the coloured prominences of the Sun may not be due to solar auroras. Since we now know that the prominences are not of this nature, may we not transfer the suggestion to the solar corona and the zodiacal light? May not these be the true solar auroras? The latter, at any rate, gives the same spectrum as the aurora, and the former gives a spectrum closely analogous. If, as General Sabine suggests, an auroral outburst in the Sun 'may perhaps be responded to simultaneously by the different planets, so that the whole solar system would seem to thrill almost like a living being under the magnetic excitement,' it seems at least probable that the solar auroras would extend to distances enormously exceeding those assigned them when the prominences were taken to be such auroras. And as we have abundant evidence that terrestrial auroras occur where the atmosphere has an inconceivable rarity, it would seem possible that under the enormous action of the Sun, even the quasi-vacuum of the interplanetary spaces might be traversed by electrical discharges. The meteoric systems occupying more or less densely the whole of these spaces, would seem to afford the requisite channel for this electric action.

been made in recent times to our acquaintance with solar physics.

The object to which Mr. Carrington specially devoted his energies was the endeavour to detect regularity in the distribution of the spots, the determination of the true period of rotation of the body of the Sun, and the detection of 'systematic movements or currents of the surface, if such existed in a definable manner.' In these researches he perfectly succeeded.

It would be desirable if space permitted to describe Carrington's method of observation, and the exact and systematic processes by which he deduced his results. I must be content, however, so far as these points are concerned, to refer the curious reader to Carrington's voluminous and masterly treatise, in which also will be found an interesting account of the results obtained by former observers. Here I have room only for the conclusions to which he was led by his series of observations, which commenced in the year 1853 and ended in the year 1861.

He discovered, in the first place, that the discrepancies between the values formerly deduced for the Sun's rotation, arise from real differences in the velocities with which the spots move in different solar latitudes. Near the equator a spot moves at a rate indicating a more rapid rotation than in higher latitudes. Further, even among spots in the same latitude, proper motions may be recognised. These latter motions are to be regarded, however, as abnormal, and simply rendering unreliable such observations as are made on but a few spots. The peculiarities affecting the motions of spots in different latitudes have been reduced by Mr. Carrington into a formula.* The following table gives the observed rates of rotation for different latitudes (the formula being based on these values):—

| | Sun's rotation-period. | | | Rotation per day. |
|---|---|---|---|---|
| *deg.* | *d.* | *h.* | *m.* | *m.* |
| 50 N. Lat. | 27 | 10 | 41 | 787 |
| 30 „ | 26 | 9 | 46 | 824 |
| 20 „ | 25 | 17 | 8 | 840 |
| 15 „ | 25 | 9 | 10 | 851 |
| 10 „ | 25 | 3 | 29 | 859 |
| 5 „ | 25 | 0 | 42 | 863 |
| 0 Equator | 24 | 2 | 11 | 867 |
| 5 S. Lat.† | 24 | 23 | 18 | 865 |
| 10 „ | 25 | 5 | 35 | 856 |
| 15 „ | 25 | 13 | 31 | 845 |
| 20 „ | 25 | 17 | 52 | 829 |
| 30 „ | 26 | 12 | 50 | 814 |
| 45 „ | 28 | 11 | 0 | 759 |

---

[*] This formula is as follows:—Let $\xi$ be the angle through which a part of the Sun in latitude $\lambda$ rotates in one day. Then

$$\xi = 14^\circ\,25' - 165' \sin^{\frac{7}{4}} \lambda.$$

Spörer gives the formula

$$\xi = 16^\circ{\cdot}8475 - 3^\circ{\cdot}3812\,(\sin \lambda + 41^\circ\,13').$$

[†] It is remarkable that in all southern latitudes the observed daily mean rotation is less than in the corresponding northern latitudes. It is doubtful whether we have in this relation any indication of the true cause of the observed variations in the rate of rotation, or merely a peculiarity which would have disappeared in a longer series of observations. In favour of the former view, we have the consideration that the determination for each southern as well as for each northern latitude was independently effected, so that the coincidence of the results indicates the existence of some real cause. If Sir John Herschel is right in considering that the more rapid rotation near the solar equator implies the action of external matter in maintaining the rotation of the photosphere, it may be suggested that the northern surface of the Sun being directed somewhat more fully towards that region whither the Sun's

I shall have occasion to dwell further on upon the significance of the varying rotation-period deduced for different solar latitudes. This result of Mr. Carrington's labours cannot but be regarded as one of the most important contributions recently made to our knowledge of solar physics.

Spörer has re-examined the whole subject, taking into account later observations, and in particular those which have been made by Fr. Secchi. The following table includes the general elements of the solar rotation as deduced by Carrington and Spörer, and reduced by Secchi to the year 1869:—

| Elements | Carrington | Spörer |
|---|---|---|
| Longitude of node of solar equator | 73° 57′ | 74° 37′ |
| Inclination of solar equator | 7 15 | 6 57 |
| Diurnal rotation | 14 18 | 14 26·64 |
| Rotation-period | $25^{\mathrm{d}}{\cdot}\,38$ | $25^{\mathrm{d}}{\cdot}\,2340$ |

(No weight can be attached to the last two decimal figures in Spörer's value of the diurnal rotation and the rotation period.) In fig. 46 the varying presentation of the Sun towards the Earth on account of the inclination of his equator to the Earth's orbit is exhibited as

---

proper motion is carrying him (see the concluding chapter of this work, and the illustrative cuts), would probably be more exposed to the influence of this external action—'the frictional impulse of circulating planetary matter in process of subsidence into, and absorption by, the central body'—much as our northern hemisphere is saluted with a larger number of meteoric missiles from June to December, when the northern hemisphere is in advance, than from December to June, when this hemisphere is towards the more sheltered side of the Earth.

exactly as possible. On or about December 6,* the Earth crosses the plane of the Sun's equator (passing southwards), and then the Sun is presented as at I, fig. 46. Three months later, the Earth reaches her greatest distance south of the solar equator, so that on or about March 6 the Sun is presented as at II. On about June 5 the Earth again crosses the plane of the solar equator, this time passing northwards, and the Sun is presented as at III. Lastly, on about September 5, the Earth reaches her greatest distance north of the solar equator, and the Sun is presented as at IV.

The observations of Schwabe and Carrington have been continued, under improved conditions, by De La Rue, Stewart, and Loewy. The powers of photography, under the able superintendence of Mr. De La Rue, have been applied to secure records of the aspect of the solar disc on every clear day. But independently ot the valuable series of records thus obtained, the three physicists above named have undertaken a careful scrutiny, not only of the solar photographs taken at Cranford and afterwards at Kew, but of the observations made by former students of the solar surface. Accumulating a vast mass of records, they have applied processes of statistical research to educe any information

---

* The date for any year can always be determined from the almanac. It is only necessary to note when the Sun's longitude is 180° + the longitude of node of Sun's equator (say 180° + 74°, or 254°). In like manner, the Earth is again in the plane of the solar equator when the Sun's longitude is about 74°. The solar longitude is given for each day in *Hannay's Almanac*, in the last column of the first page for each month.

respecting the Sun, which, though in reality contained in observations already made, may not lie at or even near the surface.*

In the three papers by De La Rue, Stewart, and Loewy, entitled 'Researches on Solar Physics,' we find an extension and elaboration of the modes of research employed by Wilson, Schwabe, and Wolf. Where Wilson inquired into the behaviour of individual spots, our three allies discuss the peculiarities presented by hundreds of spots. Where Schwabe discussed the number of new groups of spots, they consider the area of the spotted portion of the Sun's surface. And, lastly, where Wolf examined the evidence which the numerical statistics respecting spots afford in favour of the theory that the planets exert an influence on the solar envelopes, De La Rue and his colleagues inquire into the behaviour of individual spots as they approach or cross the region towards which the several planets lie, they examine the general distribution of spots as respects proximity to the equator under certain circumstances of planetary position, and by discovering other like peculiarities they obtain evidence altogether distinct in character from that adduced by Professor Wolf.

Taking first the question whether the umbræ of spots are below the level of the photosphere, the inquirers examine 605 observed cases of spots having measurable

---

* It is a promising sign of progress when students of science are thus willing to discuss the labours of others as well as their own, undeterred by the fear of being called mere theorists.

penumbræ, and they find that while in seventy-five instances the penumbra is equal on both sides (referring to Dr. Wilson's mode of dealing with the question), in 456 instances the penumbra was widest on the side nearest the limb, while in seventy-four the penumbra was narrowest on that side. Hence the percentage of favourable instances is 75·37; of unfavourable cases, 12·23; and of neutral cases, 12·40.* This seems to place the existence of a real depression beyond question; while at the same time it demonstrates the truth of what Carrington has said respecting Wilson's observations,—that 'there is more variety in the appearances than Wilson confesses to, and there are marked departures from his description of form, which is rather one specific type out of several which might be adduced, and will be familiar to every one when photography has furnished us with forms on which all, whether observers or not, may rely.' I would invite the special attention of the reader at this point to Plate I., where Mr. Browning has delineated a case in which two spots close by each other exhibit altogether different characteristics, one agreeing with Dr. Wilson's description, the other presenting an opposite peculiarity. I was much struck with the great variety observable in this respect, when I was drawing the picture which

---

* In the *Researches* the neutral instances are not taken into account, and thus the percentage of favourable cases becomes 86·04, and that of unfavourable cases 13·96. But the rejection of neutral cases is not in accordance with the accepted rules for dealing with such matters. I can discover no reason for making an exception in this particular case.

illustrates this chapter (Plate II.), though the low power I employed did not permit me to see the minuter details of the spots.*

Next, investigating the relative position of spots and their accompanying faculæ, our inquirers find that out of 1,137 spots, 584 have their faculæ either wholly or mostly on the left—that is, behind them, as respects the motion of solar rotation, 508 have the faculæ nearly equally on both sides, while only forty-five have their faculæ mostly on the right. 'The most natural explanation of this would be,' they say, 'that the faculæ of a spot have been uplifted from the very area occupied by the spot, and have fallen behind to the left from being thrown up into a region of greater velocity of rotation.'

Thirdly, they attack the following question:—'Is a spot, including both umbra and penumbra, a phenomenon which takes place beneath the level of the Sun's photosphere or above it?' They note in evidence on this matter, that there are many instances in which

---

* The telescope I employed was a very small one, about $2\frac{1}{8}$ inches in aperture. I used an ordinary erecting eye-piece, having a power of about 26. Thus seen, the Sun appeared in the middle of a large field, and I could scarcely have believed that I should have been able to recognise the features I had seen with larger telescopes and higher powers. I had never before examined the Sun with so low a power (for four years before the day on which the design for Plate II. was drawn I had had few opportunities for observation), and it was with a sense of considerable pleasure that I found the familiar features coming clearly into view as I scrutinised the tiny image more and more searchingly. I found myself able to comprehend better than ever before how Galileo with his small telescope and low power (30) had been able to detect so many features of the Sun's surface.

'a bridge of luminous matter of the same apparent luminosity as the surrounding photosphere, and unaccompanied by any penumbra, appears to cross over the umbra or centre of a spot.' Detached portions of luminous matter are also seen at times to move across a spot without producing any permanent alteration. 'On these accounts,' say the inquirers, 'we are disposed to think that a spot, including both umbra and penumbra, is a phenomenon which takes place beneath the level of the brighter part of the Sun's photosphere.' Summing up the results of this portion of their researches, they express their belief that,—

1. The umbra of a spot is nearer the Sun's centre than the penumbra, or, in other words, it is at a lower level.

2. Solar faculæ, and probably also the whole photosphere, consist of solid or liquid bodies of greater or less magnitude, either slowly sinking or suspended in equilibrium in a gaseous medium.

3. A spot including both umbra and penumbra is a phenomenon which takes place beneath the level of the Sun's photosphere.

As respects the sequent series of researches by which Messrs. De La Rue, Stewart, and Loewy have endeavoured to estimate the influence of the planets upon the solar spots, it is to be remarked that the evidence adduced seems as yet not wholly decisive. They believe that it has been rendered probable that Venus exerts a special influence on the solar spots, and that the conjunctions of the planets also affect importantly the condition of the solar photosphere. There is room, in my judgment, for some doubt as to the justice of either conclusion. It should not be forgotten that the planetary system presents so many periodic relations as to render it almost certain that any observed periodic changes in the Sun's condition may be associated statistically with some period of planetary motion—sidereal, synodical, nodical, or otherwise.* There is a remark towards the close of Carrington's volume on the solar spots, which bears very significantly on this subject. After exhibiting the relation between the phenomena of the solar spots (as tabulated by Professor Wolf) and Jupiter's variations of distance, he says, that 'from the year 1770, there is a very fair agreement between maxima of frequency and maxima of Jupiter's radius vector, and between minima and minima;' . . . but 'in the two periods which precede that date there appears to be a total disagreement.' 'It is important,' he then adds, 'to see before us an instance in which eight consecutive cases of general, but imperfect agreement, between the variations of two physical phenomena, are shown to be insufficient to base any conclusion upon, at the same time that they powerfully stimulate further inquiry with the view of ascertaining whether the discrepancy may admit of future explanation.'

---

* I was much struck with this fact when perusing a valuable contribution by Professor Kirkwood to the subject of planetary influences on solar phenomena. His long experience in dealing with such matters enables him to exhibit relation after relation, each showing a remarkably close agreement, as respects period, with periodic solar phenomena.

I would by no means be understood to imply, however, that I regard the conclusions of Messrs. De La Rue, Stewart, and Loewy, respecting the influences exerted by the planets on solar phenomena, as inadmissible. On the contrary, I regard them as, on the whole, the most probable yet advanced. Based as they are on observed facts and on statistical relations, they are worthy of the most attentive consideration. They do not seem to me, however, to be by any means demonstrated, nor are they so regarded (it is proper to add) by their propounders.

It remains only that I should indicate in a general, and necessarily brief manner, those features of the Sun's surface which recent observations have revealed to us.

Let me begin with the famous 'willow-leaves.' It was announced by Mr. Nasmyth in 1862 that the *pores* seen in the solar photosphere are 'polygonal insterstices' (I quote Sir John Herschel's account) 'between certain luminous objects of an exceedingly definite shape and general uniformity of size, whose form (at least as seen in projection in the central portions of the disc) is that of the oblong leaves of a willow-tree. These cover the whole disc of the Sun (except in the space occupied by the spots) in countless millions, and lie crossing each other in every imaginable direction.' The appearance of the Sun, according to this view, is exhibited in figs. 47 and 48, both of which are from drawings by Mr. Nasmyth.

This announcement led to a controversy which still remains undecided. Mr. Dawes asserted his belief that no such interlacing as Mr. Nasmyth described is ever observable among the small bright spots which lie scattered over the general ground of the photosphere; that these spots can in no sense be said to resemble willow-leaves, though they present every variety of figure and size; and, lastly, that they had been long known to solar observers, and are, in fact, no other than the *nodules* of Sir William Herschel. 'The only situation,' he wrote, 'in which I have usually noticed them to assume anything like the shape of willow-leaves, is in the immediate vicinity of considerable spots, on their penumbræ, and frequently projecting beyond it for a small distance on to the *umbra*,—an appearance with respect to which, in April 1852, I

FIG. 47.

Sun-spot observed by Nasmyth, showing three bridges composed of solar willow-leaves. (*Nasmyth.*)

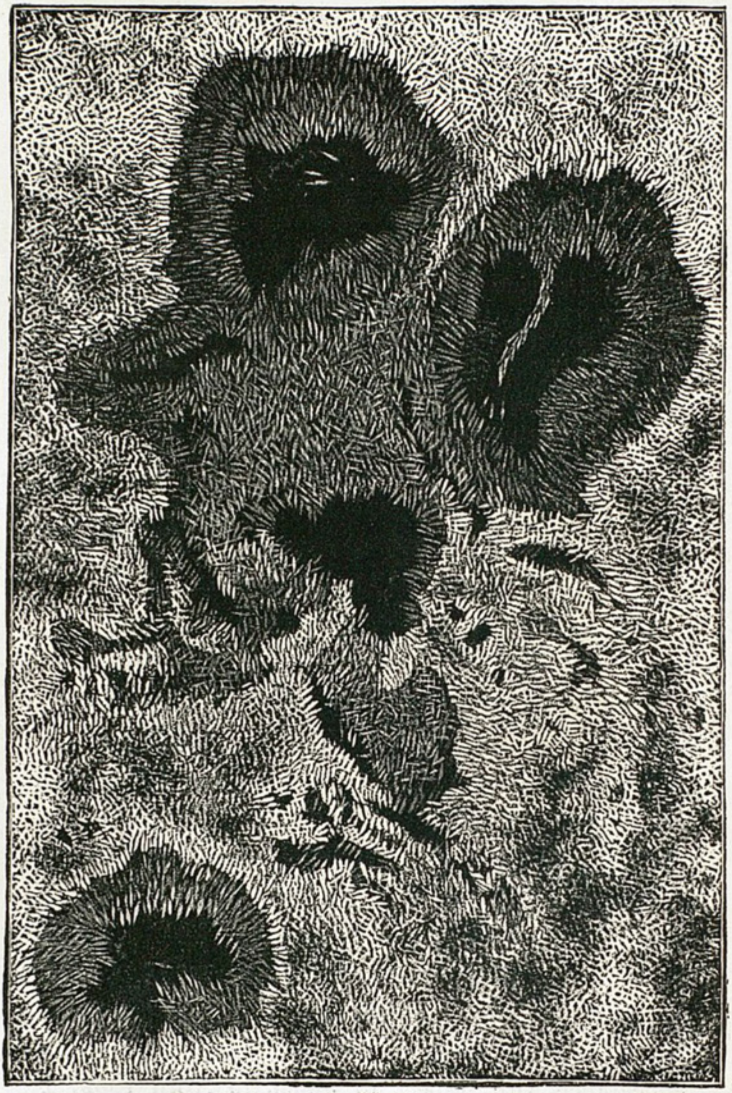
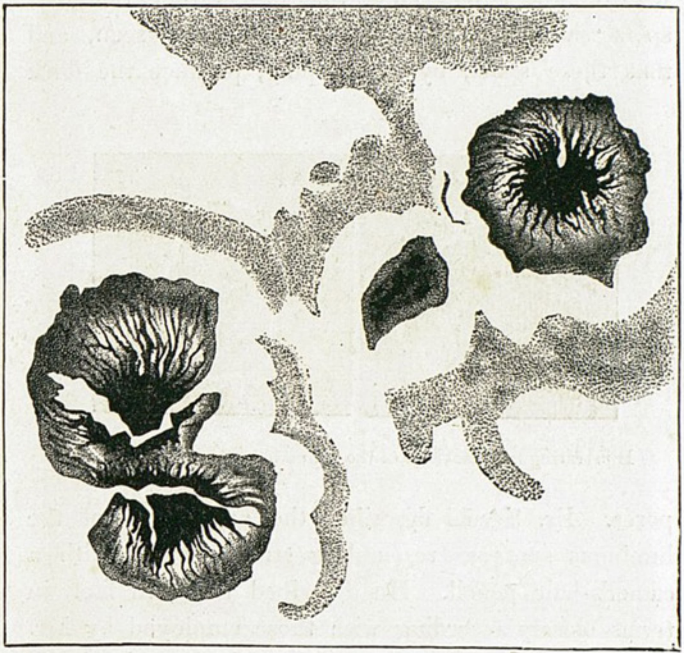


FIG. 48.—A large spot-group, showing the solar willow-leaves.

used the following expressions:—"The interior edge of the penumbra frequently appears extremely jagged; the bright ridges on its surface, which are directed nearly towards the centre of the spot, being seen projected to irregular distances on to the cloudy stratum (or *umbra*), and looking much like a piece of coarse thatching with straw, the edge of which has been left untrimmed." After nearly twelve years of careful observation of the same phenomena, I do not think

FIG. 49.

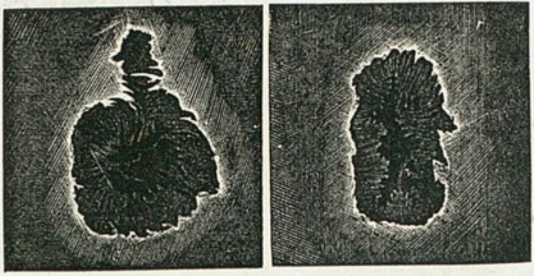


Sun-spots, showing penumbral rills. (*Secchi.*)

that I could improve upon this description.' The appearance here described will be recognised in Mr. Browning's drawing (Plate I.); it is also observable in figs. 49 and 50.

Other astronomers now joined in the discussion. Messrs. Stone and Dunkin, of Greenwich, asserted that with the fine equatorial of that observatory, luminous spots shaped like rice-grains could be seen, and that these spots, by overlapping, produce the dark pores. Fr. Secchi described the appearance of the luminous spots as resembling strokes made with a camel's-hair pencil. He described them, in fact, in terms closely according with those employed by Mr. Dawes. Dr. Huggins speaks of these luminous objects (or granules, as he calls them) as certainly not interlacing on the general surface of the photosphere. Fig. 51 represents a view of a portion of the Sun's disc as seen by him. It will be noticed that if this view is held at a considerable distance from the eye the

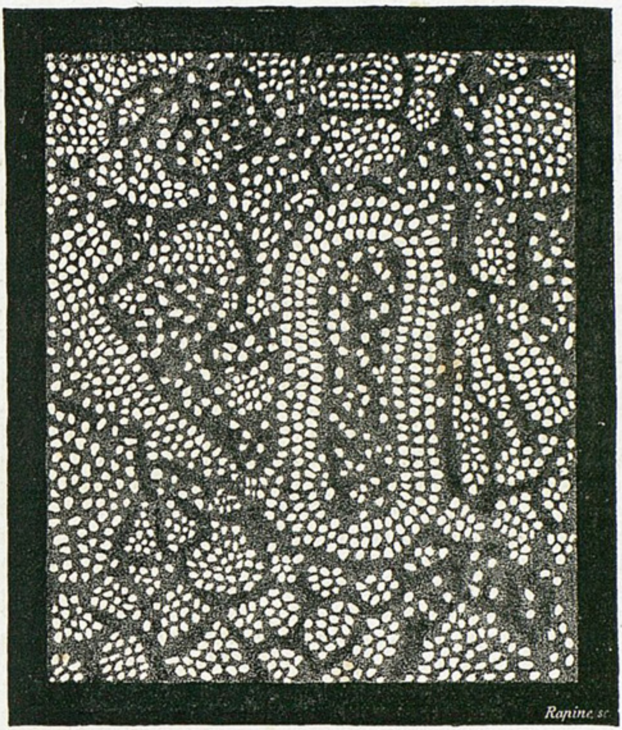
Fig. 50.



Illustrating the markings of the penumbræ of spots. (*Capocci.*)

general aspect corresponds closely with the mottled appearance presented by the Sun in telescopes of moderate power.*

FIG. 51.



A portion of the solar surface, showing granules. (*Huggins.*)

Other observers agree with Mr. Nasmyth, who adheres, I believe, to his original opinion. As regards

---

* In drawing Plate II. with the low power and small aperture described at p. 215, I was struck with the perfect distinctness of this mottling all over the disc.

the assertion that Sir William Herschel had long since recognised these objects, it is of course to be rejected or accepted according as Mr. Nasmyth's opinion as to the real nature of the objects is confirmed or confuted. If there really is the interlacing he speaks of on the general surface of the Sun, then undoubtedly he is the discoverer of the phenomenon, for Sir William Herschel nowhere speaks of any such appearance (to my recollection, at least). Sir John Herschel, too, deals with Nasmyth's observation as undoubtedly new and also as of unmistakable interest and importance.*

---

* Let me here quote Sir John Herschel's account of Nasmyth's willow-leaf theory, partly on account of its liveliness and graphic clearness, partly for the sake of his reflections on the subject:—' These leaves, or scales, are not arranged,' he says, ' in any order (as those of a butterfly's wings 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 one to another; and the way in which they lie across and athwart each other (except where 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 develope, 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 develope at once heat, and light, and electricity. These wonderful objects have been

Before leaving the subject of the willow-leaves, I must quote one of the latest observations bearing on this subject. On April 13, 1869, Secchi noticed that over the whole of a large spot and its neighbourhood, there were multitudes of *leaves*, and that a bridge across the spot was formed of elongated leaves. The leaves in the neighbourhood of the spot were oval, the greater diameter about three times the less—thus,  'like the leaves of the olive and certain *salices*.' He says, 'What are these things? There are veils of the most intricate structure. The spot has a third nucleus in which are the leaves seen in the drawing (fig. 52), all arranged in a radiating manner, precisely like a crystallization of sal-ammoniac seen by means of the solar microscope—attached to a stalk (*lingua*). It is clear that the rest of the nucleus is due to the rarefaction of

FIG. 52.

seen by others as well as by Mr. Nasmyth, so that there is no room to doubt of their reality. To be seen at all, however, even with the highest magnifying powers our telescopes will bear when applied to the Sun, they can hardly be less than a thousand miles in length, and two or three hundred in breadth.' So that if these things are solar inhabitants whose fiery constitution enables them to illuminate, warm, and electricise the whole solar system, they are not wanting in that evidence of might which gigantic size affords. Truly, Milton's picture of him who on the fires of Hell 'lay floating many a rood,' seems tame and commonplace compared with Herschel's conception of these floating monsters, the least covering a greater space than the British Islands.

such leaves. There cannot be a shadow of a doubt on this matter.'

On April 14, the spot in which Secchi had observed these singular objects presented the appearance depicted in fig. 53. 'It was in a marvellous condition,' says Secchi, 'full of bridges, arcs, stalks, and leaves, like the great spot of 1866.'*

Fig. 53.

Sun-spot observed on April 14, 1869, by Secchi.

---

* *Memoria III. sugli spettri prismatici de' corpi celesti.* Secchi assigns the following dimensions to this remarkable group:—

| | |
|---|---|
| Total diameter in length | 2′ 37″·5 |
| Diameter of the two principal nuclei | 0′ 23″·78 |
| Breadth of the spot | 0′ 42″·03 |
| Breadth of the nucleus | 0′ 23″·70 |
| Breadth of the bridge | 0′ 1″·50 |

The discovery by Dawes that within the umbra, or what was formerly called the nucleus of every fully-developed spot, there exists a darker region, or (so far as telescopic research has yet gone) a true nucleus, not only opens a new field for speculation, but renders the interpretation of the phenomena before recognised altogether more difficult. By using a solar eye-piece of his own invention, in which the field of view was so contracted as to exclude even the light from the penumbræ of large spots, he detected in the stratum which had before been regarded as black, 'a mottled appearance—the degree of darkness being by no means uniform, and suggesting the idea that the surface is far from level, the lighter parts being probably the most elevated, and feebly reflecting the light received from the self-luminous strata above it. . . In all spots which are tolerably symmetrical, and large enough to admit of accurate scrutiny, this *umbra* will be found to be perforated near its centre by a *perfectly black hole*, which is to be regarded as the true nucleus.'

From the researches of Fr. Secchi it would appear as though the umbral portion of large spots were formed of luminous matter which is undergoing a continual process of dissipation towards its interior edge. He compares the process to the gradual dissipation of cumulus clouds under the heat of a summer Sun, and regards the umbra as of the nature of a veil of clouds; the nucleus as a region where an intenser heat has caused these clouds to melt away. Within spots such as these he has recognised the presence of coloured matter, which he compares to the coloured envelope whence the prominences spring; and it was partly on account of his having noted such appearances that he pronounced, so far back as 1860, his conviction that an envelope of this coloured matter—the chromosphere of the next chapter—surrounds the whole globe of the Sun. These appearances have been noticed also by other observers. Mr. Lockyer has recognised the gradual disappearance of portions of luminous matter (as if by subsidence in some semi-transparent medium) in the umbræ of large spots; while the presence of various tints of red within the spots has been noticed also by Schwabe, Capocci, Schmidt, and other observers.

The processes of formation, enlargement, and disappearance of spots are well worthy of study; and although no regular law has been detected in their succession, we can yet recognise certain distinctive features ordinarily belonging to each stage of development. The formation of a spot is usually preceded by the appearance of faculæ. Then a dark point makes its appearance which increases in size, the penumbral fringe being presently recognised around it, and the distinction between the umbra and the penumbra being well defined. The same clearness of definition continues ordinarily until and after the spot has reached its greatest development. But when the spot is about to diminish, there is a change in this respect. The edges seem less sharp, and an appearance is presented as though there floated over them a luminous cloud-veil, brighter in some places than in others, and not unfrequently attaining a brightness which seems to exceed even that of the faculæ. At certain parts of the spot's circumference, this bright matter projects, hiding the whole width of the penumbra and forming a sort of cape or promontory, with sharply serrated edges, singularly well defined against the dark background of the umbra. It is usually in this manner that the formation of a bridge begins, two promontories on opposite sides of a spot, or even on the same side, joining their extremities, so as to form either a bridge of light across the umbra, or a curved streak having both its extremities on one side of the spot. But indeed no strict law or sequence has yet been assigned to these processes of change. In a large spot the wildest and most fantastic variations will take place, and often when the spot seems approaching the stage of disappearance, it will seem to renew its existence, as though fresh forces were at work in disturbing the region it belongs to.

Some of the processes of change which take place in large spots are very well exemplified in fig. 54, where the drawings 1 to 4 show the successive changes of appearance presented by the great spot of 1865, from October 7, when it was on the Sun's eastern limb, until October 16, when it had passed the central part of the disc. These drawings were made by the Rev. Mr. Howlett, one of our most enthusiastic solar observers, and specially skilled in the delineation of Sun spots. Were not his accuracy beyond all question, it would be

Fig. 54.

The great Sun-spot of 1865, from Oct. 7 to Oct. 16. (*Howlett.*)

worth while to denote the fact that on October 16, Chacornac made a drawing of the same spot, corresponding so closely with 4, fig. 54, that one might well suppose the two drawings were tracings from one picture.* The reader will be prepared to examine with so much the greater confidence the interesting picture

FIG. 55.

Facula near a Sun-spot. (*Chacornac.*)

of a spot with the surrounding faculæ (fig. 55), drawn by M. Chacornac. He will recognise also the close correspondence between the appearance of the region

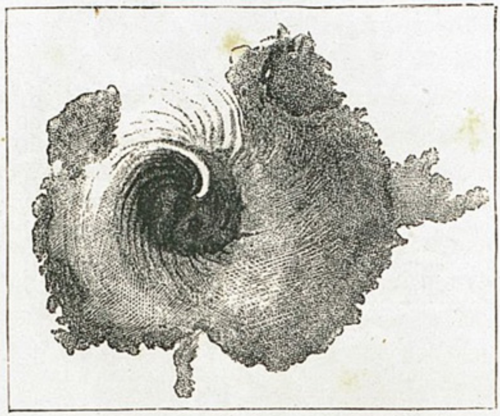
---

* Unfortunately such extreme accuracy of delineation is not ordinarily to be met with among solar observers.

around this spot and Sir William Herschel's description of the faculæ (p. 180).

We owe to Mr. Dawes the detection in certain spots of a rotatory motion, as though these regions were the scene of some tremendous solar tornado. A spot of enormous dimensions was observed by him to have rotated through half a complete circuit in the course of about six days. Other spots have exhibited an even more rapid motion, and the spectroscopic observations made by Mr. Lockyer on parts of the Sun near the limb (where such cyclonic motions would necessarily involve a rapid motion towards and from the eye) seem to place beyond question the existence of solar tornadoes having a velocity of 40 or 50, in some cases even 120 miles per second. So that we

FIG. 56.



A spot presenting the appearance of cyclonic motion. (*Secchi.*)

need no longer regard (with Spörer) the whirling appearance noticed in the accompanying drawing by Fr. Secchi (fig. 56) as due to optical illusion.

The immense dimensions of some spots well deserve thoughtful consideration. When we remember that the least spot which could be perceived with the most powerful telescope must have an area of at least 50,000 miles, it will be understood how enormous these spots must be which have been distinctly visible to the unaided eye.* But we have trustworthy measurements to refer to in this matter. Pastorff, in 1828, measured a spot whose umbra had an extent four times greater than the Earth's surface. In August 1859, a spot was measured by Newall which had a diameter of 58,000 miles—that is, exceeding more than seven times the diameter of our Earth. But spots of even greater dimensions have been observed. In June 1843, a spot was visible, which, according to Schwabe's measurements, had a length of no less than 74,816 miles. On March 15, 1858, observers of the great eclipse had the good fortune to witness the passage of the Moon over a spot which had a breadth of 107,520 miles. It was in the same year that the largest spot of any whose records have been handed down to us, was visible upon the solar disc. It had a breadth of more than 143,500 miles; so that across it no less than eighteen globes as large as our Earth might have been placed side by side. At a very moderate

---

* Of the groups shown in Plate II. three were visible to the naked eye, the largest of them (near the centre) being quite conspicuous.

computation of the depth of this solar cavity, it may be assumed that the mass of 100 earths such as ours would barely have sufficed to fill it to the level of the solar photosphere.

During the past two years many spots and groups of enormous extent have been noticed. Those shown in Plate II. may be cited as instances; but others fully as large have lately been observed.

The rapidity with which some spots have changed in figure, or even wholly disappeared, would be wholly incredible were it not that astronomers of the highest repute for accuracy have supplied the records of such changes. Dr. Wollaston says:—'I once saw with a 12-inch reflector a spot which burst in pieces as I was looking at it. I could not expect such an event, and therefore cannot be certain of the exact particulars; but the appearance, as it struck me at the time, was like that of a piece of ice when dashed on a frozen pond, which breaks in pieces and slides on the surface in various directions.'* Biela also notes that spots disappear sometimes almost in a single moment. The converse of such a change has been witnessed by Krone, who observed a spot of no inconsiderable dimensions which sprang into existence in less than a minute of time. On one occasion a momentary distraction caused Sir William Herschel to turn away his

---

* Of course this description refers only to the appearance which the spots ordinarily present, of being real bodies rather than openings. What Wollaston has described as the breaking up of a spot into pieces, must in reality be looked upon in all probability as the sudden change of a single whirlstorm into a number of smaller ones.

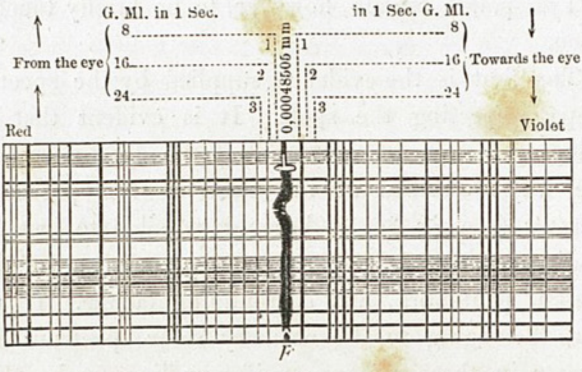
eyes from a group of spots he was observing. When he looked again the group had vanished!

In this place I shall not enter into the discussion of the nature of the spots—a matter, indeed, on which at present it is very difficult to form an opinion. It has been my purpose in this chapter to consider rather the evidence which has been adduced respecting the solar surface than the physical theories put forward in explanation of that evidence.

Two points remain, however, to be briefly touched upon.

The first is the evidence supplied by the spectroscope respecting the spots. It is evident that by bringing a spot under the slit of the spectroscope in the way described at the close of the preceding chapter (see fig. 35) it becomes possible to institute a direct comparison between the spectrum of the umbra, penumbra, and surrounding faculæ. If any lines belonging to the ordinary solar spectrum disappear in these regions, or if new lines make their appearance, we can at once become cognisant of the fact, because we see the spectra of these regions simultaneously. In like manner we can determine whether any change takes place in the character and appearance of any solar line—for instance, whether it is wider or narrower in the spectra of certain regions, or whether it changes into a bright line. Now, fig. 57 will illustrate the peculiarities which make their appearance when a spot is brought in this way under examination. Here the length of the spectrum (only a small portion of which is shown in the figure), is horizontal, so that the vertical lines are the dark lines of the spectrum. The horizontal lines indicate the regions of the spectrum corresponding to those parts of a spot where a general absorption takes place. It will be seen that where this general absorption is sufficient only to produce a degradation of brilliancy, all the lines in this part of the spectrum are visible. The F line (belonging to hydrogen) is, however, peculiarly affected across the whole region of the spot. At the upper and lower extremity we see it of its normal width, while over all the remaining breadth of the spectrum, except two small portions, it is much broader and has shaded edges. In one place it is bent; along another part of its length a narrow line of light is seen to be almost centrally placed upon it; and lastly in two places it appears bright and irregularly shaped.

Fig. 57.



Illustrating the changes in certain lines in the spectra of Sun-spots.

Now, the facts here noticed (first observed I believe by Mr. Lockyer, and confirmed by Dr. Huggins, Fr. Secchi, and Professor Young) are thus (probably) to be interpreted. Beginning with the top of the line F in fig. 57, we have first the normal black line showing that in the part of the Sun included within the uppermost part of the slit, the hydrogen is in its ordinary condition as respects temperature. It is less heated than the matter whence the main portion of the solar light is radiated, and so absorbs that portion of the light which it has itself the power of radiating. Next we come to a bright hydrogen line of the normal width on a shaded background. The corresponding part of the Sun (that is the part next below the former within the slit) is, then, either somewhat reduced in temperature, or else partially covered by generally absorbing matter, over which there is a layer of hydrogen at the normal pressure, but more heated than the radiating region below. Hence in this place the hydrogen radiates more light than it absorbs, and the line F is rendered relatively bright. Next is a region where the hydrogen line is still bright but very much wider. Over the corresponding portion of the Sun, therefore, the hydrogen not only exists at a higher temperature but at a greater pressure. Then we come to the widened dark line, indicating that over the corresponding portion of the Sun there is hydrogen at a relatively low temperature and at an abnormally high pressure. The bend towards the red end of the spectrum indicates that the corresponding portion of the hydrogen envelope is moving from the eye,* or, in other words, that there is in this part of the Sun a downrush of hydrogen. Where we see a relatively bright line superposed on the relatively dark one, we learn that above the compressed hydrogen at a relatively low temperature there is a layer (or tongue, or prominence) of more heated hydrogen. While, lastly, where we see the pointed dark line close to the bottom of the figure, we learn that above hydrogen as heated as the general radiating substance of the Sun, there is the usual layer of hydrogen at lower temperature, very shallow where the line is pointed, but deepening within a short distance to its normal condition.

Along a narrow strip, then, crossing the width of a solar spot, all these varieties of condition are thus recognisable. Nor is hydrogen the only element whose lines exhibit such peculiarities. The lines of sodium, magnesium, barium, and other elements, have been observed to exhibit similar indications of violent action, rapid motion, and remarkable changes of pressure. But perhaps the most striking of all the phenomena revealed by the spectroscope is the occurrence, in the spectra of large spots, of lines and bands corresponding to those due to the presence of aqueous vapour in our own atmosphere. Fr. Secchi not only testifies to this, but

---

* The vertical dotted lines 1, 2, 3, on either side of F indicate how far the line F should be shifted to indicate a velocity of 8, 16, and 24 geographical miles respectively from or towards the eye. The decimal figures between the vertical lines numbered 1, 1, indicate the length of the light-waves (in parts of a millimetre), corresponding to the part of the spectrum where the line F is.

he describes experiments by which he convinced himself that these lines really belong to the spots, and not to our own atmosphere. He found that these 'water-lines' were not visible when the instrument was directed in clear weather to unspotted parts of the solar disc; but that as the instrument was shifted, the approach of a spot was clearly indicated by the appearance and gradually intensifying of these lines. When the sky was covered with thin clouds he saw the same lines towards whatever part of the Sun the instrument was directed; but they always appeared strongest in the spectrum of a spot.*

The second point which I wish to notice, in conclusion, is the evidence of the polariscope respecting the general condition of the solar photosphere. I do not feel justified in giving space to an account of the principles on which polariscopic analysis depends, because, to say the truth, the polariscope has thrown but little light on the subject of solar physics. I therefore merely state that light under certain conditions of emission, reflection, and refraction, acquires a peculiar property called polarisation, by which its capability for subsequent reflection or refraction is materially modified. We have here to deal with emission; and the special law which concerns us is this, that light emitted from an incandescent solid or liquid at a

---

* The reader is referred for fuller details than there is here space for, to Dr. Schellen's work *Die Spectralanalyse*, already referred to. The English edition now preparing for publication, under the able supervision of Dr. Huggins, will be specially worthy of very careful study in all matters relating to the spectral analysis of the Sun.

very oblique angle is partially polarised in such sort that when incident on a plane at right angles with the angle of emanation, the polarised portion does not, like the rest, undergo reflection. Now, the light from near the edge of the Sun's disc shows no signs of having this particular quality. Hence Arago and others have concluded that the solar photosphere cannot be formed of incandescent solid or liquid substance, but must necessarily be gaseous. We shall have occasion further on to consider the bearing of this evidence on the views we are to form respecting the Sun's physical constitution. It is necessary to note, however, that Sir John Herschel has called in question Arago's conclusion; and, without asserting that the solar photosphere must necessarily be either solid or liquid, he has shown that the evidence of the polariscope is more than questionable, since the Sun can by no means be regarded as a smooth uniform globe. Its surface is in all probability so rough and uneven that the light received from near the edge may come for the most part from surfaces nearly at right angles to the visual line.*

Here I conclude my survey of the solar surface. I have presented but such portions of the vast mass of material really available as seemed most instructive

---

* The case may be compared with that of the Moon. If the Moon were a smooth uniform globe, she ought, when full, to seem much darker near the edge than near the centre of her disc. That she does not is due to the inequalities of her surface; and Dr. Zöllner has been able to show from the observed luminosity of the Moon at different times that the probable average inclination of the lunar mountains is about 56 degrees.

and to bear most pertinently on the views we are to form respecting the Sun's physical condition. Ten or twelve such volumes as the present would be needed to contain even an abstract of the observations which have been made on the great central luminary of our system. To the student of solar physics I cannot too earnestly recommend the careful study of all such observations as he can obtain access to; but it has been necessary for my present purpose to give a more general view of the subject. This chapter must be regarded as bearing the same relation to the ponderous volumes in which our Carringtons and Herschels, our Schwabes and Spörers and Secchis, have recorded their observations, as Plate II. bears to Plate I., or, generally, to such large-scale and elaborate views as are afforded by powerful telescopes. We must now pass on to other matters well worthy of attention—to matters that are perhaps even more interesting and instructive than the facts which astronomers have discovered in their survey of the solar surface.

## CHAPTER V.

### *THE PROMINENCES AND THE CHROMOSPHERE.*

THE coloured prominences which have recently attracted so large a share of the attention of solar physicists were first fully recognised during the total solar eclipse of 1842. It is probable, however, that they were seen more than a century before that date, though their real nature was not suspected. During the total solar eclipse of May 2, 1733, Vassenius, at Gottenburg, observed several red clouds floating, as he supposed, in the atmosphere. One of them seemed larger than the rest, and appeared to be composed of three masses placed one above the other, and completely detached from the Moon's limb. 'These spots seemed,' he writes, 'composed in each instance of three smaller parts or cloudy patches of unequal length, having a certain degree of obliquity to the Moon's periphery. Having directed the attention of my companion, who had the eyes of a lynx, to the phenomenon, I drew a sketch of its aspect. But while he, not being accustomed to the use of the telescope, was unable to find the Moon, I again, with great delight, perceived the same spot, or if you choose, rather the invariable cloud occupying its former situation in the atmosphere near the Moon's periphery.' We need not be surprised that Vassenius assigns the spots without scruple to the Moon's atmosphere, since it was thought by many in his time that the Moon has an atmosphere of appreciable extent. Yet it was unfortunate for science that the prominences (for we can scarcely doubt that the appearances seen by Vassenius were really prominences) should have been thus explained away as relatively unimportant phenomena, since otherwise observers during succeeding eclipses would probably have searched for similar objects, and we might thus have possessed a long series of observations tending to indicate the laws according to which these objects make their appearance.

For more than a century eclipse passed after eclipse, and no observer recognised these flames of coloured light, which have seemed to the observers of recent eclipses so striking and obvious. Ferrer, indeed, in 1806, and Van Swinden, in 1820, noticed faint traces of some peculiar coloured appendages; but their observations were not satisfactory, nor was any attention drawn to the subject.

During the great eclipse of 1842, however, a number of first-rate observers were distributed along the line of total obscuration. Airy, Arago, and the younger Struve; Littrow, Baily, Santini, Valz, and Biela,—a host, in fine, of the most skilful astronomers in Europe—watched the eclipse with careful scrutiny. All of them recognised with surprise the presence of rose-coloured prominences round the disc of the eclipsed Sun.

The Astronomer-Royal saw three prominences at the summit of the disc; Arago, Struve, and Schidlowski saw two near the lowest point of the disc; Schumächer, of Vienna, saw three—two below, and one above.

It will be instructive to consider the account given by the first observers of these interesting objects.

The Astronomer-Royal compared them to the inclined teeth in a circular saw, and estimated their height at about one minute of arc.

Schumächer compared the protuberances to icebergs, and the pictures which illustrate his paper represent them as much more closely resembling icebergs than any protuberances seen in recent times. We may not unfairly conclude that Schumächer's drawings are somewhat idealised.

Baily compared the prominences to Alpine peaks coloured by a setting Sun. He noticed that one was bifurcated almost to its base. M. Mauvais employs a similiar comparison. He had seen a reddish point soon after the Sun was totally obscured. 'When fifty-six seconds had passed after the commencement of totality,' he writes, 'this reddish point transformed itself into two protuberances, resembling two adjacent mountains, and well defined. Their colour was not uniform, streaks of a deeper red marking their flanks. I cannot describe them better than by comparing them to distant Alpine peaks, illuminated by the rays of the setting Sun. One minute and ten seconds from the time of total obscuration a third mountain became visible to the left of the other two. In colour it resembled the others. Beside it were some smaller peaks, all of them well defined.'

Mauvais noticed that the other two protuberances grew higher while the third was making its appearance. Near the end of the eclipse they were no less than two minutes of arc in height.

Biela, Schumächer, and others recognised a border of rose-coloured light surrounding a part of the Moon's limb at a lower level than that attained by the prominences. It is worthy of note that this phenomenon had been noticed earlier than the prominences themselves; for during the total eclipse of 1706, Captain Stannyan remarked that a blood-coloured streak of light appeared, before the Sun's limb emerged from behind the Moon. In 1715, also, Halley noticed that two or three seconds before the emersion, the Moon's limb appeared to be tinged with a dusky but strong red light, forming a long and narrow streak; and during the same eclipse, Louville saw what he describes as an arc of deep red colour along the edge of the Moon's disc. The latter astronomer was careful to assure himself that the appearance was no illusion, and to this end he brought the red arc into the middle of the telescopic field of view, when he found that the red colour remained unchanged. Don Ulloa, in 1778, and Ferrer, in 1806, had noticed a similar phenomenon.

When the various accounts of the eclipse of 1842 came before the astronomical world, several theories were propounded in explanation of the red prominences. The theory that they are mountains in the Sun was for a while in favour; but Arago pointed out that some of them were too considerably inclined to the perpendicular to be so regarded. Others supposed them to be clouds in the solar atmosphere; while others again suspected them to be enormous flames. As ordinarily happens in such cases, there were not wanting those who denied that the coloured prominences had any real existence whatever. M. Faye, for example, asserted his belief that they are purely optical illusions—'mirages, perhaps, produced near the Moon's surface.'

The eclipse of 1851 removed these doubts for the most part, though it is to be noted in passing that despite the evidence obtained then, and yet again in 1860, there were some who continued, even until the great Indian eclipse of 1868, to deny that the coloured prominences and the rose-tinted arcs seen at a lower level could really be regarded as solar appendages.

During the total eclipse of 1851 many observers of great skill made drawings of the very remarkable prominences which were visible on that occasion. These pictures exhibited a sufficiently satisfactory agreement to convince the observers that they had all witnessed the same phenomena; though the discrepancies between the pictures afford instructive evidence of the difficulty of delineating with exactness the details presented during eclipses. The following six pictures represent in order the work of the Astronomer-Royal, Mr. Dawes, Mr. Hind, Mr. Lassell, Mr. Gray, and Mr. Stephenson. Mr. Airy thus writes respecting the prominences:—

'The form of the prominences was most remarkable. That which I have marked *a* (fig. 58) reminded me of a boomerang. Its colour for at least two-thirds of its breadth—from the convexity towards the concavity—was full lake-red; the remainder was nearly white. The most brilliant part of it was the swell farthest from the Moon's limb; this was distinctly seen by myself and my friends with the naked eye. I did not measure its height; but judging generally by its proportion to the Moon's diameter, it must have been three minutes of arc. This estimation perhaps belongs to a later

Fig. 58. Fig. 59. Fig. 60.

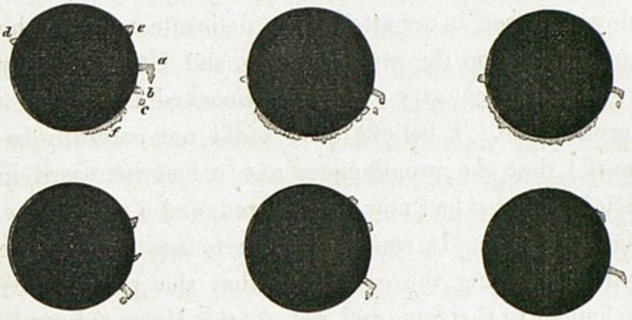


Fig. 61. Fig. 62. Fig. 63.

The coloured prominences seen during the eclipse of 1851.

period of the eclipse. The prominence *b* was a pale white semicircle based on the Moon's limb. That marked *c* was a red detached cloud or balloon of nearly circular form, separated from the Moon's limb by a space differing in no way from the rest of the corona of nearly its own breadth. That marked *d* was a small triangular, or conical, red mountain, perhaps a little white in the interior. These were the appearances seen instantly after the formation of the totality. I employed myself in an attempt to delineate roughly the appearances on the western limb, and I took a hasty view of the country; I then remarked the Moon a second time. I believe (but I did not carefully remark) that the prominences *a b c* had increased in height; but *d* had now disappeared, and a new one, *e*, had risen up. It was impossible to see this change without feeling the conviction that the prominences belonged to the Sun, and not to the Moon. I again looked round, when I saw a scene of unexpected beauty. I went to my telescope with a hope that I might be able to make the polarization observations, when I saw that the *sierra*, or rugged line of projections shown at *f*, had arisen. The *sierra* was more brilliant than the other prominences, and its colour was nearly scarlet. The other prominences had perhaps increased in height, but no additional new ones had arisen. The appearance of the *sierra* nearly in the place where I had expected the appearance of the Sun warned me not now to attempt any other physical observations. In a short time the white Sun burst forth, and the corona and every prominence vanished.'

Mr. Hind's narrative is as follows:—'On first viewing the Sun without the dark glass after the commencement of totality, three rose-coloured prominences immediately caught my eye, and others were seen a few seconds later. The largest and most remarkable of them (*a* in Mr. Airy's drawing) was straight through two-thirds of its length, but curved like a sabre near the extremity, the concave edge being towards the horizon. The edges were of a full rose-colour, the central parts paler, though still pink. Twenty seconds, or thereabouts, after the disappearance of the Sun, I estimated its length at forty-five seconds of arc, and on attentively watching it towards the end of totality I saw it materially lengthened—probably to two minutes—the Moon having apparently left more and more of it visible as she travelled across the Sun. It was always curved, and I did not remark any change of form, nor the slightest motion during the time the Sun was hidden. I saw this extraordinary prominence *four seconds after the end of totality*, but at this time it appeared detached from the Sun's limb, the strong white light of the corona intervening between the limb and the base of the prominence. About ten degrees south of the above object I saw during the totality a detached triangular spot of the same rose-colour, suspended, as it were, in the light of the corona, which gradually receded from the Moon's dark limb, as she moved onwards, and was therefore clearly connected with the Sun. Its form and position with respect to the large prominence continued exactly the same so long as I observed it. On the south limb of the Moon appeared a long range of rose-coloured flames, which seemed to be affected with a tremulous motion, though not to any great extent. The bright rose-red of the tops of these projections gradually faded towards their bases, and along the Moon's limb appeared a bright narrow line of a deep violet tint; not far from the western extremity of this long range of red flames was an isolated prominence, about forty seconds in altitude, and another of similar size and form at an angle of 145° from the north towards the east.'

I may add Mr. Dawes' account of the great prominence marked *a* in Mr. Airy's picture (fig. 58). 'A red protuberance of vivid brightness and very deep tint, arose to a height perhaps 1½ when first seen, and increased in length to 2′ or more, as the Moon's progress revealed it more completely. In shape it somewhat resembled a Turkish scimitar, the northern edge being convex, and the southern concave. Towards the apex it bent suddenly to the south,—or upwards, as seen in the telescope. Its northern edge was well defined, and of a deeper colour than the rest, especially towards its base. I should call it a rich carmine. The southern edge was less distinctly defined, and decidedly paler. It gave me the impression of a somewhat conical protuberance, partly hidden on its southern side by some intervening substance of a soft or flocculent character. The apex of this protuberance was paler than the base, and of a purplish tinge, *and it certainly had a flickering motion.* Its base was, from first to last, sharply bounded by the edge of the Moon. To my great astonishment, this marvellous object *continued visible for about five seconds,* as nearly as I could judge, *after the Sun began to reappear,* which took place many degrees to the south of the situation it occupied on the Moon's circumference. It then rapidly faded away, but *it did not vanish instantaneously.* From its extraordinary size, curious form, deep colour, and vivid brightness, this protuberance absorbed much of my attention; and I am therefore unable to state precisely what changes occurred in the other phenomena towards the end of the total obscuration.'*

Such are a few of the records of the appearance presented by the prominences during the eclipse of 1851. It would have been easy to have filled forty or

---

* The evidence of Mr. Dawes is very valuable, on account of his exceptional powers of vision. Probably he has never been surpassed in this respect. It may therefore be regarded as fortunate that he addressed his sole attention to one prominence; since some of the facts he detected are such as no later observations could have more satisfactorily established. Such, for instance, is his observation of the flickering motion of the upper part of the prominence. He was too well accustomed to recognise the apparent motions produced by our own atmosphere to be deceived into inferring real motion where none existed. His observation of the visibility of the prominence for several seconds after the Sun's reappearance confirms Mr. Hind's, and the fact is one of extreme importance, as tending to afford a measure of the luminosity of the larger prominences.

fifty pages with the narratives of the different observers, many of them skilful and well-practised astronomers. All agreed as to the principal details, and, as will be seen by figs. 58-63, there was a very satisfactory agreement in the pictures taken by different observers. It would appear that no doubt could any longer remain that the prominences were solar appendages of some sort. They had been visibly traversed by the Moon, according to the unexceptionable evidence of such astronomers as Airy, Hind, Dawes, and others. They had continued visible when our atmosphere had already begun to be lighted up by the direct rays of the returning Sun. At stations far apart they had presented the same appearance. It would seem therefore that nothing was wanting to establish their real relation to the solar orb, and that no question should any longer have existed as to the fact that the prominences are true solar appendages, since the proofs were so complete that they belonged neither to the Moon nor to our own atmosphere, and, further, that they were not mere optical illusions.

Yet, in the face of all this evidence, some astronomers were still found who maintained that the observations were insufficient to establish the existence of coloured objects so enormous as these must be if they really were solar appendages. It has always happened that in the ranks of the scientific army some have been found who refuse to credit the marvels which observation is continually revealing on every hand. Despite all the known wonders of the universe, the mere circumstance that the sole available interpretation of observed facts involves some surprising conclusion, is held by such men to be a sufficient reason for rejecting the observations of the most trustworthy astronomers.*

So it was in this instance. For nine long years astronomy was compelled to wait before she could be allowed to take possession of her well-won new territory. The amazing fact had been proved beyond all possibility of reasonable question that the great globe of

---

* One is almost ready to despair of the cause of scientific progress—to despair at least that that progress will ever be so rapid as it might readily become—when one finds that each new result must be established over and over again before it is admitted by a large proportion of the scientific world. It may be remarked, indeed, that the progress of science has been at least as seriously checked by undue caution as by undue boldness. It would seem almost as though some students of science were continually in dread lest the work of our observers should become too productive. The value of scientific observation seems to be enhanced in their eyes precisely in proportion as its fruits are insignificant. In all ages there have been those who would thus unwisely restrain the progress of legitimate inquiry. 'We must not admit that Jupiter has moons,' they said of old; 'the Evil One may have sent these appearances to deceive us. Let us wait for further observation.' 'The Sun cannot have spots,' they reasoned again, 'for the Eye of the Universe cannot suffer from ophthalmia. These things are illusions; let us wait for more satisfactory observations.' 'The idea that the Sun-spots wax and wane in a definite period is too fanciful for acceptance; and still more absurd is the conception that terrestrial magnetism can have any relations whatever with the progress of solar disturbance. We must wait for fresh researches.' 'Who can believe that flames, or clouds, or mountains, many times exceeding the Earth in magnitude, exist upon or close by the Sun? These things must needs be illusions; at any rate, fresh observations are required before such marvels can be admitted.' And as this has happened with facts now accepted, so it is happening, and so (it is feared) it will always happen, as respects many other facts which have been in truth demonstrated, but the demonstration of which does not chance to lie exactly on the surface.

the Sun is surrounded by a deep layer of coloured matter, while from portions of this vast envelope enormous protuberances start out, their height so vast that ten globes such as our Earth might be piled one upon the other on the Sun's surface without attaining to the summit of the highest prominences. But this great fact was not to take its place in our treatises of astronomy until, although twice proved already, it had been proved once again at least.

Accordingly, in 1860, when a total eclipse was to be visible in Spain, preparations were made for finally resolving the problem of the prominences. A host of skilful observers devoted their powers to demonstrating what had already been abundantly demonstrated. It happened fortunately, however, that amongst the astronomers who took part in observing this important eclipse, there were some few who duly recognised the importance of the occasion, and who therefore, leaving fruitless labours to others, applied themselves to solving important questions respecting the coloured prominences. Their results I now propose to describe; but, in the first place, I will quote the account which Goldschmidt, one of the most skilful telescopists of modern times, gave of the prominences visible on this occasion. Some of the facts recorded by him are of extreme interest and importance, especially as respects the colour of the prominences, since M. Goldschmidt's practice as a painter gave him exceptional experience in this respect.

M. Goldschmidt employed a telescope of four inches in aperture, magnifying about forty times. About *half a minute before totality* he could distinguish 'little grey clouds, isolated in part, and floating outside the solar disc at some distance from the edges. One of these isolated clouds of a rounded form, and another of an elongated form which touched the exterior edge of the Sun, were observed to be of a grey colour on the ground of the sky, which was a little brighter. An instant afterwards the pyramidical cloud became more clear, and then rose-coloured.' 'I had thus been present,' adds Goldschmidt, 'at the formation of a protuberance'—a remark which has been somewhat misunderstood through being quoted apart from the context. Clearly, Goldschmidt did not mean that under his eyes a prominence had started into existence; but that he had been able to recognise the gradual process by which the prominence became visible with the diminishing sunlight. 'Several smaller prominences,' he proceeds, 'were seen in the neighbourhood of this one, resembling globules of mother-of-pearl, but of an irregular form. These likewise became of a rose colour immediately afterwards, but quickly disappeared. The most imposing as well as complicated of the prominences—which I will call the chandelier (fig. 64) was grand beyond description. It rose up from the limb, appearing like slender tongues of fire, and of a rose colour; its edges were purple and transparent, allowing the interior of the prominence to be seen; in fact, I could see distinctly that this prominence was hollow. Shortly before the end of the totality, I saw escape from the extremities of these rose-coloured and transparent sheaves of light, a slight display in the shape of a fan, which gave to the protuberance a real resemblance to a *chandelier*. Its base, which at the commencement of the totality was noticed very decidedly on the black limb of the Moon, became slightly less attached, and the whole took an appearance more ethereal and vapourish; however, I did not lose sight of it for an instant. The jets of light which came from the extremities disappeared with the appearance of the first rays of the Sun; but it was not so with the protuberance itself, for, an instant before the end of the totality, I saw several small prominences appear lying close to each other on the right of its base, and forming a square, which is the character of toothed prominences; two others of the same height were seen on the left side of its base when the Sun had already appeared. The north horn of the solar crescent touched the last of these prominences *four minutes and forty seconds after the reappearance of the Sun.* The intense light caused me to abandon this interesting observation, for I was not at the time using a coloured glass; however, I am certain that the 'chandelier' and

Fig. 64.

the little prominences at its base had not disappeared up to that moment. Although I am convinced that the protuberances belong to the Sun, nevertheless I ought to remark that at the last moment I was surprised to see the direction of the *chandelier* referred to the centre of the Moon rather than to the centre of the Sun' (in other words, the chandelier was somewhat inclined). 'The height of the prominence was estimated about three minutes and a half at the commencement of the totality, and four minutes at the end. A second protuberance (fig. 65) appeared on the apparent right of this, at a distance of about thirty-five degrees, being about $3\frac{1}{3}$ minutes in height and nearly of the form of the symbol of the planet Saturn (♄); this prominence I have called the *hook*. A third, to the right of the two preceding and at a distance equal to that of the two others, assumed a form of which it is difficult to give an idea; however, I will call it the *tooth*. About eleven degrees to the right of the second protuberance, I noticed a fourth, small and in the form of a square; between this and the third there was situated a rose-coloured cloud, the shape of which was

FIG. 65.

elongated and bent, inclined at an angle of forty-five degrees towards the left limb of the Moon. This cloud was entirely detached, floating on the corona like a red cloud at sunset. Its centre was elevated above the limb of the Moon about one-half the altitude of the other prominences, or about two minutes. A fifth protuberance also appeared at the beginning in the south-east, and was of increased size in the middle of the totality. *I ought to remark that all the protuberances which I noticed had a tendency in their form to describe a curve, the concavity of which was turned from the side of the west.*'

But the principal interest of the eclipse of 1860 attaches to the photographic work of Fr. Secchi and Mr. De La Rue.

To secure a photograph of the Sun itself is a problem of a wholly different character from that which these two astronomers had set themselves; for owing to the intense brilliancy of the solar light, the exposure necessary to secure a photograph of the Sun is of the briefest. For a minute fraction of a second the image of the Sun must fall on the prepared plate; but for no longer interval, or a mere blurred patch would reward the photographer. But to secure a picture of the solar prominences, an exposure of appreciable length is required. Nor was it one of the least difficulties of Secchi and De La Rue that the length of time actually required was unknown to them. All their work was tentative; and there was every reason to fear that success was impossible on account of the colour of the prominences. A red or orange light has commonly no actinic power whatever; insomuch that the 'dark room' of the photographer is, in a photographic sense, nearly as dark when its walls are of orange-coloured glass as though they were absolutely opaque. So that if the light of the prominences really were pure red, it was hopeless to endeavour to obtain photographs of these objects.*

Despite the difficulties which the problem presented, and the disheartening anticipations which their experience as photographers justified them in forming, the two physicists I have named boldly entered on their task. They adopted different methods. Mr. De La Rue employed the Kew heliograph, and the small image formed at the focus of the object-glass was enlarged before being received upon the plate. Secchi preferred to receive on the plate the image formed by the object-glass of his telescope. This image was about an inch in diameter.

'The result,' says Secchi, 'proved that both systems

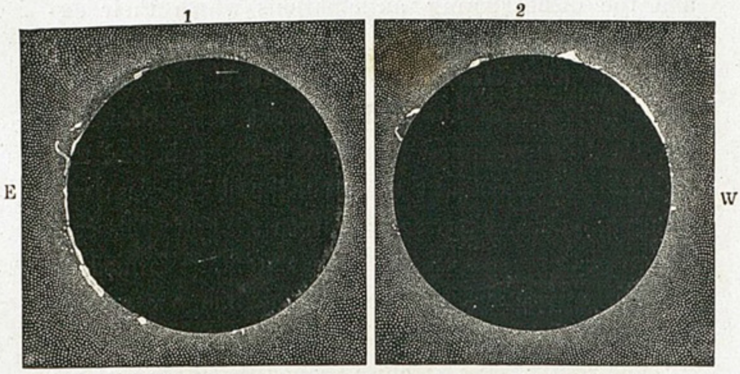
---

* It is worthy of notice, and affords a fresh proof (if proof were wanted) of the fact that observations may involve important results altogether apart from their direct significance, that the successful photographing of the prominences afforded all but complete proof of that which was afterwards demonstrated by the spectroscope—the fact, namely, that the prominences consist of glowing vapour. Secchi or De La Rue might quite confidently have asserted that when the prominences came to be examined with the spectroscope, their spectra would show a band or bands near the blue end of the spectrum, separated by wide dark gaps from certain bands in the red and orange part. The greater part of the light of the prominences corresponded to these latter bands, but that part by which the photographs were taken corresponded to the former.

are excellent, each having its special advantages. In the enlarged image one can distinguish more details, but the direct image gives a greater extension to the corona.'

The two observers were situated at different stations. —Fr. Secchi at the Desierto de las Palmas, near the Mediterranean; Mr. De La Rue at Riva Bellosa, near the Atlantic. Thus an interval of about six minutes elapsed before the Moon's shadow passed from one station to the other, and an opportunity was afforded of determining whether the prominences change rapidly in figure. Besides this, there was a slight difference in the apparent course traversed by the Moon's disc in crossing the Sun's; for Secchi and De La Rue were at different distances—and, as a matter of fact, on opposite sides of the path of the centre of the Moon's

Fig. 66. Fig. 67.



From photographs of the Sun during the total solar eclipse of June 1860. (*De La Rue.*)

shadow, so that De La Rue's series of photographs shows more of the prominences on the superior part of the Sun's limb, while Secchi's series shows more of the prominences on the inferior portion.

It will be unnecessary—so closely do the two series resemble each other in all essential respects—to exhibit both; but further on there will be found a copy of one of Secchi's pictures (fig. 82, p. 327), which the reader may compare with figs. 66 and 67, copied from Mr. De La Rue's photographs.

It will be noticed that fig. 66 represents the earliest phase. The Moon is advancing from right to left, and has just hidden the last fine thread of direct sunlight on the left. Thus we see the full height of the prominences on the left, while no prominences are seen at all on the right of the Sun. At the upper and lower part of the Sun's limb the prominences are partly concealed, and necessarily remain so throughout the eclipse. In fig. 67 the Moon has obliterated a large proportion of the prominences on the left, while it has in turn revealed a number of small prominences, a long range, or sierra, and a lofty and massive projection, on the upper right-hand quadrant. Fr. Secchi observed the prominences directly, and with great care, while his assistants managed the photography. Amongst the phenomena he noticed, I may mention the circumstance that the strange prominence seen in both figures on the upper left-hand quadrant, possessed a helicoidal*

---

* It would be well if this word 'helicoidal' were always employed in this sense, in preference to the word 'spiral,' which might be conveniently

structure. In the magnified picture from Mr. De La Rue's photographs, this structure can be clearly recognised.*

Fr. Secchi thus summed up the result of his observations:—

1. The prominences are not mere optical illusions; they are real phenomena appertaining to the Sun. Our observations having been made at two places separated a hundred leagues from each other, it is impossible to suppose that shapes so well defined and so exactly identical could be produced by a phenomenon resembling mirage.

2. The prominences are collections of luminous matter of great brilliancy and possessing a remarkable photographic activity. This activity is so great that many of the prominences which are visible in the negatives could not be seen directly, even with powerful instruments, perhaps because they emitted only chemical rays and few or no luminous rays.†

3. There are masses of prominence matter suspended

---

restricted to curves lying in a plane. I am not insisting on either term as more correct, but only on the convenience of a recognised term to express spiral curves not lying in one plane.

* Secchi notices the apparent encroachment of this prominence upon the Moon's limb—a peculiarity which he ascribes to the fact that the Moon was moving away from the prominence while the plate was under exposure. Doubtless, this circumstance produced its effect; but the phenomenon is chiefly due, according to the experimental researches of Dr. Curtis, to a process of chemical encroachment taking place during the development of the plate.

† The spectroscopic observations made during the American eclipse tend to throw doubt on this conclusion, which, however, is in accordance with the observations made by Mr. De La Rue, and also, be it noticed, with laboratory experiments on the spectrum of hydrogen.

and isolated like clouds in the air. If their form is variable the variations take place so slowly that it is impossible to recognise their effect during an interval of ten minutes.*

4. Besides the prominences, a zone exists of the same material, enveloping the whole of the Sun's globe.† The prominences spring from this envelope; they are masses which raise themselves above the general level, and even at times detach themselves from it. Some among them resemble smoke from chimneys or from the craters of volcanoes, which, when arrived at a certain elevation, yields to a current of air, and extends horizontally.

5. The number of prominences is incalculable. When observing the Sun directly, its globe appeared to be encircled with flames; there were so many that it seemed hopeless to attempt to count them.

6. The height of the prominences is very great, especially if we notice that account must be taken of the portion concealed by the Moon. Thus estimated, the largest protuberance visible in 1860 was certainly not less than three minutes in height, which corresponds with about ten times the diameter of the Earth; the others had a height of from one to two minutes.

---

[*] Later observations show that this opinion must be modified, and that though many of the large prominences remain unchanged in figure for a considerable interval, yet others change very rapidly.

[†] Grant, Swan, and Von Littrow had already recognised this; and Leverrier, from observations made during the same eclipse, had come to the same conclusion. The matter is alluded to further on. (See note in pp. 290, 291.)

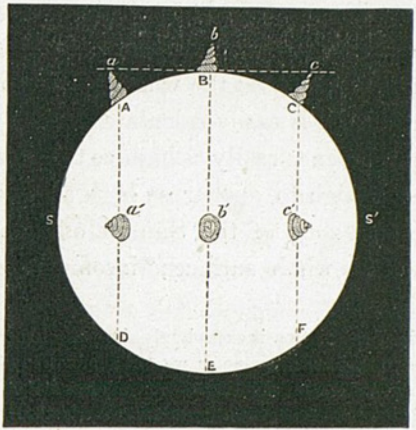
We may consider that the prominences were finally placed in their true place in the solar scheme by the observations made in 1860. Doubts still continued to be expressed by a few; but the majority were satisfied; and henceforth the coloured prominences were very generally regarded as solar appendages.

It may be well to consider briefly the interest of this result before proceeding to inquire into the researches of the last few years, which even surpass in importance those already described.

In the first place, it must be remembered that though the prominences are seen all round the circumference of the solar disc they do not really form a circle. They are the foreshortened projections of objects which may lie—and many of which *must* lie—thousands of miles from that circle on the Sun which at the moment forms the apparent boundary of his disc. We know, in fact, that certain prominences are as high as three minutes—that is, extend to some 800,000 miles from the Sun's surface. Now, supposing A B C to represent a part of the Sun's circumference, and *a b c* three prominences of this height, an observer, viewing the Sun from a point at a great distance away towards the right or left would only see the extreme tips of the prominences *a* and *c*, while he would see the full height of the prominence *b*. But in order that these two prominences should be thus in appearance sunk below the solar limb, the line *a c* would need to be about 500,000 miles in length. So that if there were any prominence of so great a height as *a*, *b*, or *c* along any part of the arc A B C it would appear to rise above the Sun's level. Nor need we modify this conclusion on account of the layer of red matter which has been shown by the observations already described to surround the solar globe, since undoubtedly the highest prominences extend considerably more than three minutes even above the upper surface of this red layer.

But now, having considered a side view, let us conceive the case of a view from above. Suppose *a′*, *b′*, and *c′* (fig. 68) to represent the same three prominences brought round so as to be viewed from above. Then the actual boundary of the Sun thus viewed would be represented by the circle s s′, such that whereas *a′ c′* represents 500,000 miles, s s′ represents 850,000 miles. In order, therefore, that a prominence of the full height of 80,000 miles seen from a distant point lying towards the right or left may be visible above the level of the red envelope, it is obvious

Fig. 68.



Illustrating the distribution of large prominences over the Sun's surface.

that it must lie somewhere on the part A D F C, so far as the hemisphere shown in fig. 68 is concerned, or on a similar portion of the other hemisphere. Now, the curved surface A F bears to the whole surface of the hemisphere S D S′ C the same proportion that *a′ c′* bears to S S′. Hence the extent of surface on which a prominence 80,000 miles high must be placed in order that it should be visible from a distant point, is about fifty eighty-fifths, or ten-seventeenths of the whole surface of the Sun. Hence it would follow that if ten such prominences could at any time be counted by the observer, then only some seventeen exist in all probability at that time over the whole surface of the Sun.

Furthermore, it is easily calculated in the same way that for a prominence really as high as those which seem three minutes high to appear so high as two minutes, it must lie on a zone of the Sun including nearly one-third part of his whole surface;* insomuch that if even

---

* The mode of calculation is exceedingly simple. Thus, adding twice three minutes to the Sun's diameter we get about thirty-eight minutes (which may be regarded as including the red layer *as well as* three minutes for the prominences). Now, for a prominence really three minutes high to appear at least two minutes high (I use minutes here as a convenient expression both for real and apparent height) its summit must obviously lie not further than $\sqrt{1 \times 37}$ minutes from a great circle of an imaginary sphere thirty-eight minutes in diameter, this great circle being that whose plane is perpendicular to the line of sight from the observer. Thus the whole zone on which its summit may lie will constitute rather more than twelve thirty-eighths of the surface of that imaginary sphere, and obviously the whole zone on which its base may lie will constitute the same proportion—that is, nearly one-third—of the surface of the Sun itself.

The general rule would be as follows:—The zone on which a prominence—which if really on the limb would appear *m* minutes high—must so many as ten prominences of this height could at any time be counted (which cannot but be regarded as a very exceptional circumstance) then only about thirty such prominences exist in all probability at that time, over the whole of the Sun's surface. In such a case also the total number of prominences actually visible above the level of the red layer should (by the result of the preceding paragraph) be ten-seventeenths of thirty, or about eighteen,—unless, besides the thirty prominences of the height mentioned, there were other inferior ones, in which case more than eighteen would of course be visible.

It follows obviously from the comparatively small number of very high prominences ordinarily seen either during eclipses or by the methods presently to be described, that even if the astronomer could roam at his will around the Sun in search of lofty prominences, instead of being limited, as he is in reality, to a single view of the Sun as a disc, he would seldom (if ever) be able to discover many of these amazing objects. Although, taking eclipse with eclipse, the prominences cannot be regarded as exceptional, since every total eclipse yet observed has revealed one or more prominences of an amazing altitude, yet as regards the Sun's surface they are few and far between. So few, indeed,

---

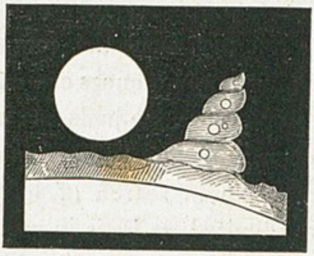
lie, in order that it may not appear less than $m'$ minutes high, covers a space of

$$\frac{\sqrt{(m-m')\,(d+m+m'}}{d+2m}.\mathrm{S}$$

where $d$ is the Sun's apparent diameter in minutes and S the Sun's surface.

must they be, as to render it extremely probable that not one of those yet seen has been so exactly on the real limb of the Sun as to be seen in its full proportions; nor would it be very hazardous to assign five or six minutes as a height to which the highest prominences, when most favourably seen, may be expected to attain.* So that we may regard 130,000 miles as no exaggerated estimate of the height of these wonderful objects. If fig. 69 represents a flame four minutes in height, then the four interior (or so-called terrestrial) planets would be represented on the same scale by the four small discs shown on the convolutions—Mercury uppermost, then Venus, the Earth and Moon, and Mars; while even the giant bulk of Jupiter would bear no greater proportion to the enormous solar prominence than is shown by the dimensions of the largest disc in fig. 69.

Fig. 69.



Illustrating the vast scale of the larger prominences.

Let us proceed, however, to those more recent re-

---

[*] Since this was written I received the account of Respighi's observations. In one instance he has seen a prominence no less than six minutes, or about 160,000 miles in height.

searches by which the actual constitution of the prominences, and even in some sort the part they play in the economy of the Sun's globe, have been ascertained by astronomers and physicists.

Direct observation had been employed during the eclipses of 1842, 1851, 1855, and 1860. Photographic records had been obtained during the last of these eclipses. But a method of research more powerful than telescopic observation or photography was now to be applied to the solar prominences. These objects, whether regarded as self-luminous or as shining by reflecting certain portions of the solar light, were clearly well suited for the application of the spectroscope. That powerful mode of analysis which will detect the elements present, even to the minutest conceivable extent, in coloured terrestrial flames, seemed to promise results of the highest interest and importance whenever it should be applied to these coloured solar prominences.

Accordingly, Colonel Tennant (to whom astronomy owes a debt of gratitude on this account) early called the attention of the scientific world to the importance of studying the great eclipse of August 18, 1868, not only with the telescope and by means of photographic appliances, but under the searching powers of the most wonderful method of research yet invented by man.

Accordingly, two well-furnished expeditions set forth from England, supplied, through the liberality of the Indian Government and of the learned societies of this country, with the means of satisfactorily pursuing the important investigations which had been suggested by Messrs. Huggins, De La Rue, and other experienced astronomers and physicists.

In the first place it will be well to consider the photographic and other records of this eclipse.

I have selected the six photographs taken by Major (now Lieut.-Col.) Tennant, at Guntoor, as not only affording the most complete illustration of this particular eclipse, but as supplying the best illustration of eclipse photographs yet obtained. The length of time during which the Indian eclipse lasted gave Major Tennant the means of taking a large number of records, with a longer exposure than was possible during the American eclipse; and a value belongs to one series taken at a given station with the same instrument and by the same process which cannot be assigned to a larger series combining the labours of different observers. It is on this account that I have preferred to present in Plate III. the photographs of the eclipse of 1868, rather than those obtained during the eclipse of 1869.

The instrument employed by Major Tennant was a Browning, with reflector of nine inches clear aperture.

'It was determined,' says Major Tennant, 'to adopt the Newtonian form, and to place the sensitive plates at the side of the tube in order to find room, which would be very difficult at the end of the tube; the small mirror thus rendered necessary was made unusually large in order that it might reflect all the rays from a surface of some diameter, and it was hoped that by this means, and by giving a sufficient exposure, a picture of the corona might be secured; but light clouds so decreased the actinic power of the light as to defeat this, though still we have traces of the corona.' Major Tennant remarks, also, that although, owing to the lowness of the latitude, an entirely new design was required, while 'from the necessities of its construction the instrument could not be tried before being sent out,' yet the photographic arrangements were very good and convenient, the instrument very firm and steady, while the force required to direct the telescope was very small.

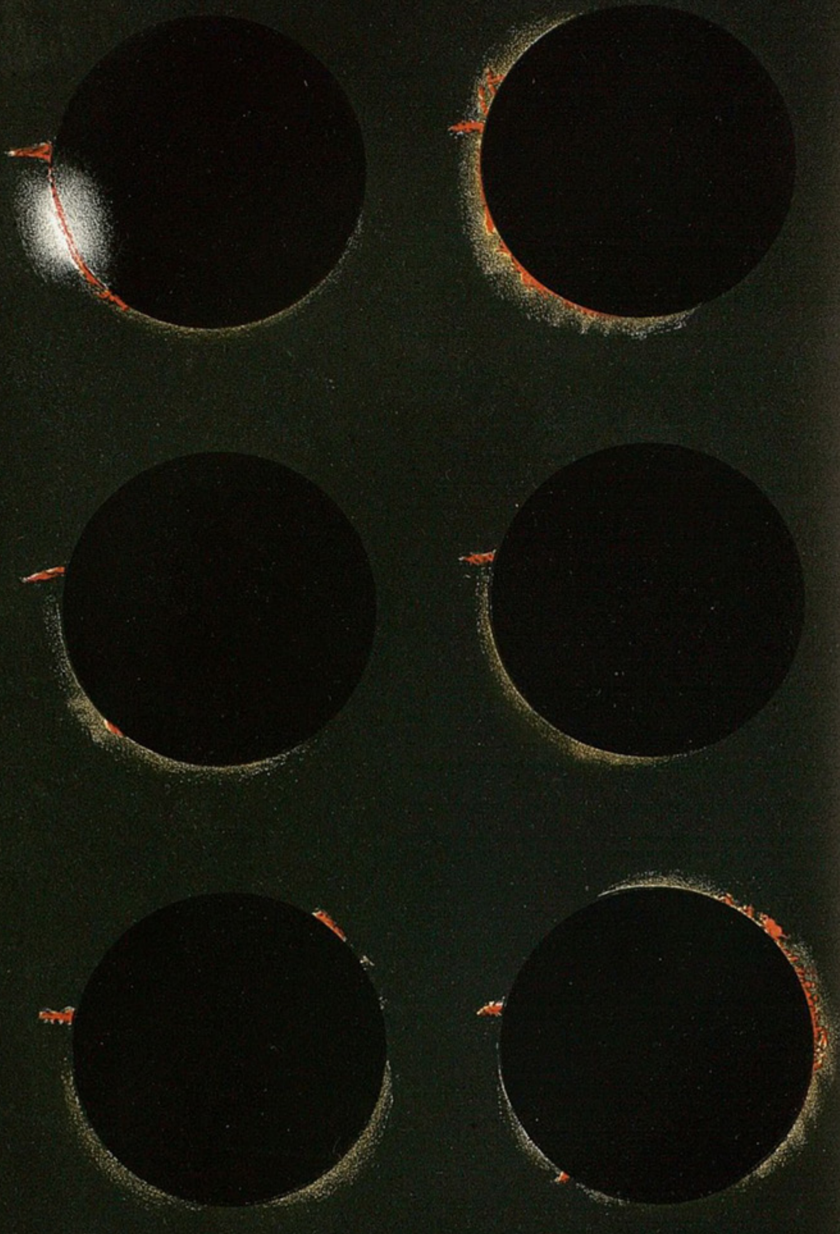
The weather had been remarkably clear for several days before the eclipse, and a magnificent view of the phenomenon would have been obtained had August 18 resembled its precursors. But unfortunately on the morning of the eclipse the eastern sky was clouded over with cumulo-stratus clouds, which although not sufficiently dense to interfere with vision, were very annoying accompaniments to the processes of solar photography. It had been arranged that during the six minutes of totality six plates should be exposed. Notwithstanding the unfavourable condition of the air, this arrangement was carried out; and it was fortunate, as it turned out, that no longer time was given for the exposure of the successive negatives, since otherwise they would in all probability have been spoiled.

Sergeant Phillips and two Sappers, specially trained for the purpose at Mr. De La Rue's observatory, Cranford, superintended the subordinate arrangements.

Major Tennant thus describes the six photographs (see Plate III):—

Photograph 1. The exposure was as short as possible—probably less than a second. In this photograph some glare has fogged the plate a little, but the following things are noteworthy:—First. The enormous horn on the north side, whose height, as derived from the Moon's diameter (about thirty-four minutes), is three minutes, eighteen seconds, corresponding at the distance of the Sun to 88,900 miles. Secondly. The ridge of light extending round the upper limb of the Moon, so bright as to be markedly seen through the thickest of the fog, though the glorious horn is paled. Near the horn this light is clearly broken into beads, as may be seen in Mr. James's picture of the horn, and is occasionally double. Thirdly. At its southern end this ridge is terminated by a protuberance of singularly complicated structure.

Photograph 2. Taken almost exactly one minute after, and exposed for five seconds. 'North of the great horn we see a peculiar and faint figure, shaped (generally) like a bow, but with interruptions and nuclei of light, and having an arrow-like spike in the middle of its base. Next we have the great horn, which has here been over-exposed. The Moon's limb has advanced as far as the point where the two lower branches seen in No. 1 meet, leaving only a trace of the bifurcation, and the upper part of the horn is of course larger than in No. 1, as fainter parts have come



ECLIPSE OF AUGT 18TH 1868.

Photographed by Major Tennant.

into view. When this photograph is well illuminated, traces of the spiral structure, so apparent in the rest, are seen; but it would be difficult to recognise it without the other pictures. Passing on, the Moon's limb is on the edge of the faintly luminous stratum I have described in No. 1, which is, of course, now far more marked; and as we approach the south we see the prominences mentioned under No. 1 as being in the faint stratum, and then reach the upper part of the former terminal prominence, where the long exposure has now blotted out all traces of structure.' *

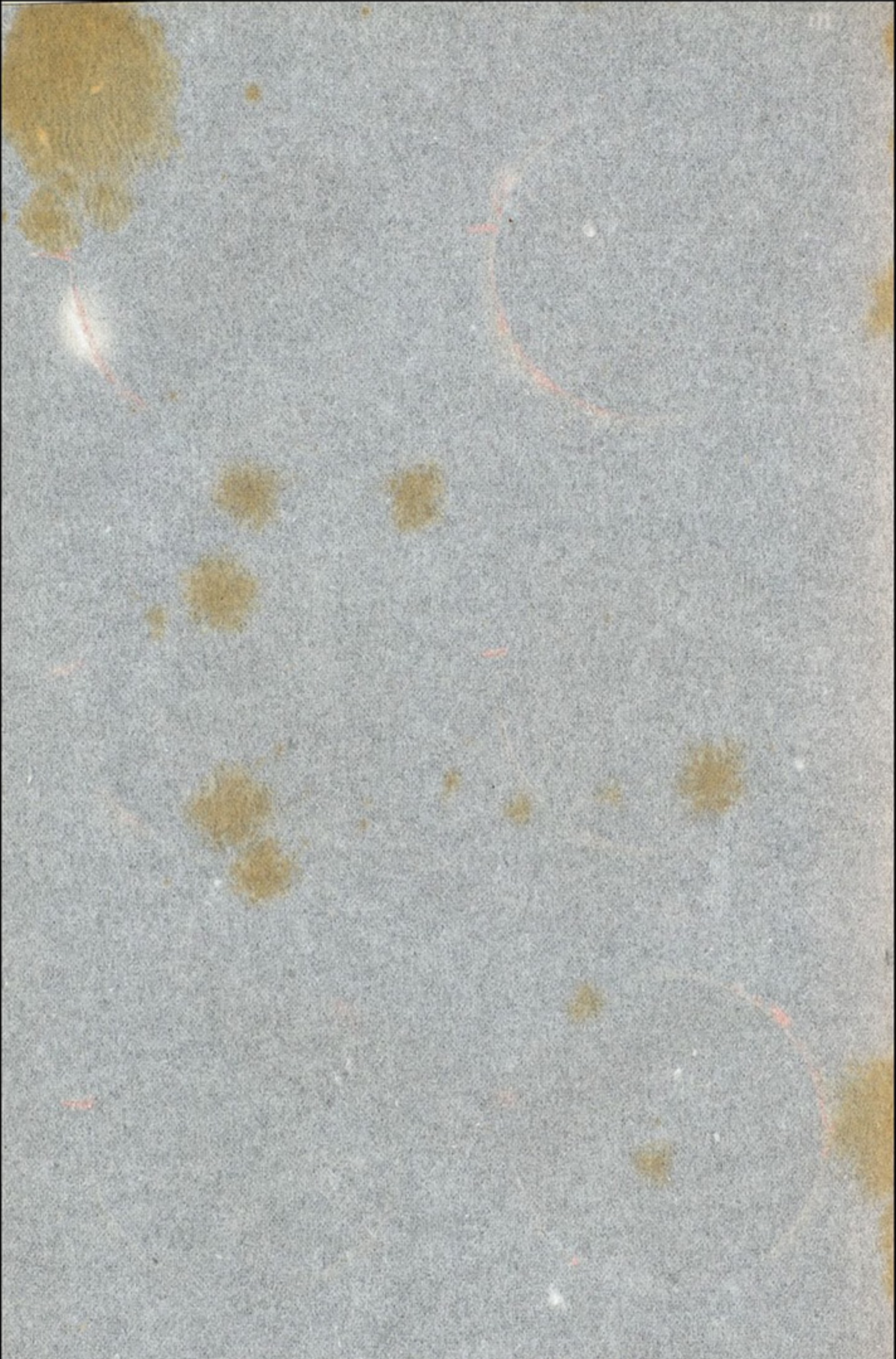
Photograph 3. Exposed one minute six seconds later, and for ten seconds. 'It is evidently less affected by light than No. 1, and was probably taken through a light cloud. Here the great horn again claims attention; its spiral structure is now clearly made out. The outline of the Moon is wanting till we reach the south terminal protuberance of No. 1, which is now nearly hidden; and in its neighbourhood we again see the flare of which I spoke in describing No. 2.'

Photograph 4. Exposed fifty-two seconds later, and for five seconds. 'Nothing is seen but the great horn, but that picture is of singular beauty. The continually varying intensity of the light and its markedly spiral structure are the material points.'

Photograph 5. Exposed one minute three seconds

---

* 'The neighbourhood of both these prominences is marked by light, flaring into the corona in irregularly-curved lines,' adds Major Tennant, 'and this corresponds to the part of the corona which has been described by Captain Branfill and others as most bright.' This remark will be found important when we are upon the subject of the corona.



ECLIPSE OF AUGT 18TH 1868.
Photographed by Major Tennant.

into view. When this photograph is well illuminated, traces of the spiral structure, so apparent in the rest, are seen; but it would be difficult to recognise it without the other pictures. Passing on, the Moon's limb is on the edge of the faintly luminous stratum I have described in No. 1, which is, of course, now far more marked; and as we approach the south we see the prominences mentioned under No. 1 as being in the faint stratum, and then reach the upper part of the former terminal prominence, where the long exposure has now blotted out all traces of structure.'*

Photograph 3. Exposed one minute six seconds later, and for ten seconds. 'It is evidently less affected by light than No. 1, and was probably taken through a light cloud. Here the great horn again claims attention; its spiral structure is now clearly made out. The outline of the Moon is wanting till we reach the south terminal protuberance of No. 1, which is now nearly hidden; and in its neighbourhood we again see the flare of which I spoke in describing No. 2.'

Photograph 4. Exposed fifty-two seconds later, and for five seconds. 'Nothing is seen but the great horn, but that picture is of singular beauty. The continually varying intensity of the light and its markedly spiral structure are the material points.'

Photograph 5. Exposed one minute three seconds

---

* 'The neighbourhood of both these prominences is marked by light, flaring into the corona in irregularly-curved lines,' adds Major Tennant, 'and this corresponds to the part of the corona which has been described by Captain Branfill and others as most bright.' This remark will be found important when we are upon the subject of the corona.

later, and for a short interval only. 'The great horn is still conspicuous on the east side of the disc; and the outline of the Moon is marked round three-quarters of the circumference. The top of a prominence on the north-west side of the limb is seen.'

Photograph 6. This plate was exposed but for a short time, like the first and fifth, and one and a half minutes after the last. 'It is a picture as late as it was thought prudent to take it during the absence of the Sun. On the south-east side, where the corona was brightest, are traces of it; and the whole limb from north to south-west shows a trace of corona, and a series of beautiful protuberances, only one of which, however, is large. In the original it appears inflated. To me it resembles exactly a toy balloon in the shape of an animal. There is a mark as of an open mouth; the eye and the neck are marked, and the tail stands out stiffly behind. I mention these details because they complete the appearance of inflation by the resemblance they cause to what one has seen in that state. I would especially draw attention to the appearance as of a strong current of air (so to speak) blowing from north to south, and bending over and even detaching and carrying away the tops of prominences.'

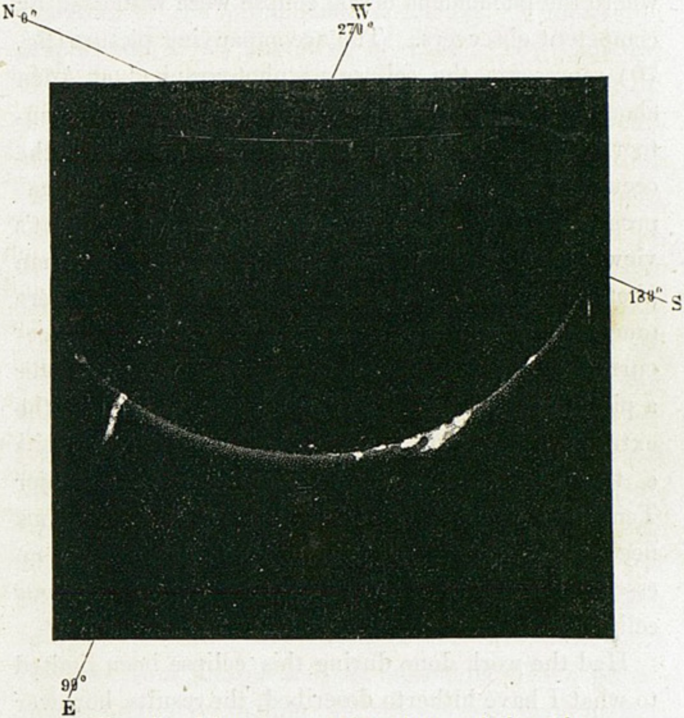
It is well worthy of notice, also, that the horn still continues partly visible. It cannot, indeed, have been completely hidden even at the moment when the Sun was about to reappear.*

---

* I think this account would be incomplete were I not to quote the generous remarks with which Major Tennant closes this section of his

At other places along the track of the eclipse similar appearances were perceived; nor have we any

FIG. 70.



The eclipsed Sun, August 1860; photographed at Aden.

---

report. 'Throughout,' he says, 'I have spoken of myself as sharing in the photography. I certainly exposed and developed a few plates, and generally superintended all that went on, and so far I was the superintendent of this department; but the trouble of endeavouring to break in new and unskilled hands, and of carrying out the details, fell on Sergeant Phillips, to whose steadiness and determination to succeed, the result in this matter is mainly due. He was, moreover, most useful distinct evidence pointing to a change of figure during the progress of the Moon's shadow from Aden, in the west, to Mantawaloc Kekee, the most easterly station where the phenomena of the eclipse were witnessed by competent observers. The accompanying picture (fig. 70) represents the eclipse as photographed at Aden about forty minutes before the shadow reached Guntoor. There is little in this picture to establish the occurrence of a change of figure in the 'great horn prominence. In comparing it with Major Tennant's views it must be remembered (in justice to the German photographers) that the atmospheric conditions were much less favourable at Aden, where the eclipse occurred very shortly after sunrise. In the next chapter a picture (fig. 84) of the eclipsed Sun as seen at the extreme easterly station, Mantawaloc Kekee, exhibits a tolerably close general accordance with Major Tennant's views. Other pictures and descriptions need not here be referred to, because they differ in no essential respects from those relating to previous eclipses.

Had the work done during this eclipse been limited to what I have hitherto described, the results, however interesting, would not have advanced in any important respects our knowledge of solar physics. We have now, however, to consider far more important results.

At nearly all the stations preparations had been

---

in other ways, for I could always rely on his care, and was thus enabled to avoid constant exposure to the Sun. Indirectly he has been the means therefore of facilitating my other work.'

made to observe the coloured prominences and the corona with the spectroscope. All the spectroscopic observers were successful in determining the general character of the prominence spectrum. I select the graphic and lively narrative of Lieut. (now Captain) Herschel, who commanded the expedition sent out by the Royal Society, and observed the eclipse at Jamkhandi. 'The week preceding the event,' he writes, 'had quite prepared me for disappointment. There seems to be an annual cloudy and rainy season at Jamkhandi, which lasts about a fortnight, and was said to be somewhat more marked than usual this year. The morning broke, however, as usual—clear, but the driving monsoon-clouds soon showed the kind of sky we were to expect. About a quarter of a minute before totality a third cloud obscured the Sun.' He had directed the spectroscope towards that part of the Sun which would be the last to disappear, and was watching the solar spectrum as it grew gradually narrower and narrower. It will be understood from what has been said towards the close of Chapter III. that the moment this spectrum disappeared the spectrum of the prominence matter (or failing the presence of such matter, the spectrum of the corona) would start into view. 'You may conceive,' he says, addressing Dr. Huggins, 'my state of nervous tension at this moment. Whatever the corona was competent to show must in a few seconds have been revealed; unless, indeed, it should happen that a prominence or *sierra* should be situated at that particular spot, in which case the double spectrum would be presented. But the solar spectrum faded out while it had still appreciable width, and I knew a cloud was the cause.' He turned to the finder of the telescope, removed the dark glass, and waited for the passage of the cloud. As the telescope was mounted so as to follow the motion of the Sun by means of clockwork, there was simply nothing to be done. 'I was,' he writes, 'in that fever of philosophical impatience, which recognises the futility of irritation even while it chafes under the knowledge of fleeting seconds. How long I waited I cannot say—perhaps half a minute. I can well recall the kind of frenzied temptation to turn screws and *look somewhere else*, checked by the calm ticking of the clock telling of a firm hold in the right place, cloud or no cloud. And soon the cloud hurried over, following the Moon's direction, and therefore revealing first the upper edge of the disc, with its radiating and—as I fancied—scintillating corona; and then the lower. Instantly I marked a prominence (of whose identity there will be no question) near the needle point in the finder. Those few seconds of unveiling were practically all that I saw of the eclipse as a spectacle. With the exception of a hurried glance into the finder at a later period to watch for another break, I was the whole time engaged at the spectroscope. I have not the remotest idea (from actual experience) of the external phenomena which were presented to the thousands of upturned faces of those whose voices I heard outside. I might easily have lifted the curtain and looked out while the clouds were obstructing. That I did not do so is only to be explained by the absence of mind as regarded all else, produced by the concentration of my attention on the problem before me.' Resuming the narrative of his observations, Lieut. Herschel proceeds:—'A rapid turn of the declination-screw covered the prominence with the needle point, and in another instant I was at the spectroscope. A single glance, and the problem was solved. *Three vivid lines—red, orange, and blue!* I think I was a little excited about this time, for I shouted quite unnecessarily to my recorder, "Red! green! yellow!" quite conscious of the fact that I meant orange and blue.'

The problem was indeed in a general sense solved. Among all the ideas that had been put forward respecting the actual constitution of the prominences one, and one only, now remained available. Had the prominences been self-luminous solar mountains or clouds (properly so called) a continuous spectrum would have been seen; had they been bodies which shine by reflecting solar light the rainbow-tinted streak crossed by dark lines which forms the solar spectrum would have made its appearance. The sole remaining theory was that the prominences consist of glowing gaseous matter.

But it remained now to determine what the nature of this matter might be. To this work Lieut. Herschel turned his attention, and to this work also all the observers who, like him, had succeeded in solving the general problem, devoted themselves during the remaining minutes of totality. The results they obtained may be thus summed up:—Herschel noted three lines—a red one, which he regarded as possibly C, the red line of hydrogen; an orange one, which he regarded as almost certainly D, the orange (double line) of sodium; and a blue one, which he thought might possibly be F, the blue-green line of hydrogen. Major Tennant saw five lines, which he associated with the lines C, D, *b*, F, (probably) and G. M. Janssen saw six—red, yellow, green (two), blue, and violet, and established the coincidence of the red and blue lines with the lines C and F. M. Rayet saw no less than nine lines, five of which he recognised as brighter than the rest; these he associated with the lines B, D, E, *b*, and F.

But we need not consider the evidence on which these various determinations rest, since a new and far more effective mode of observation was to place beyond all possibility of question the true position of these several lines, and of others as yet undetected.

M. Janssen had been struck during the progress of the eclipse by the exceeding brilliancy of some of the bright lines which formed the prominence spectrum. 'Immédiatement après la totalité,' he writes, 'deux magnifiques protubérances ont apparu: l'une d'elles, de plus de trois minutes de hauteur, brillait d'une splendeur qu'il est difficile d'imaginer.' As he looked, the idea seized him that these lines might be seen when the Sun is not eclipsed. He cried out, he tells us, 'Je reverrai ces lignes-là!'

The principle by which he hoped to effect this will be understood by the reader who has carefully studied the latter portion of Chapter III. Briefly re-stated it is this: The light of the prominences when dispersed by the spectroscope forms a few lines; that of the illuminated terrestrial atmosphere is spread out into the rainbow-tinted solar spectrum. Therefore, if we only use adequate dispersive power, we can cause the prominence-lines to show conspicuously on the background of dispersed atmospheric light.

Clouds which gathered over the face of the Sun soon after totality prevented M. Janssen that day from pursuing this idea. But on the morrow he applied it with complete success. 'I have experienced,' he said, 'to-day a continuous eclipse.'

We have seen how it becomes possible by this method not only to recognise the spectrum of a prominence during full daylight, but by a series of sectional views, so to speak, to determine the figure of a prominence. To this work Janssen applied himself, and he was presently able to forward to Europe news of his complete success in this new branch of research.

The news of this important discovery was nearly two months in reaching Europe, and a few days before it arrived Mr. Lockyer had independently obtained a similar result. The history of his work and of the way in which it was suggested is not without interest. In May 1866, Dr. Huggins had examined the spectrum of the star which blazed out suddenly in that year in the constellation Corona. He found that this star—now known as T Coronæ *—gave a spectrum which

---

* In Mr. Roscoe's valuable work on spectroscopic analysis, this star

differed altogether from any he had previously examined. There was the rainbow-tinted streak crossed by dark lines which ordinarily forms the spectrum of a star; but over this, and obviously corresponding to a large proportion of the star's light, there were bright lines. These bright lines corresponded in position with the lines belonging to hydrogen; so that Dr. Huggins was able to pronounce that the great increase in the star's light was due to an outburst of glowing hydrogen. Afterwards he found that other stars, and notably the middle star in the conspicuous **W** group of Cassiopeia, show bright lines, superposed on the rainbow-tinted background, though relatively far less bright than those seen in the spectrum of T Coronæ. In these cases, also, the lines were those of glowing hydrogen.

Here, then, was a perfectly apt illustration of the principle dwelt on in the concluding part of Chapter III. Here were the lines of a certain element rendered separately visible as bright lines, though the total amount of light received from the glowing hydrogen was not necessarily (in the case at least of the star in Cassiopeia) equivalent, or nearly so, to the remaining portion of the star's light. The concentration of the light into three definite lines enabled it to

is always referred to as $\tau$ Coronæ. This is a mistake. The star $\tau$ Coronæ is a well-known fifth-magnitude star towards the north of the constellation. The real variable (ordinarily a tenth-magnitude star) lies to the south of all the conspicuous stars of Corona. It is called T in accordance with the rule by which the variables successively discovered in a constellation are named from the last letters of the alphabet, beginning with R.

prevail, as far as intrinsic brightness was concerned, over the diffused light, absolutely greater in amount, which formed the rainbow-tinted streak.

We cannot wonder, therefore, that the idea should at once have been suggested that if any portions of the Sun—as, for example, the coloured prominences—consist of glowing gas, the spectrum of such portions might be recognisable even amidst the spectrum of the far more intense light (so far as absolute brightness is concerned) of the solar photosphere.

Mr. Lockyer was preparing about this time to undertake a careful scrútiny of the photosphere with more powerful spectroscopic appliances than had yet been employed. Amongst the expectations which he formed at that time, there is a reference to the problem presented by the prominences. It may be interesting to give at full length the concluding words of the paper he addressed to the Royal Society :—

'Seeing that spectrum analysis has already been applied to the stars with such success, it is not too much to think that an attentive and *detailed* spectroscopic examination of the Sun's surface may bring us much knowledge bearing on the physical constitution of that luminary. For instance, if the theory of absorption be true (!) we may suppose that in a deep spot rays might be absorbed which would escape absorption in the higher strata of the atmosphere; hence, also, the darkness of a line may depend somewhat on the depth of the absorbing atmosphere. May not also some of the variable lines visible in the solar spectrum be due to absorption in the region of the spots? and may not the spectroscope afford us evidence of the existence of the 'red-flames' which total eclipses have revealed to us in the Sun's atmosphere, although they escape all other methods of observation at other times? and if so may we not learn something from this of the recent outburst of the star in Corona?'

It does not appear quite clearly from this passage whether Mr. Lockyer expected (as De La Rue had before) to find traces of the prominences, when examining the Sun's surface, or whether it fell in with his plans to examine outside the Sun's disc. But the latter is the more probable explanation. The grant he asked for from the Royal Society was allowed him, and the construction of a suitable spectroscope was entrusted to Mr. Browning (after a delay consequent on the death of Mr. Cooke). In the meantime Dr. Huggins applied his spectroscope unsuccessfully to the search, and Fr. Secchi, who had thought of examining the Sun's edge, gave up the attempt on hearing of Mr. Lockyer's failure to detect anything remarkable there.*

Whether, supposing no further discoveries had been made, Mr. Lockyer would have succeeded in recognising the bright lines of the prominences, is one of those questions which never can be answered. For

---

* 'Era già gran tempo che avevamo intenzione di esplorare l' orlo del sole con lo spettroscopio, ma l' assicurazione data da molti abili osservatori e dal Sig. Lockyer stesso, che nulla vedevasi di più all' orlo del disco che al centro, ce ne avea distolto.'—*Memoria III. sugli spettri prismatici de' corpi celesti.*

my own part, I cannot but think that he would assuredly have succeeded in the long run, even if the Indian expedition had been a failure. My reason for believing this is that Mr. Browning had so successfully mastered the optical conditions of the problem as to ensure complete success on the observer's part, whenever the examination of the Sun's limb should be thoroughly undertaken. However, owing to one cause and another, there ensued considerable delay before Mr. Lockyer attempted to make the required observations; and in the meantime news came to England that the spectrum of the prominences included three conspicuous bright lines. This placed it out of Mr. Lockyer's power to *discover* the lines; but it was still left for him to show that they can be seen when the Sun is not eclipsed. Two months after the eclipse he announced that he had so seen them; and the story runs that some five minutes or so before, the announcement of Janssen's prior success had been received by the President of the Imperial Academy at Paris, a communication from Mr. Lockyer to a similar effect had been read to that learned body.*

---

* M. Faye, while admitting Mr. Lockyer's claim to the independent recognition of the visibility of the lines without eclipse, expresses a feeling of regret that the mere distance between India and Europe should have prevented France—as represented by M. Janssen—from enjoying the full credit of the discovery. There is something almost childish in this mode of viewing the matter. How M. Janssen's credit can be diminished because Mr. Lockyer independently effected the same observation it would be hard indeed to say. But this is not all. It seems to us that nothing can be more mischievous to science than the promulgation of the idea that mere priority in making and announcing an observation is to afford the chief measure of the credit to be assigned

Soon after Secchi successfully applied the same method, as did Dr. Huggins, whose spectroscope was, however, not altogether well adapted to the work, having been specially designed for stellar researches.

It was a peculiarity of the new mode of observing the prominence-spectrum that it enabled the observer to determine at his leisure, and in a much more satisfactory manner than had been possible during the eclipse of 1868, the true position of the prominence-lines, their characteristics as respects shape (the significance of which feature has been already referred to, p. 146), and also the existence of lines which had escaped observation while the eclipse was in progress. It is

---

to the observer. If this were so, the wisest (though most selfish) course which the students of science could follow would be to conceal all original ideas as to modes of observation or processes of research until they could apply those ideas on their own account. Now, the true lover of science cannot do this. He feels bound to publish any such ideas at once, let who will adopt and utilise them (and let who will obtain credit for the result). There may be cleverness in a more diplomatic course, but there is no true love of science.

I may add that as regards the question of priority in this matter more importance might be attached to it were it not well known that after Dr. Huggins's observations of T Coronæ, the principle on which the new mode of observation rests was recognised by all who understand spectroscopic analysis. Like the discovery of Sun-spots, therefore, this recognition of the prominence-lines in full daylight was an inevitable sequel of the construction of an adequately dispersive spectroscopic battery. This was, however, a work requiring a degree of skill on the part of the optician who was to devise the instrument which should cause us to assign no inconsiderable share of the credit to him. M. Janssen, I believe, devised the principle of the battery he employed. The battery employed by Mr. Lockyer was constructed, as already mentioned, by Mr. Browning, the inventor of the automatic spectroscope. Mr. Lockyer tells us that it so perfectly fulfils the required conditions that the least experienced observer can see the prominence-lines.

obvious that the spectrum of the Sun's limb seen at the same time (or that of the illuminated terrestrial atmosphere) affords the most satisfactory means of determining the position of the bright prominence-lines; for *there* in the solar spectrum are those very lines—the dark lines C and D and F, and so on—with which the eclipse observers had associated the bright lines. It is only necessary to see whether those dark lines coincide in position with the bright ones (see figs. 36, 37, and 38, in which, however, but a few dark lines are shown) in order to tell whether the bright prominence-lines are or are not due to the presence of those particular elements to which the solar dark lines belong.

Fig. 71.

Janssen found in this way that the orange line of the prominences does not correspond, as had been thought, with the D or sodium (double) line of the solar spectrum. He found, however, that the red and blue prominence-lines do actually coincide with the C and F lines of hydrogen. So that these enormous objects, extending in some instances to a height of more than 80,000 miles above the Sun's surface, consist in part, at least, of the glowing vapour of hydrogen.

But it was possible to recognise other lines besides these three by the new method, when an instrument of adequate dispersive power was employed. The accompanying picture (fig. 72), for example, exhibits the spectrum of a prominence, and of the adjacent portion of the Sun's limb, as shown by Mr. Lockyer's spectroscope. It will be seen that the double line of sodium as well as three lines of the magnesium spectrum are shown. It is only necessary to suppose that M. Rayet saw the two close double lines as single ones in order to account exactly for the nine lines seen by him.*

---

* The following is Fr. Secchi's description of the principal lines seen in the prominence-spectrum under ordinary conditions :—

'The line c of hydrogen,' he says, 'is the most easily seen of all. It sometimes reaches the enormous height of three minutes, indicating the presence of such colossal prominences as are seen during eclipses. On the limb of the Sun generally the height of this line is very irregular, and on the average attains to from ten to fifteen seconds. This line also extends in a well-marked manner within the limb, overlapping the disc by ten seconds and more. Yet further on the disc there is a region where the line cannot be seen, being neither bright nor dark, but of the same tint exactly as the neighbouring part of the spectrum, which is thus in this part of its length continuous and uniformly bright. With a slit placed parallel to the limb the space throughout which the line disappears is often considerably extended. Outside the disc the line is much brighter near its base than at the summit, and the line is dilated at the base, and seems to terminate in a point where its light fades off, until, as I have already said, it becomes of the same brightness as the neighbouring part of the spectrum before becoming a dark line. Outside the Sun the line is bounded by two dark lines, which appear at first sight to be the effect of contrast, but may probably have another and a real cause. The bright line is often formed of knots and separate pieces, which are evidently so many fragments of different prominences, placed one beyond the other and unequally bright. If the aperture of the slit be enlarged as much as is possible without rendering the light unbearable, the bright line of the rose-coloured fringe is seen to be marked by irregularities which are due to the roughness of texture (*scabrosità*) of the prominences themselves.'

Mr. Lockyer was presently able to confirm the views of Grant, Secchi, and others, who had, as

After mentioning that he has been able to apply Janssen's method of determining the shape of the prominences by taking line-sections, Secchi remarks that he has often tried to determine whether there is any fixed law as regards the direction in which the prominences are bent, but has hitherto not succeeded in tracing any.

'The line in the yellow, near the sodium lines, is about twice and a half as far from the nearest of these as these are from each other. This line is sensibly the prolongation of a bright line in the solar spectrum. In height and brightness it corresponds closely to the C-line, but I have noticed that it will not bear high magnifying power so well. While the F and C lines remain brilliant under such powers, this line becomes weaker, so that only the practised eye can detect it. The line ends in a point, and it often extends itself brilliantly upon the disc of the Sun. Outside the disc it is not bordered by dark lines; on the contrary, with high powers it becomes diffused, and does not stand out sharply like the C-line.

'The F-line is in general not so high as the C-line, and grows faint at the extremity, where it takes the form of a lance. Outside the disc it is accompanied by a narrow dark zone on the more refrangible side. Sometimes I have seen it prolonged beyond the edge of the solar disc, as in the case of the C-line; at other times a very fine black thread shows itself on the more refrangible side. This seems to show that the F-line is not due to hydrogen alone.' (This conclusion, however, has been negatived.)

'The third line of hydrogen near G I have seen as a bright line, but it is necessary to reduce the dispersive power of the prismatic battery in order to obtain sufficient light.'

Secchi is of opinion that where there are faculæ on or close to the limb, there prominences exist; but he is unable to say with equal confidence that wherever prominences exist there are also faculæ.

He was surprised to find on one occasion, when several lines besides the above-mentioned were visible, that of the three lines forming the group *b* of magnesium one and one only was visible,—a second line holding a position midway between the other two lines of the same group. He remarks that Rayet had observed during the eclipse of August 1868 only two lines of this group, and that these two doubtless corresponded with those seen by himself, of which, as we have seen, one does not accord with either of the two remaining magnesium lines. He was so surprised at this peculiarity that he searched dilialready mentioned, enunciated the theory that the Sun is wholly surrounded by a layer or envelope of the coloured matter whence as it would seem the prominences spring. To whatever part of the Sun's edge he directed his spectroscope the bright lines belonging to the prominence-spectrum could be seen, though reaching but to a short distance from the edge of the Sun, save where there is a prominence. Unaware * that this

---

gently for two hours to detect traces of the other lines of magnesium, but could find none whatever. He considers the objections which have been urged against this observation as 'inconclusive—to use no more severe expression—since it is unreasonable to suggest that he would have been careless when observing so remarkable a phenomenon.'

* In Mr. Lockyer's detailed account of his work (*Report of the Astronomical Society*, February 1869) he states that 'at the time the spectroscope revealed to him that the prominence-spectrum was never absent, and that, in fact, the prominence-matter formed a continuous envelope round the Sun, he was not aware that such an envelope had been suggested by previous observers.' The account goes on to speak of the physicists who had propounded this theory—Swann, Grant, Von Littrow, Leverrier, and Secchi—describing their views as 'ideas,' 'suggestions,' and 'surmises,' and adding that 'the experimental proof of the truth of these surmises is due to Mr. Lockyer.' It is bare justice to remark, however, that with some at least of the five physicists above-named the theory was much more than a surmise or conjecture. Thus Professor Grant writes, in 1858, 'The zone of a deep red colour observed at Toulon toward the part of the Moon's limb where the Sun was about to emerge, *clearly indicates* the accumulation of nebulous matter in the lower regions of the solar atmosphere, as well as the condensation of the circumambient fluid of which the latter is composed towards the surface of the Sun, arising from the pressure of the superincumbent strata.' Leverrier wrote in 1860, 'The existence of a bed of rose-coloured matter, partially transparent, covering the whole surface of the Sun, is *a fact established* by the observations made during the time of totality in the eclipse of this year.' Secchi was equally convinced of the existence of this rose-coloured region. 'The observation of eclipses,' he wrote, '*furnishes indisputable evidence* that the Sun is really surrounded by a layer of this red matter, of which we commonly see no more than the most elevated points.' If these be surmises, how shall men of science express conviction?

layer had already been discovered, Mr. Lockyer devised a name for it, entitling it the *chromosphere.*

Whether this envelope is to be regarded as a true solar atmosphere, in the sense in which that term is usually understood, may be seriously questioned. As seen in the telescope it presents a well-defined and very uneven limit, giving the idea rather of a region of flames or clouds than of an envelope of the nature of an atmosphere. That the substance producing the coloured light of the chromosphere is gaseous admits indeed of no question; but so also the prominences are gaseous. Yet we do not regard these as of the nature of an atmosphere. Now, if the surface of the Sun be covered at all times with small prominences, bearing somewhat the same relation to the gigantic 'horns' and 'boomerangs' seen during eclipses that the bushes covering certain forest regions bear to the trees, then there can be no doubt that the chromosphere could not rightly be regarded as an atmosphere. We have

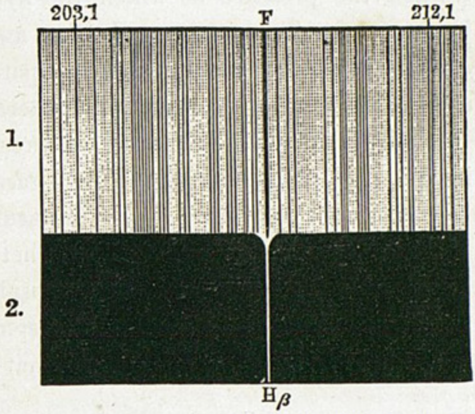
---

Leverrier and Secchi believed in the existence of this envelope because they had seen it; and Secchi had the further evidence (though no further evidence could be needed) derived from his photographs. Under these circumstances no subsequent observer can claim to have 'been the first to give experimental proof of the existence of the chromosphere.' Or if,—then Galileo might have claimed to have been the first to give experimental proof of the existence of the Moon or of Jupiter, since undoubtedly he first studied these bodies telescopically. That Mr. Lockyer *would* have discovered the chromosphere had it not been discovered before (unknown to him), may be well believed, precisely as we may be assured that had not the eclipse observers in 1868 discovered the nature of the prominence-spectrum, Mr. Lockyer would have done so. That he did not do either of these things is no discredit to his abilities, since no man that ever lived has succeeded in discovering the discovered; and it may be surmised that no man ever will.

no evidence that it even extends downwards as far as the visible limits of the photosphere, since undoubtedly if a distance of forty or fifty, nay, even of two or three hundred miles, separated the lower limit of the chromosphere from the photosphere, no telescopes we possess could suffice (when supplied with suitable spectroscopic appliances) to reveal any trace of this space. A width of two hundred miles at the Sun's distance subtends an arc of less than half a second; and telescopists who know the difficulty of separating a double star whose components lie so close as this will readily understand that a corresponding arc upon the Sun would be altogether unrecognisable.

I am very far, therefore, from accepting with confidence the view that the chromosphere is the true solar atmosphere. Rather I believe it consists of matter—gaseous, no doubt, but—suspended in the true solar atmosphere. I think that not only is this view confirmed by the appearance of the chromosphere as depicted in Plate VI., further on, but that the general resemblance as regards structure and appearance between the chromosphere and the prominences suffice to render it at least highly probable that the former can no more be described as the solar atmosphere than the latter. But even more striking is the evidence deduced from Plate V. For here we see in the first figure a prominence which has been obviously erupted. We see it as it springs upward, expanding with the diminishing atmospheric pressure, and we see in the second figure how the erupted matter has slowly begun to sink, with expansion, indicating low pressure in the upper regions, but with a figure indicating a real resistance. It is then this invisible atmosphere, through which the upper portion of the prominence has begun to subside, that would seem to constitute the true solar atmosphere, while the chromosphere should be regarded as consisting of a multitude of smaller prominences, either freshly erupted or the remains of former eruptions, floating in the lower regions of the solar atmosphere, much as the second prominence of Plate V. is floating (for a brief while) in the upper regions.

Fig. 72.



Illustrating the widening of the F-line of hydrogen near the base of the chromosphere.

The inferences which have been deduced respecting the pressure exerted by the solar atmosphere at and near the level of the photosphere remain unchanged, however, whatever view we adopt on this point. Fig. 72 illustrates the evidence on this point. Here 1 is a part of the spectrum of the Sun's limb, while 2 represents the line F in the spectrum of the chromosphere. The widening of this line close by the Sun's limb may be regarded as unquestionably indicating an increase of pressure, because the researches of Plücker, Hittorf, Huggins, and Frankland have demonstrated that the F-line of hydrogen does actually increase in this way in width when the pressure at which the hydrogen subsists is increased. Temperature, also, has an effect on the hydrogen lines; nor is it quite easy to separate the effects due to pressure from those due to temperature.* But, on the whole, it seems probable that pressure is chiefly in question, while it may be regarded as absolutely certain that temperature alone is insufficient to account for the observed change. Now, whether we regard the glowing hydrogen of the chromosphere as forming a true constituent of the solar atmosphere or as bearing a somewhat similar relation to that atmo-

---

* In a paper by Dr. Zöllner (*Über die Temperatur und physische Beschaffenheit der Sonne*), an abstract of which, by the present writer, appeared in the *English Mechanic* for September 1870, it is suggested that means might be devised for distinguishing between the effects of pressure and temperature by causing a discharge of gas to take place at the moment when the induction spark is passed through the hydrogen. It is certainly a problem of the utmost importance thus to distinguish between the two effects which play the chief part in solar phenomena.

The disappearance of one of the magnesium lines at a certain height above the spectrum of the limb (see fig. 71) in which the lines B belong to magnesium, is significant of a very low degree of pressure at the level where the shortest line ceases to be visible.

sphere that the aqueous vapour in our own air bears to the permanent constituents, it is yet certain that the pressure of the hydrogen near the solar photosphere is a measure of the atmospheric pressure there. And from the observed width of the F-line near the Sun's limb (see fig. 72) it has been estimated by Wüllner that 'the pressure on the base of the chromosphere, or at the surface of the photosphere, is below the pressure of the Earth's atmosphere.* He has even assigned the limits of pressure at the level of the solar photosphere as lying certainly 'between 50 and 500 millimetres (or between 2 inches and 20) of a mercurial barometer at the Earth's surface.'

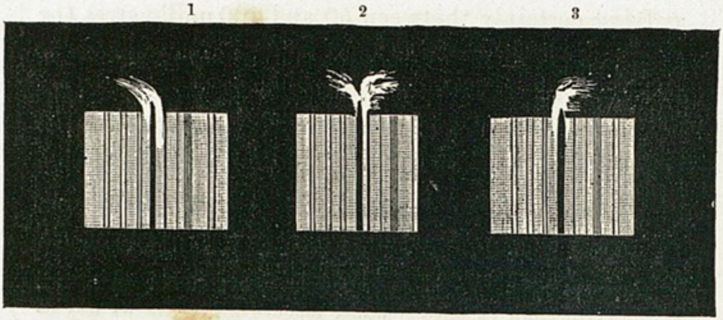
It is within the chromosphere and certain of the prominences that spectroscopes of high dispersive power exhibit those signs of cyclonic motions taking

---

* It must not be forgotten, however, that the width of the hydrogen-line where it actually reaches the spectrum of the limb is not known. The observed width on which Wüllner founded his researches may be that corresponding to a height of 50, 100, or even 200 miles above the photosphere; and within these 50, or 100, or 200 miles an increase of pressure may take place by which the actual density of the atmosphere close by the photosphere may be enormously increased. There may be an atmosphere including the vapours of iron, sodium, magnesium, &c. (of all the elements, in fine, whose dark lines appear in the solar spectrum) extending, say, 100 miles above the photosphere; and yet no instruments we possess could suffice to reveal any trace of its existence, *unless the dark lines in the solar spectrum be thought to demonstrate the fact that such an atmosphere actually does exist.* The fact that on some occasions Mr. Lockyer has seen hundreds of the Fraünhofer lines (as bright lines) in the spectrum of the chromosphere renders this far from improbable. The arguments on the strength of which it has been assumed that the absorption to which the dark lines are due takes place below the visible photosphere, appear, to say the least, far from demonstrative.

place in the solar atmosphere the recognition of which has seemed so surprising and inexplicable. The study of the last part of Chapter III. will have shown the reader that if very rapid motions are taking place, either due to the swift rush of the glowing hydrogen through the solar atmosphere or to the effects of cyclonic motions in the atmosphere itself, by which the glowing hydrogen is borne away, the spectroscope—if only its dispersive power be sufficient—cannot fail to give unmistakable evidence on the point. The problem is altogether simpler and easier indeed than that which Dr. Huggins had to deal with when he undertook to measure the velocity with which Sirius is winging its flight through space. For *there* in the solar spectrum are the very lines of hydrogen with which the chromosphere lines are to be compared. If the spectroscopic dispersion suffice, the least experienced observer can

Fig. 73.



Illustrating the spectroscopic indications of solar cyclonic action.

tell as certainly that a solar storm is in progress as the terrestrial observer can tell by the motions of cirrus clouds that the upper regions of our own atmosphere are disturbed. The accompanying illustration (fig. 73), for instance, shows how the F-line in the chromosphere-spectrum is at times swayed (as it were) from coincidence with the dark F-line of the ordinary solar spectrum. At 1 we see the line deflected towards the violet, showing that the portion of the chromosphere under examination was moving rapidly towards the observer; at 2 we see a deflection both towards the red and towards the violet, indicating that in the same field of view (that is, in the portion of the chromosphere included within the slit) there were masses moving towards as well as from the eye; while, lastly, at 3 we see a deflection towards the red, indicating a rapid motion from the eye. During some observations such as these, Mr. Lockyer has had evidence of motions at the almost inconceivable velocity of 120 miles per second. I cannot, however, accept his conclusions as to the distribution of the motions over the Sun's surface, because he seems to take very little account of what certainly is the fact, that the extension of the portion of chromosphere under examination is very great in the direction of the line of sight. Assuming an apparent depth of only ten seconds (which is within the usually observed limits), a tangent-line to the Sun's surface passes through a range of upwards of 60,000 miles; and to speak of the clear recognition of a solar cyclonic storm only 1,500 miles in diameter (as described by Mr. Lockyer) when the visual ray passes through a depth forty times as great, seems to me wholly inadmissible. We have no evidence that the portions of the chromosphere giving the three displacements shown in fig. 73 may not have lain 20,000, 30,000, or even 40,000 miles apart, in the direction of the line of view;* in other words, that the observed solar storms, though undoubtedly raging with amazing fierceness, were necessarily cyclonic in character. This remark does not apply with equal force to the evidence deduced from the appearance of the prominence F-line depicted in fig. 74,† because we cannot consider it likely that two prominences of the enormous height indicated by the extension of the shattered F lines from the spectrum of the limb, would lie along the same visual line at the moment of observation.

Professor Young, of America, has, however, noted a remarkable circumstance which seems in some sense to throw doubt on the inferences which have been deduced from the displacement of the F-line.

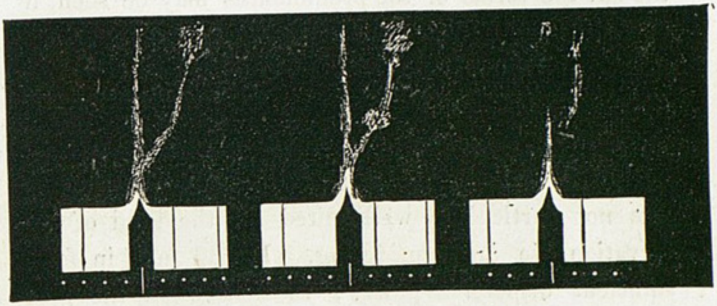
---

[*] Throughout Mr. Lockyer's papers there is to be noted a mode of explaining the observed phenomena which seems to imply that the spectroscope exhibits to us the condition of a mere slice of the solar envelopes. The effect of the Sun's solidity in giving an enormous extension to the visual lines through the chromosphere seems wholly neglected, even where a reference to the breadth of the region really included within the slit would appear to invite a reference to the other form of extension.

[†] In this figure the dots below the spectra indicate the amount of displacement on either side of the normal position of the F-line, corresponding to a velocity of 8, 16, and 28 German geopraphical miles per second (a German geographical mile being equal in length to a fifteenth part of a degree of the Earth's equator—*i. e.*, to about $4\frac{3}{5}$ English miles).

He observed on one occasion that while the F-line of hydrogen in a prominence was absolutely *shattered*, the C-line of the same element presented its ordinary appearance. It seems absolutely essential to the interpretation hitherto placed on the displacement of *one* line of hydrogen that the other line should exhibit a similar displacement, differing only to a slight degree as regards extent. If the observation should be confirmed that the C and F lines of hydrogen in the same prominence-spectrum behave differently, a most perplexing problem will be presented not only to astronomers, but to physicists.

FIG. 74.



Illustrating the spectroscopic indications of rapid motions in tall coloured prominences.

We have now, however, to turn to a yet more interesting and valuable series of researches, by which astronomers have been able to observe the exact figure and changes of figure of the prominences. The principle on which these researches, first successfully pursued by Dr. Huggins, have been based, have been already explained towards the close of Chapter III. The visibility of the prominence-lines depends on the dispersive power of the spectroscope; and, clearly, the narrower the opening of the slit the more effective will this dispersion be. The lines of the prominences are thus, indeed, narrowed, but they do not diminish in brightness; whereas the spectrum of the illuminated atmosphere diminishes in brightness in precise proportion to the narrowness of the slit. But if we widen the slit the forms of the prominences may be seen, if only the brightness of the atmospheric spectrum is not too greatly increased. In order, therefore, that this feat may be achieved, we must have a spectroscope of great dispersive power.

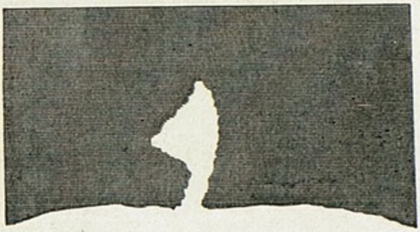
Now, I have said that Dr. Huggins's spectroscope was not particularly well suited for the kind of observation we are considering. It had not, in fact, sufficient dispersive power; so that when the idea occurred to him of seeking for the prominences with an open slit, he had no great hope of succeeding. As a matter of fact, he failed. The increased light blotted out the prominences altogether.

But he was not so to be foiled. He who had shown astronomical spectroscopists the way to success in the beginning, was now—with inadequate means—to show his pupils (if one may so speak) how to study the prominences to new purpose. Supplementing the powers of his spectroscope by the use of coloured glass, he was able to solve the great problem which was the true end of all the observations hitherto made. Others were aiming at the solution of the problem. Lieut. Herschel had suggested the use of coloured media admitting only such rays as the prominences emit. Mr. Lockyer was trying I know not what device of rotating or vibrating slits,—

> And one had aimed an arrow fair,
>   But sent it slackly from the string;
>   And one had pierced an outer ring
> And one an inner here and there;
> But last the master bowman, he
>   Had cleft the mark.

Fig. 75 represents the picture—rough, but instructive—of the first solar prominence ever *seen* when the Sun was not eclipsed.

FIG. 75.



The first prominence seen by means of the spectroscope. (*Huggins.*)

So soon as the open-slit method had been suggested by Huggins many other observers adopted it. Mr. Lockyer, availing himself of the great dispersive power of the instrument Mr. Browning had made for him, found that he could dispense with the use of coloured glasses. The prominences were rendered distinctly visible with the open slit alone; and he could readily watch the changing figures of these mysterious objects. The accompanying drawings (figs. 76 and 77) exhibit two views of a wild and fantastic group of prominences, drawn by Mr. Lockyer, the second only ten minutes after the first was completed.

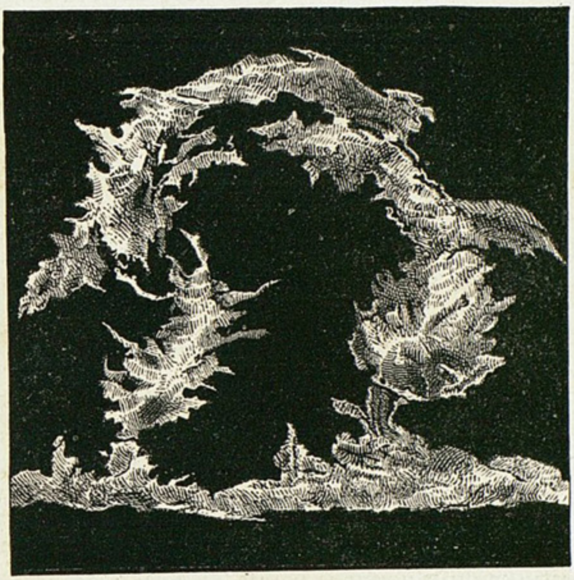
Fig. 76.

A group of solar prominences.—March 14, 1869, 11h. 5m. (*Lockyer.*)

Dr. Zöllner, the eminent German photometrician, applied the same method in a systematic manner. Some of the pictures he has published are singularly interesting.

It is to be noticed, in the first place, that Dr. Zöllner observed the same protuberance in three different colours, corresponding to the three lines of its spectrum. He found a material difference between the red and the blue image on the one hand and the yellow on the other. The latter is very intense only in close proximity to the Sun's limb,[*] and corresponds there to the other images; but the more delicate details disappear at a greater distance. Zöllner suggests that

FIG. 77.



The same group ten minutes later.

---

[*] It will be observed that Zöllner's results as regards the coloured images correspond with those obtained by Secchi in observing the coloured *lines*.

this may be explained on the hypothesis that 'the rays which give rise to the yellow image emanate from a gas having a greater specific gravity than hydrogen, and therefore existing at a lower level.'

The most interesting of Zöllner's drawings are exhibited in Plates IV. and V. It must be conceived by the reader that these views are only the red images. Zöllner saw perfectly similar blue images, and nearly similar orange-yellow images. The combination of the three produces the complete image as seen, during eclipse, with the telescope.

Respecting the several protuberances shown in Plate IV., he makes the following remarks:—'In fig. 1 we see an intensely luminous peak-shaped mass rising from the border of the Sun, above which is spread a cloud-like formation of less intensity. To the same type belong the protuberances of figs. 4 and 9. In fig. 4 it is remarkable that the surprisingly beautiful cumulus shape of the cloud was separated from the peak by a considerable distance. The cloud was exceedingly delicate, and even its finest details could be recognised. The separate cumuli of which it was composed appeared almost like dull luminous points.'

Fig. 2 was one of the most remarkable formations. 'I hardly believed my eyes,' says Zöllner, 'when I noticed in it the tongue-like motion of a flame. This motion was slower, however, compared with the size of the flame, than that of high towering flames at great conflagrations. The time required by such a wave in passing from the base to the apex was about two or

July 1, $3^h\ 20^m$

Fig 4

July 1 $3^h\ 45^m$ July 1 $6^h\ 45^m$

Fig. 5

$6^h\ 55^m$ $6^h\ 57^m$ $7^h\ 0^m$ $7^h\ 4^m$ $7^h\ 8^m$

Fig. 6

July $2^d\ 11^h\ 15^m$

Fig 7

July 4 $9^h\ 0^m$

Fig 8

July 4 [illegible]

$11^h\ 50^m$ [illegible]

Fig 10.

this may be explained on the hypothesis that 'the rays which give rise to the yellow image emanate from a gas having a greater specific gravity than hydrogen, and therefore existing at a lower level.'

The most interesting of Zöllner's drawings are exhibited in Plates IV. and V. It must be conceived by the reader that these views are only the red images. Zöllner saw perfectly similar blue images, and nearly similar orange-yellow images. The combination of the three produces the complete image as seen, during eclipse, with the telescope.

Respecting the several protuberances shown in Plate IV., he makes the following remarks:—'In fig. 1 we see an intensely luminous peak-shaped mass rising from the border of the Sun, above which is spread a cloud-like formation of less intensity. To the same type belong the protuberances of figs. 4 and 9. In fig. 4 it is remarkable that the surprisingly beautiful cumulus shape of the cloud was separated from the peak by a considerable distance. The cloud was exceedingly delicate, and even its finest details could be recognised. The separate cumuli of which it was composed appeared almost like dull luminous points.'

Fig. 3 was one of the most remarkable formations. 'I hardly believed my eyes,' says Zöllner, 'when I noticed in it the tongue-like motion of a flame. This motion was slower, however, compared with the size of the flame, than that of high towering flames at great conflagrations. The time required by such a wave in passing from the base to the apex was about two or

IV.

Fig. 1.

July 1. 3$^{h}$ 20$^{m}$

Fig. 2.

July 1. 3$^{h}$ 45$^{m}$

Fig. 3.

July 1. 6$^{h}$ 45$^{m}$ 6$^{h}$ 55$^{m}$ 6$^{h}$ 57$^{m}$ 7$^{h}$ 0$^{m}$ 7$^{h}$ 4$^{m}$ 7$^{h}$ 8$^{m}$

Fig. 4.

July 2$^{nd}$ 11$^{h}$ 3.5$^{m}$

Fig. 5.

July 4. 9$^{h}$ 0$^{m}$

Fig. 6.

July 4. 11$^{h}$ 40$^{m}$ 11$^{h}$ 47$^{m}$ 11$^{h}$ 50$^{m}$ 12$^{h}$ 50$^{m}$

Fig. 7.

July 4. 5$^{h}$ 30$^{m}$

Fig. 8.

July 4. 5$^{h}$ 45$^{m}$

Fig. 9.

July 4. 6$^{h}$ 30$^{m}$

Fig. 10.

July 4. 7$^{h}$ 10$^{m}$ 7$^{h}$ 20$^{m}$

SOLAR PROMINENCES OBSERVED BY D$^{R}$ ZÖLLNER IN 1869.

three seconds. I endeavoured on the following days to verify this observation by the discovery of similar formations, but I have not succeeded, in spite of my arduous and continued search. I therefore request that this statement may be considered as one requiring further proof.'

In figs. 3, 6, and 10 we have remarkable illustrations of the great rapidity with which the protuberances change both in form and brilliancy. In these figures the different shapes are represented which the same protuberance assumed after the intervals of time indicated by the hours and minutes (Leipsic time) named underneath them. The heights range from 30 to 120 seconds, and when it is remembered that a second of arc at the Sun's distance corresponds to a distance of no less than 450 miles, it will be seen on how enormous a scale these prominences are formed.

Zöllner, even at this early stage of his inquiries, was led to arrange the prominences into two distinct classes —the cloud-like and the eruptive. Respecting objects of the former class, he remarks that they remind him of the different forms of our clouds and fogs. 'The cumulus type is completely developed in some of the figures. Other formations remind one of masses of clouds and fogs floating closely over lowlands and seas, whose upper parts are driven and torn by currents of air, and which present the ever-varying forms so well known, when viewed from the tops of high mountains.' An important observation was made by Zöllner on June 27, 1869. 'On this day,' he says—'the first clear day after a long spell of cloudy weather—I observed the bright protuberance-lines, without however being able at that time to make a complete observation of these formations. As soon as I approached the slit of the spectroscope to a certain position in the Sun's limb, where the protuberance-lines appeared particularly long and bright, brilliant linear flashes passed through the whole length of the dull spectrum *over* the limb of the Sun, about three or four minutes distant from the latter. These flashes passed over the whole of the spectrum in the field of view, and became so intense at a certain point of the Sun's limb as to produce the impression of a series of electrical discharges rapidly succeeding one another and passing through the whole of the spectrum in straight lines. Mr. Vogel, who afterwards for a short time took part in these observations, found the same phenomenon at a different portion of the Sun's limb, where protuberance-lines also appeared. 'The phenomenon can be explained,' adds Zöllner, 'by the hypothesis that small, intensely incandescent bodies, moving near the surface of the Sun, emit rays of all degrees of refrangibility, and produce flashes of a thread-like spectrum as their image passes before the slit of the spectroscope.'*

---

* We must not, however, forget the possibility that these objects may have been insects in the air at no very considerable distance from the observer. Lieut. Herschel was for some time misled by a similar phenomenon, but, after long watching, one of the bright objects suddenly stopped and flew off in a new direction, and he then recognised the fact that he had been intently observing with the telescope, and carefully

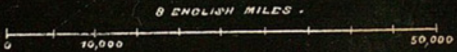
V

# ERUPTION PROMINENCES.

observed by Zöllner.

1.

2.



1869 August 29.

*Fig. 1.*
A PROMINENCE OBSERVED
AT $10^h\ 22^m$

*Fig. 2.*
THE SAME PROMINENCE
AT $11^h\ 20^m$

Plate V. illustrates a later paper of Zöllner's, and the most cursory study will serve to show that he observed the prominence there exhibited under exceptionally favourable conditions. Nothing can be more striking than the aspect of the prominence shown in these two drawings. It is obvious that Zöllner had here been regarding the action of solar eruptive forces casting forth enormous masses of glowing hydrogen gas.* The evidence on this point has already been referred to. It seems impossible to conceive that any evidence could be more convincing.

But we owe to Professor Respighi the most systematic and successful attempts yet made to deal with the solar prominences. He has in fact demonstrated the possibility of applying to the prominences sys-

---

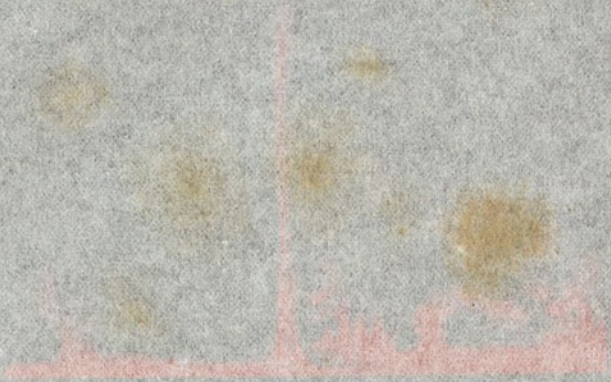
observing with the spectroscope, *a flight of locusts!* These objects were very much more conspicuous than those seen by Zöllner, however; and the observation made by Mr. W. S. Gilman, jun., during the American eclipse, that in the heart of a large prominence (G of fig. 78, p. 311) there were exceedingly bright red points, seems to confirm the justice of Zöllner's assertion.

* Zöllner remarks that the prevalence of one or other of the characteristic forms into which prominences may be divided—viz., the cloud form and the eruptive form—appears to depend partly on their local distribution over the solar surface, partly on the time of observation; so much so that at a given time one form, at another the other, may be the prevailing type. 'That the cloud-formed prominences present so marked a resemblance to terrestrial cloud and smoke, is easily explained,' he says, 'when we recollect that the forms of our clouds must be supposed to depend, not on the vesicles of water suspended in them, but essentially only on the mode in which the air-masses, variously warmed and moved, expand. The water-vesicles in terrestrial clouds form but the material by which such differences of temperature and motion are rendered visible to us. In the clouds of the protuberances this visibility is brought about by the glow of the luminous hydrogen masses.'

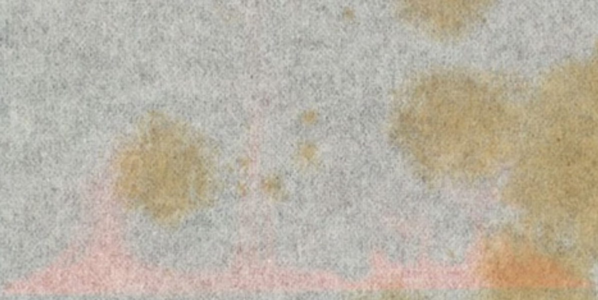
## ERUPTION PROMINENCES

observed by Zöllner

1



2



---

1869 August 29

*Fig 1*
A PROMINENCE OBSERVED
AT 10h 22m

*Fig 2*
THE SAME PROMINENCE
AT 11h 20m

Plate V. illustrates a later paper of Zöllner's, and the most cursory study will serve to show that he observed the prominence there exhibited under exceptionally favourable conditions. Nothing can be more striking than the aspect of the prominence shown in these two drawings. It is obvious that Zöllner had here been regarding the action of solar eruptive forces casting forth enormous masses of glowing hydrogen gas.* The evidence on this point has already been referred to. It seems impossible to conceive that any evidence could be more convincing.

But we owe to Professor Respighi the most systematic and successful attempts yet made to deal with the solar prominences. He has in fact demonstrated the possibility of applying to the prominences sys-

---

analysing with the spectroscope, *a flight of locusts!* These objects were much more conspicuous than those seen by Zöllner, however; and the observation made by Mr. W. S. Gilman, jun., during the American eclipse, that in the heart of a large prominence (G of fig. 78, p. 311) there were exceedingly bright red points, seems to confirm the justice of Zöllner's conclusion.

* Zöllner remarks that the prevalence of one or other of the characteristic forms into which prominences may be divided—viz., the cloud form and the eruptive form—appears to depend partly on their local distribution over the solar surface, partly on the time of observation; insomuch that at a given time one form, at another the other, may be the prevailing type. 'That the cloud-formed prominences present so lively a resemblance to terrestrial cloud and smoke, is easily explained,' he adds, 'when we recollect that the forms of our clouds must be supposed to depend, not on the vesicles of water suspended in them, but (essentially) only on the mode in which the air-masses, variously warmed and moved, expand. The water-vesicles in terrestrial clouds form but the material by which such differences of temperature and motion are rendered visible to us. In the clouds of the protuberances this visibility is brought about by the glow of the luminous hydrogen masses.'

tematic observations comparable with those applied by Schwabe and Carrington to the spots. On each of the days, 7, 12–15, 17, 18, 21, 24, 26–29, and 31 of January 1869, he took views round the solar limb, until he had completed the circuit, ranging the observed prominences in a straight line (the prominences being presented, for convenience, on a scale twice that of the chromosphere's horizontal extension). The result is shown in Plate VI. We have only to suppose each horizontal row of coloured prominences bent round a disc representing the Sun, and the prominences reduced to one-half their present dimensions, in order to have a complete picture of the Sun's condition on any of the days illustrated in the plate. It cannot be but that the application of Respighi's system throughout a long series of years will result in revealing laws in the number and magnitude of prominences at different times and in different solar latitudes. Such laws, once clearly recognised, will probably serve to indicate relations, as yet wholly unthought of, between the prominences, the phenomena of the photosphere, the magnetic relations of the planetary scheme, and many other periodic phenomena.

Leaving the reader to study at his leisure the singularly interesting features shown in Plate VI., I will briefly indicate certain general conclusions to which Professor Respighi's researches have led him.

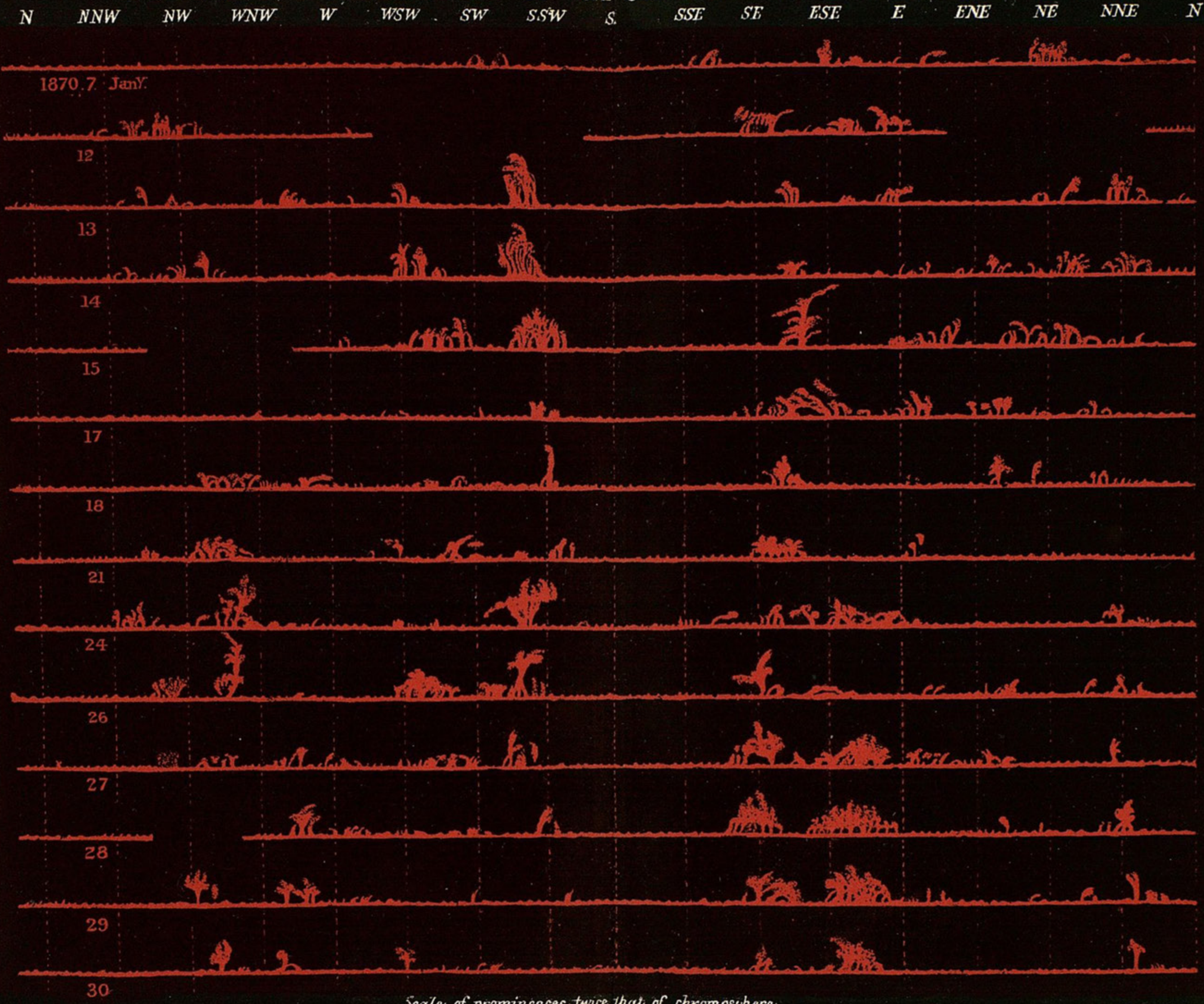
He considers that the prominences, at least as regards their origin, bear no analogy whatever to terrestrial clouds, but are strictly phenomena of eruption.* He

---

* This view is not altogether opposed to Dr. Zöllner's, who regards

VI

# SPECTROSCOPIC OBSERVATIONS OF THE SUN'S BORDER. BY PROFR. RESPICHI.



*Scale of prominences twice that of chromosphere.*

M & N. Hanhart lith.

has noticed that where there are faculæ there are usually prominences, but he considers that the prominences are certainly not identical with the faculæ. Over Sun-spots there are low jets, but no high prominences. In the circumpolar regions great prominences are never recognised, and the prominences actually seen, besides being small, are few in number, and last but a short time. At the solar equator, also, the prominences are less frequent, less active, and less developed than in higher solar latitudes. He noticed some prominences exceeding three minutes, or ten terrestrial diameters, in altitude; and one prominence observed by him had an elevation of no less than twenty terrestrial diameters, or about 160,000 miles. He found that the formation of a prominence is usually preceded by the appearance of a rectilinear jet, either vertical or oblique, and very bright and well defined. This jet rising to a great height, is seen to bend back again, falling upon the Sun like the jets of our fountains, and presently the sinking matter is seen to assume the shape of gigantic trees more or less rich in branches and foliage. Gradually the whole sinks down upon the Sun, sometimes forming isolated clouds before reaching the solar surface. It is in the upper portions of such prominences that the most remarkable and rapid transformations are witnessed; but a great difference is observed in the rate with which prominences change in figure.

---

the two types into which he divides prominences as different forms of the same objects, and, therefore (necessarily), the cloud form as the sequel of the eruptive.

Their duration, too, is very variable. Some develope and disappear in a few minutes, while others remain visible for many successive days. He considers that the sharply-defined bases of the eruptive jets prove that the eruption takes place through some compact substance forming a species of solar crust.* He agrees with Zöllner in considering that the enormous velocity with which these gaseous masses rush through the solar atmosphere implies that the latter is of exceeding tenuity. His conclusion that repulsive forces must balance the solar gravity, is not shared, however, by Zöllner, who prefers to regard the forces at work in producing the prominences as strictly analogous to those which produce eruptive action on our Earth.†

I had proposed to give a somewhat full account of the observations made by the American astronomers upon the solar prominences visible during the total solar eclipse of August 1869. But so much space has already been occupied by the subject of this chapter that I prefer to introduce only those narratives of the American observers which relate to the corona. These will be found in the

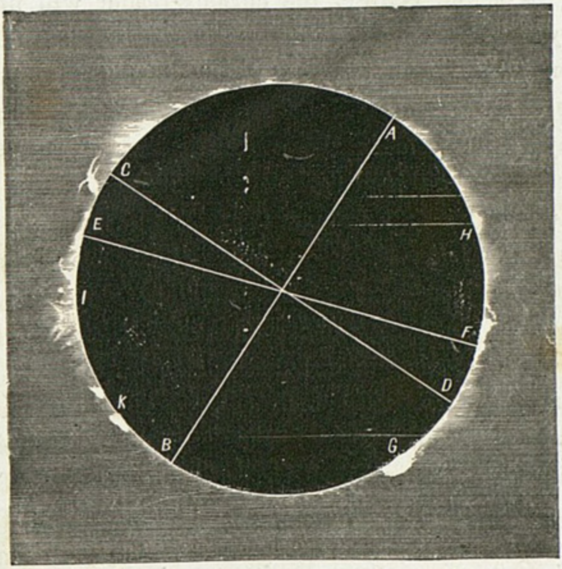
---

* This corresponds to the *Trennungschicht* of Zöllner's theory, who, however, does not agree with Respighi in regarding some form of electric action as the probable cause of these eruptions.

† It seems conceivable that the phenomena of geysers may have their counterpart in these solar prominences. It is true that a gaseous, not a liquid, mass is erupted; so that there is here nothing comparable to the rapid change of a column of liquid into vapour which causes the outbursts of geysers. But the temperature and pressure at which dissociation occurs may bear the same relation to the outbursts of these gaseous masses that the temperature and pressure at which water boils, bear, in Bunsen's theory of geysers, to the occurrence of geyser eruptions.

following chapter. The accompanying drawing (fig. 78) exhibits the prominences photographed by the American astronomers, the Moon's disc being reduced so as to permit all the prominences to be visible at once. The line F E indicates the course of the Moon's centre across the Sun, A B being a declination-circle, and C D a declination-parallel. The most successful photographing party was that headed by Dr. Mayer, at Burlington. But the two photographs by Dr. Curtis possess a special value on account of their delicacy and the number of details they exhibit. He has

Fig. 78.



The eclipse of August 7, 1869. From photographs by the American astronomers.

shown that the sharply-defined outlines of prominences, and even of the coronal glare, seen in most photographs of eclipses, result from excessive development. He proves satisfactorily, also, that the encroachment of the prominence-bases on the black disc of the Moon is due to a similar cause.

In Dr. Curtis's contribution to Dr. Sand's elaborate report of the eclipse, the curious reader will find a very interesting investigation of the aspect of the prominence shown between E and C in fig. 78. He refers all its peculiarities of appearance to the action of a cyclonic storm in the upper regions of the solar atmosphere at this place.

It remains only to be noticed that, even while this work has been passing through the printers' hands, Professor Young, of America, has succeeded in obtaining a photograph of a prominence when the Sun has been shining in full splendour. Although the result is, he tells us, not remarkable as a presentation of a solar prominence, yet as indicating the possibility of applying photography to record the condition of the chromosphere and prominences from day to day, from month to month, and from year to year, it is full of promise. The time seems not far off when we shall be as familiar with the laws according to which these mysterious objects appear, develope, and disappear within the solar atmospheric envelope, as we have already become with the general laws affecting the behaviour of Sun-spots.

# CHAPTER VI.

## *THE CORONA AND ZODIACAL LIGHT.*

THE coloured prominences, as we have seen, are phenomena which have been recognised only in recent times. We have now to deal with a phenomenon which has been known to astronomers for a much longer period, but has proved more difficult of interpretation, and remains even at the present day not clearly understood.

It may fairly be believed that during the earliest total solar eclipses observed by mankind, the corona, or crown of glory, which surrounds the black disc of the Moon must have attracted attention. Yet the records of the recognition of this phenomenon are by no means so distinct as might have been expected. We do not find, indeed, in ancient works professedly treating of the phenomena of nature, any reference to the imposing appearance presented by the solar corona. The earliest allusion to the phenomenon is held by Professor Grant to be that passage in the Life of 'Apollonius' where Philostratus, speaking of the signs and wonders which occurred before the death of Domitian, says, 'In the heavens there appeared a prodigy of this nature;

a certain corona, resembling the Iris, surrounded the orb of the Sun, and obscured his light.' Taken alone, this passage would certainly not seem intended to describe the phenomena of a total eclipse; but as Philostratus afterwards remarks that the darkness was so great as to resemble night, we may assume with some confidence that a total solar eclipse had occurred.

Plutarch more distinctly describes the appearance actually presented by the corona when he endeavours to explain why the darkness during a total eclipse is not so great as that of night. 'Even though the Moon,' he says, 'should hide at any time the whole of the Sun, still the eclipse is deficient in duration as well as amplitude, for a peculiar effulgence is seen around the circumference which does not allow a deep and very intense shadow.'

I do not propose to record in full the observations which have been made upon the corona. To do so would occupy, indeed, much more space than can here be spared. Referring the reader who wishes for a more complete account of the earlier observations to Professor Grant's admirable 'History of the Physical Sciences,' I shall consider here those observations alone which tend to throw light on the nature of the corona.

Some of the earlier observers of total solar eclipses would seem to have been misled by the great brightness of the corona close by the Sun, and to have supposed that a ring of direct sunlight had remained uncovered.

We shall see presently that modern observers have also been struck with the brightness of the light close by the Sun; and it seems obvious that this bright light is to be regarded as wholly distinct from the light of the chromosphere, the redness of which is too marked to escape recognition. Remembering that before the invention of the telescope the corona was the only marked phenomenon to which observers were able to direct their attention, a certain weight attaches to their comments on the brightness near the Moon's disc.

Clavius having expressed his belief that the eclipse of 1567 was annular, Kepler was led to investigate the subject, and he proved that that eclipse must needs have been total. In 1605 he witnessed a total eclipse at Naples, and found, in the features it presented, the explanation of the remarks of Clavius. 'The whole body of the Sun,' he says, 'was completely covered for a short time, but around it there shone a brilliant light.' We might suppose that he referred to the chromosphere, because he says that the light was 'of a reddish hue;' but as he adds that it was 'of uniform breadth, and occupied a considerable part of the heavens,' there can be no doubt that he is speaking of the corona.

Dr. Wyberd gives a remarkable account of the appearance of the corona during the total eclipse of March 29, 1652. 'When the Sun was reduced to a narrow crescent of light,' he says, 'the Moon all at once threw herself within the margin of the solar disc with such agility that she seemed to revolve like an upper millstone, affording a pleasant spectacle of rotary motion. In reality, however, the Sun was totally eclipsed, and the appearance was due to a corona of light round the Moon, arising from some unknown cause. It had a uniform breadth of half a digit, or a third of a digit at least;* it emitted a bright and radiating light, and appeared concentric with the Sun and Moon' when the middle of totality was reached.

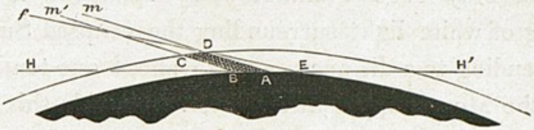
It need scarcely be remarked that the agility with which the Moon seemed to throw herself within the margin of the Sun's disc was merely apparent. It has been noticed by many observers that the total obscuration of the Sun seems to occur suddenly, the Moon covering the last sickle of sunlight, apparently at a leap. Irradiation is no doubt in question. The sickle of light, even when in reality it is indefinitely fine, appears, through the effects of irradiation, to have a definite breadth; so that the Moon seems to traverse a definite distance in obliterating what is in truth the finest possible curve of light. But beyond this there is a circumstance which cannot but give an appearance of somewhat agile motion to the eclipsing disc of the Moon. Up to the very moment when totality is about to begin, the air between the observer and the Moon is illuminated by direct sunlight. This is easily seen by a consideration of fig. 79, in

---

* A digit signifies the twelfth part of the solar diameter. The term is nearly obsolete.

which ABCD represents the portion of the Moon's shadow in the atmosphere, E the place of an observer towards whom this shadow is swiftly advancing. Then, until the shadow actually reaches E (at which moment totality begins) a line drawn from E to D will pass to the left of the line A D *m*—as in the direction E D *f*; so that, since the line A D *m* is necessarily directed towards the Moon's limb, the directly illuminated air (bounded, of course, by A D) extends between the observer and the Moon. Hence, the Moon's disc seems lighted up by this atmospheric glare until the

Fig. 79.



Illustrating the condition of the Earth's atmosphere a few minutes before totality in a solar eclipse.

very moment when totality begins; and as the curtain of glare is drawn suddenly away towards the edge where the last sickle of the Sun's direct light is vanishing, all the circumstances tend to give an appearance of agile motion in that direction.

As respects the rotatory motion which seemed to accompany this leap forward on the Moon's part, we can very well understand it as referring to the completion of the corona, which must needs take place by a sweeping round of the bounding rays to close in upon each other opposite the point where the last part of the

Sun's disc disappears. It will be well to notice, however, as we proceed, whether we have convincing evidence of an apparent motion of the coronal beams after totality has begun. The evidence on this point cannot but have an important bearing on the views we are to form respecting the corona.

Dr. Wyberd saw a corona of very limited extent—indeed only half a digit wide—corresponding to a height not half so great as that of many prominences which have been observed during recent eclipses.

Our next observation refers, however, to a much more favourable view of the corona.

In May 1706 Plantade and Capiés saw a bright ring of white light surrounding the eclipsed Sun, and extending to a distance equal to about one-tenth part of the Moon's apparent diameter. Outside this bright ring a fainter light could be recognised, which extended no less than four degrees from the eclipsed Sun, fading off insensibly, until its light was lost in the obscure background of the sky.

In 1724 Maraldi noted a circumstance of some importance. At the beginning of the total eclipse which was observed in France in that year he perceived that the corona was wider on the side towards which the Moon was advancing than on the opposite side. At the close of totality the widest part of the corona was on the opposite side. As this would exactly correspond to what would be observed if the corona lies beyond the Moon, and so is traversed by the Moon precisely as the Sun himself is, it will be well for us to notice as we proceed whether Maraldi's observation has been confirmed or disproved, or how the evidence stands with respect to it.

In 1733 a total eclipse occurred in Sweden, which was observed in a manner reflecting great credit on the astronomers of that country. The Royal Society of Sweden invited all who could spare the time to assist, as far as their ability permitted, in recording the phenomena presented during total obscuration. Accordingly there are few eclipses—perhaps there is not one—to the phenomena of which so many independent witnesses give testimony. 'At Catherinesholm the pastor of Forshem noticed that a ring of light which appeared round the disc of the Moon was of a reddish colour—an observation confirmed by Vallerius, another pastor, who noticed, however, that at a considerable distance from the Sun the ring appeared of a greenish hue. The pastor of Smoland states that "during the total obscuration the edge of the Moon's disc resembled gilded brass, and that the faint ring around it emitted rays in an upward as well as in a downward direction, similar to those seen beneath the Sun when a shower of rain is impending." The mathematical lecturer in the Academy of Charlesstadt, M. Edstrom, observed these rays with special attention, and remarks, respecting them, that "they plainly maintained the same position, until they vanished along with the ring upon the reappearance of the Sun." On the other hand, the ring, as seen at Lincopia, seemed to have no rays. Professor Grant remarks that "from the descriptions given by several observers, it would seem that at the commencement of the total obscuration the ring appeared brighter and broader at the part of the Moon's limb where the Sun had disappeared, but that towards the close of the obscuration it was more conspicuous in both these respects at the part where the Sun was about to emerge." '[*]

It is observable, therefore, of this well-watched eclipse that it confirms Maraldi's observation, while the variety of appearance presented by the corona at different stations would point to the conclusion that if the corona is not a phenomenon of our own atmosphere, its light must for the most part be of a very delicate nature, insomuch that seemingly unimportant differences in the circumstances under which it is viewed suffice to modify its aspect to a very noteworthy extent. We shall see presently that during recent eclipses similar evidence has been afforded of the extreme faintness of a large portion of the coronal light.

During the eclipse of 1766 the corona exhibited four remarkable expansions, separated from each other by nearly equal intervals.

In the account given by Don Antonio d'Ulloa of the appearance presented by the total eclipse of 1778, we again find a reference to the appearance of rotatory motion in the corona. 'Five or six seconds after the commencement of the total obscuration,' he writes, 'a brilliant luminous circle was seen surrounding the Moon, which became vivid as the centre of

---

[*] From an article in the *Cornhill* for October 1870 (by the present writer).

that body continued to approach the centre of the Sun. About the middle of the eclipse its breadth was equal to one-sixth of the Moon's diameter. There appeared issuing from it a great number of rays of unequal length, which could be discerned to a distance equal to the lunar diameter. It seemed to be endued with a rapid rotatory motion, which caused it to resemble a firework turning around its centre. The colour of the light was not uniform throughout the whole breadth of the ring. Towards the margin of the lunar disc it appeared of a reddish hue; then it changed to a pale yellow, and from the middle to the outer border the yellow gradually became fainter, until at length it seemed almost quite white.'

The next eclipse during which new features of importance were noticed, was that of 1842. We have seen how carefully the prominences were observed during that eclipse, and how many eminent astronomers were engaged in noting the phenomena presented during the total obscuration of the Sun. The corona was also carefully studied. Indeed, one is inclined almost to regard it as a misfortune, that the same astronomers who tell us about the appearance of the prominences, are those from whom we derive our information respecting the corona. One cannot but feel that the accurate observation of both phenomena was more than could be expected even from the most skilful astronomers, and that a division of labour might have been advisable.

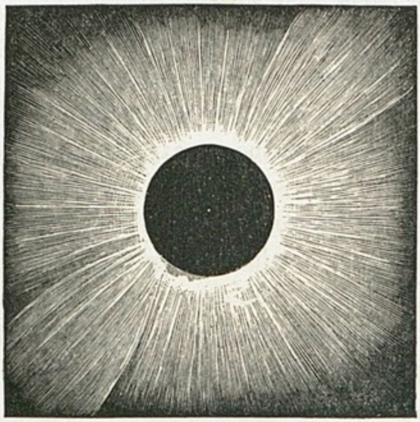
The apparent motion of the corona was noticed by several observers. Some of those stationed at Montpellier thought that the corona had a rotatory motion. Francis Baily compared the appearance of the corona to the flickering light of a gas illumination. Otto Struve, also, was much struck by the violent agitation to which, as it seemed to him, the light of the ring was subjected. The apparent extent of the corona as seen by different observers varied in a somewhat surprising manner. Otto Struve, observing at Lipesk, found the breadth of the corona equal to the Moon's apparent diameter; while M. Petit, observing at Montpellier, assigned to the corona a breadth scarcely exceeding one-fourth of this amount. Baily's estimate lay between these values. Nor were the observers in agreement as to the general appearance of the corona. Otto Struve observed several luminous expansions, some of them extending fully four degrees from the Moon's limb. Signor Picozzi, observing at Milan, saw two jets of light occupying a position which corresponded very nearly with that of the ecliptic. Several observers in France noticed a similar peculiarity. Minor rays, also, were distinctly recognised by Mauvais at Perpignan, and by Baily at Pavia. The last-named astronomer remarks, indeed, that the diverging rays were sufficiently marked to deprive the corona of the appearance of a ring. But Mr. Airy, observing the corona from the Superga, could scarcely recognise any radiation whatever; and he remarks, that 'although a slight radiation might have been perceptible, it was not sufficiently intense to affect in a sensible degree the annular structure by which the luminous appearance was plainly distinguished.'

If we consider the accounts which the observers give of the brilliancy of the corona, we shall find that these peculiarities are in accordance with the theory that atmospheric conditions were alone in question. At the Superga the brightness of the corona seemed no greater than that of the Moon; but at Pavia, where Baily was stationed, the splendour of the corona was much greater. 'I had imagined,' this observer writes, 'that the corona, as to its brilliant or luminous appearance, would not be greater than that faint crepuscular light which sometimes takes place in a summer evening, and that it would encircle the Moon like a ring. I was therefore somewhat surprised and astonished at the splendid scene which now so suddenly burst upon my view.' We have seen that at Lipesk the corona seemed to extend much farther from the Sun than elsewhere, and accordingly we find that its brilliancy was also far greater. The light was so bright, Struve writes, that the naked eye could scarcely endure it. Many could not believe, indeed, that the eclipse was total, so strongly did the corona's light resemble direct sunlight.

But perhaps the most interesting and important observations made in 1842 are those which refer to the structure of the coronal light. It was noticed at Montpellier that the light of the corona was not uniform, nor merely marked with radiations, but that, in places, interlacing lines of light could be seen. Arago, at Perpignan, recognised this peculiarity with the naked eye. He saw, 'a little to the left of a diameter passing through the highest point of the Moon's limb, a luminous spot composed of jets entwined in each other, and in appearance resembling a hank of thread in disorder.'

The accompanying picture (fig. 80) represents the

Fig. 80.



The Eclipse of 1842.

general phenomena seen during the eclipse of 1842; but it must be remembered, that such illustrations cannot be regarded as accurately representing details, because they are usually drawn after the eclipse is over, and represent merely what the observer remembers. Until the whole duration of a total eclipse is devoted by a skilful observer and draughtsman to the delineation of the corona, we cannot expect to have really trustworthy views.

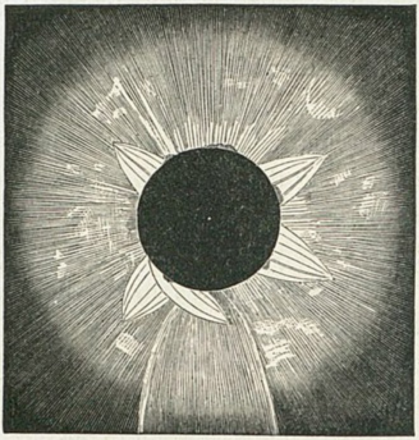
During the eclipse of 1851 no observations were made which tended to throw new light on the nature of the corona. It is worthy of notice, however, that the Astronomer Royal found the aspect of the corona different from what he had noticed in 1842. 'The corona,' he says, 'was far broader than that which I saw in 1842. Roughly speaking, the breadth was little less than the Moon's diameter, but its outline was very irregular. I did not notice any beams projecting from it which deserved notice as much more conspicuous than the others; but the whole was beamy, radiated in structure, and terminated—though very indefinitely—in a way which reminded me of the ornament frequently placed round a mariner's compass. Its colour was white, or resembling that of Venus. I saw no flickering or unsteadiness of light. It was not separated from the Moon by any interval, nor had it any annular structure. It looked like a radiated luminous cloud behind the Moon.'

These observations, combined with what had been before noticed, seem to demonstrate that the aspect of the corona is variable according to the circumstances under which it is viewed. It does not seem to be established that the rotatory and flickering motions suspected by other observers were only optical illusions, though the observation of the steadiness of the corona by such an observer as the Astronomer Royal goes far to negative observations of motion by less experienced astronomers. In the case of a phenomenon like the corona, it is easier to imagine movement in the ring of light than to become convinced of its fixedness.

The eclipse of 1858, visible in Brazil, is chiefly remarkable on account of the strange drawing made by the French astronomer Liais (fig. 81). Unfortunately we have no observations confirming the accuracy of this singular picture.

The eclipse of 1860 is remarkable as the first in which the powers of photography were employed to aid in the resolution of the problems presented by the corona. It will be seen, on a reference to figs. 66 and 67, that Mr. De La Rue succeeded in obtaining traces of the corona. Those seen in Father Secchi's photographs are somewhat more distinct, the method he employed giving a smaller and more fully illuminated

Fig. 81.

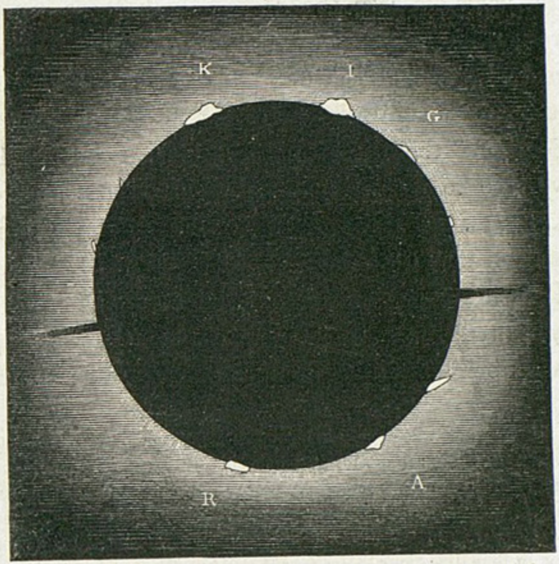


The Eclipse of 1858. (*Liais.*)

image. In a third of Fr. Secchi's photographs (fig. 82) the corona is yet more distinctly shown.

The extension of the corona in fig. 82 is regarded by Fr. Secchi as corresponding to the solar equator, whose position is very nearly indicated by the cross-wire shown in the figure. 'La couronne,' he says, 'est très-irrégulière, mais on peut remarquer qu'elle présente une étendue plus considérable à droite et à gauche que dans les autres directions, c'est-à-dire qu'elle est plus développée dans le plan de l'équateur (solaire) que suivant la ligne des pôles.' But, as Fr. Secchi himself points out further on, the figure indicates

FIG. 82.



The Eclipse of 1860. From a Photograph by Secchi.

rather an extension opposite four points lying between the equator and poles, than an extension at the equator. In fact, fig. 82 presents a very striking resemblance to Mr. Whipple's picture further on (fig. 85). It is worthy of notice, too, that not only does the outline of the corona present this quadrilateral aspect, but in the bright parts close to the Moon's limb there are four corresponding regions of greatest brilliancy. In the second of Mr. De La Rue's photographs the brightest portions seem similarly disposed.*

Some of the direct observations made in 1860 serve also to throw important light on the nature of the corona. To the Astronomer Royal the corona presented much the same aspect as in 1851. Bruhns of Leipsic states that when the last rays of the Sun disappeared, the corona shone out with a white light, of such brilliancy that the protuberances were almost

---

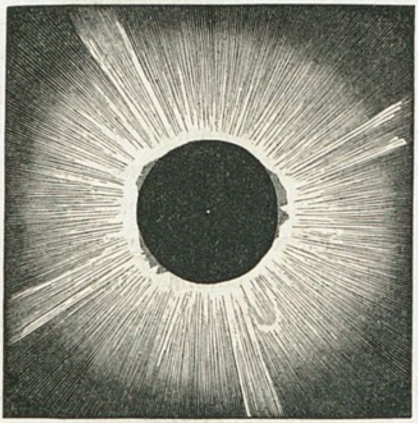
* One cannot wholly agree with Father Secchi's remark, that Mr. De La Rue's photographs afford no evidence of the peculiar quadrilateral expansion of the corona corresponding to the position of the zone of spots. It is true, however, that when Fr. Secchi 'published his results, astronomers did not conceal their doubts.' If the expansion of the corona in four directions be regarded as an ordinary phenomenon (and we have seen how often it has been noticed), some very perplexing questions would be presented as to the cause of the peculiarity. A rectangular figure, like that shown in fig. 82, would correspond to a cylindrical real figure; but it would also correspond to other figures of three dimensions. It is indeed possible that there may be no corona at all opposite the solar poles, the light we see there being merely a foreshortened view (on this supposition) of the great extension over the spot belts. In this case the true figure of the corona would resemble that due to the rotation of two hyperbolas having the same axes, around one of these axes

obliterated. He adds, ‘the black-looking Moon was surrounded by a clear light of unequal breadth. It was considerably wider below than above, varying from nearly one-half to a quarter of a degree, and its general appearance gave me the idea that the Moon was eccentrically placed within it. Its general outline was circular, but on the eastern side a long ray shone out to a distance of about a degree (that is, twice the Moon's apparent diameter); it was of a tapering figure. During the ten seconds that my attention was directed to it, neither the direction nor the length of the ray varied; its light was considerably feebler than that of the corona, which was of a glowing white, and seemed to coruscate or twinkle. My assistant, M. Auerbach, noticed, in the south-western part of the corona, a curved ray about a tenth of a degree in length.’ Father Secchi found that the corona could be seen with the naked eye for about forty seconds after the reappearance of the Sun, ‘the solar light shining like an electric lamp projecting tremulous shadows.’ Mr. De La Rue states that ‘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 seven to eight tenths of the Moon's diameter.’ This description corresponds very satisfactorily with the appearance represented in the accompanying picture (fig. 83) by Feilitsch.

During the eclipse of April 1865 it was noticed that the corona continued visible for thirty-six seconds after the appearance of the first rays of direct sunlight.

The eclipse of March 1867 was only annular, yet it presented a feature well worth careful consideration, and that at a station considerably removed from the line of central eclipse. O. Struve and Wagner, using the great equatorial of the Pulkowa observatory, noticed that when the eclipse reached its greatest phase, the outline of that part of the Moon's disc, which was outside the Sun's, could be distinctly seen. Schwabe, who observed the same phenomenon at Dessau, noticed that this part of the Moon's disc was

Fig. 83.



The Corona during the Eclipse of 1860. (*Feilitsch.*)

rendered visible by its superior blackness; a circumstance which proves that the light of the corona must be considerably stronger than the illuminated atmosphere near the Moon's place,* even when a portion of the Sun's direct light is falling upon the air there.

The total eclipse of August 1867 attracted very much less attention than those which occurred during the same month of the two following years. Yet the observations made by Herr Grosch, of the Santiago observatory, Chili, in conjunction with Vice-Director Vergaza and Lieut. Vidal, are full of significance in connection with the main object of this chapter, which is the determination of the true theory of the corona. At the moment when totality began, 'there appeared,' says Herr Grosch,† 'around the Moon a reddish glimmering light, similar to that of the aurora, and almost simultaneously with this (I mean very shortly after it) the corona. This reddish glimmer, which surrounded the Moon with a border of the breadth of at most five minutes, was not sharply bounded in any part, but was extremely diffused, and less distinct in the neighbourhood of the poles. I can decidedly confirm this at least as regards the Sun's north point, but not so much so of the south point, as that part was less observed by me, but was more particularly attended to by Lieutenant Vidal,

---

* By the atmosphere near the Moon's place, I mean that part of the atmosphere which lies nearly in the same direction as the Moon. Of course it is, in reality, very far removed from the Moon's true place.

† I quote (from the *Student* for March 1869) Mr. Lynn's translation of Herr Grosch's narrative.

who could not afterwards give any more positive information concerning this phenomenon. . . . To speak now of the corona:—its extent was considerably longer in the direction of the Sun's equator; and in considering its nature, we must, I believe, look upon it as decidedly unconnected with him.* Whereas, in the direction of its poles, its apparent height exceeded that of the Moon by only a third of her diameter; in the direction at right angles to this, its extent amounted to four-fifths of that diameter. Its light was white, brighter on the Moon's limb, and becoming gradually fainter on the other side. This white light *was not in the least radiated itself*, but it had the appearance of rays penetrating through it; or rather as if rays ran over it, especially in the direction of east and west, forming symmetrical pencils diverging outwards and passing far beyond the boundary of the white light. These rays had a more bluish appearance, and might best be compared to those produced by a great electro-magnetic light. Their similarity to these indeed was so striking that under other circumstances I should have taken them for such, shining at a great distance. The view of the corona here described is that seen with the naked eye. I employed but a very short time upon it, only as much, in fact, as was necessary to obtain a mere momentary view of the general appearance of the totality. And now, in

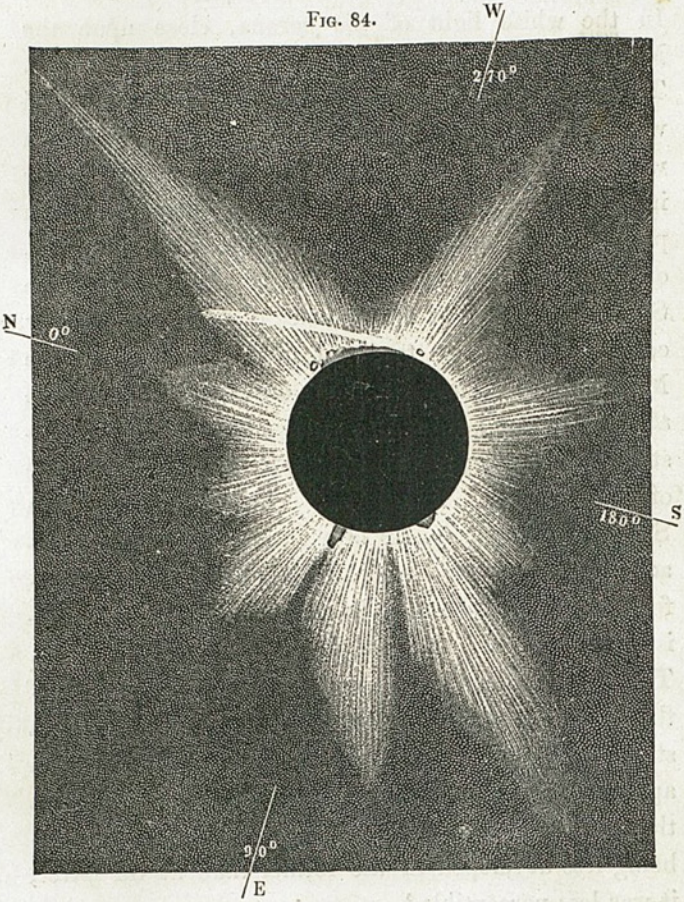
---

* Herr Grosch obviously does not mean that the corona is not, in his opinion, a solar appendage, but that there is no continuous connection between the Sun and the corona; that it is not, in fact, of the nature of a solar atmosphere.

conclusion, I would just mention another phenomenon, which seemed to me too important to pass unnoticed. It showed itself exactly at the north point of the Sun. In the white light of the corona, close upon the Moon's limb, there appeared several dark curves. They were symmetrically arched towards the east and west, sharply drawn, and resembling in tint lines drawn with a lead pencil upon white paper. They gave the impression as if they proceeded *from one point*, which point was on the other side of the Moon; not, however, on the limb, but somewhat nearer the centre of the Sun. Beginning at the distance of one minute, they could be traced up to about nine minutes from the Moon's limb. Throughout the duration of the eclipse *they underwent no alteration whatever*, remaining constant both in form and colour until the disappearance of the corona. Lieutenant Vidal (agreeing with Signor Vergaza) speaks of a similar appearance, exactly at the south point, in the form of a fan or sheaf-formed tuft of light; but he says nothing of characteristic dark curves, such as I saw at the north point. There is, however, no cause to doubt the accuracy of the observed facts. These dark curves indicate a strong magnetic polar force of the Sun, so that an appearance of the kind in question might be seen at the south as well as at the north point; but perhaps being less developed at the former than at the latter, it was less perceptible.'

It is impossible to read this account without feeling how much might be learned from a systematic survey of the corona during any considerable total eclipse. Here were three observers only, who each gave to the

Fig. 84.



The Corona during the total Eclipse of August 1868, as seen at Mantawalok-Kekee.

corona but a fugitive attention, and yet their accounts agree in pointing to the existence of appearances which merited a thorough study during the whole continuance of totality, and that not by two or three observers only, but by ten or twelve at least, each appointed to the investigation of some special feature.

The study of the corona was one of the subjects to which the attention of the observers sent out to view the great eclipse of August 1868 was specially directed. More seems to have been hoped from the application of spectroscopic and polariscopic analysis than from direct observation; and, accordingly, the best accounts we have of the general aspect of the corona are those derived from observers not belonging to the expeditionary parties. We owe to the professors of the college of Manilla, who observed the eclipse at Mantawalok, the accompanying very remarkable drawing of the corona. It is important to notice that, unlike most pictures of the corona, this one can be trusted. Owing to the want of success which had attended all attempts to photograph the corona, the professors were led to think of an ingenious plan for obtaining exact drawings. They prepared beforehand several sheets of paper, which were introduced one after another into a dark chamber, so that the image of the eclipsed Sun fell upon them, and the features of the corona were rapidly sketched out on each. The corona is described as of a somewhat triangular aspect; yet it is to be noticed that there are four rays of the longer sort, so that the corona in this case, as in so many others, exhibits a general approach to the trapezoidal figure. The slightly curved streak of white light crossing the longest of the coronal beams is a very remarkable feature. This streak is described as of an intensely white and uniform light. It appeared (or was at least first noticed) some two minutes after the beginning of the totality, and remained visible until the Sun began to reappear.

The polariscopic observations made during this eclipse on the light of the corona were not successful. The observers agreed, indeed, that the light of the corona was polarised in a plane through the Sun's centre; a circumstance which, if confirmed, would go far to prove that the corona shines by reflecting the Sun's light; but the Astronomer Royal, who has carefully examined their accounts, considers that no dependence can be placed on their conclusions. I may as well add that, during the eclipse of 1869, the American observers obtained a different result, and that, in the opinion of those best competent to judge, the question of the polarisation of the corona's light in a plane through the Sun's centre remains still in abeyance.

Lieutenant Tennant examined the light of the corona with the same spectroscope which, as we saw in the last chapter, has given information of such interest respecting the coloured prominences. He saw a faint continuous spectrum. 'Thinking that want of light prevented my seeing the bright lines which I had fully expected to see on the lower strata of the corona, I opened the jaws of the slit.' He still failed to recognise any signs of bright lines. '*What I saw,*' he writes (the italics are his) '*was undoubtedly a continuous spectrum, and I saw no lines.* There may have been dark lines, of course, but with so faint a spectrum, and the jaws of the slit wide apart, they might escape notice.'*

Before discussing this result, I proceed to mention other evidence bearing on the same point.

During the eclipse of August, 1869, several of the American observers renewed the attempt to determine the exact nature of the corona-spectrum. Their results were not accordant. Professor Pickering obtained a faint continuous spectrum crossed by three bright lines. Professor Harkness recognised only one bright line on a continuous background. After observing the spectrum of the prominences, he asked Professor Eastman, who was directing the telescope, to bring the corona into the field. A bright part of the corona was thus brought under examination, but *no spectrum appeared.* 'I asked him to try another place. Still nothing was visible; and, raising my head from the instrument for the first time since the commencement of the totality, I remarked, "Can't see any spectrum; don't believe we will get any." "Oh! yes, we will," said he.

---

* It is strange that, notwithstanding the very plain account given by Lieut.-Col. Tennant, it should continue to be asserted that, according to his observations, the corona gave a solar spectrum; that is, a spectrum crossed by the Fräunhofer lines. The American observers were so misled by this assertion as to search specially for the dark lines which Colonel Tennant was supposed to have seen. The obvious meaning of his narrative is, that he saw a continuous spectrum, without either dark lines or bright lines.

At that instant it struck me that perhaps the slit was too narrow; so I opened it a little, and then again placed my eye at the instrument. In the meantime Eastman had put the needle at a very bright part of the corona, and I at once saw a continuous spectrum, about as bright as that given by the full Moon on a clear night. Remembering that the observers in India, in August 1868, had said that the corona gives a continuous spectrum *with absorption lines'* (a mistaken idea, as mentioned in the last note), 'I looked very carefully for them; but, to my great surprise, I could see none, and I am perfectly satisfied that none were visible in my instrument. On the contrary, I saw an absolutely continuous spectrum crossed by a single bright line, whose position was recorded.' This line was in the green, and, if actually in the place assigned by Harkness, would correspond to a line belonging to the spectrum of copper. But as he makes the line coincident with one of the prominence-lines, it seems certain that it can be no other than a line of iron, close by the E lines, which has been seen by several observers in the spectrum of the prominences.* Professor

---

* This iron line appears also as a bright line in the spectrum of the aurora, according to the best observations hitherto made. As I write, I receive from Mr. Browning an account of his observations on the aurora of October 25, with one of his miniature spectroscopes. He saw a bright line near E, and another not far from B. Mr. Birmingham of Tuam, with a similar instrument, saw the usual bright line in the green, one not far from it to the left, very faint, and one of medium brightness near F. Professor Wenlock notes four lines in the yellow-green part of the spectrum, and one somewhat more refrangible than the F line. All these accounts are reconcilable when we remember the extreme faintness of the auroral light, and the fact that no exact determination by the

Young paid particular attention to the spectroscopic observation of the corona. He also had been misled by erroneous accounts respecting the Indian observations, and so expected to see a faint solar spectrum. He found, on the contrary, that the light of the corona gave a spectrum of bright lines. He saw three such lines, and he considers it certain, from their close agreement with those shown in Professor Winlock's picture of the aurora-spectrum, 'that the corona is simply an electric discharge, no doubt varying with great rapidity, as we see in the case of the aurora; in fact, that the corona is a permanent solar aurora.'

Now, although these accounts seem at first sight discordant, it appears to me that they can be brought into agreement, not only with each other, but with Lieutenant-Colonel Tennant's, by a consideration of the circumstances under which they were severally made. Tennant, seeing only a continuous spectrum, opened the slit somewhat widely: 'the jaws of the slit were wide apart,' he says; too wide, I imagine, to show the bright lines. For, from what is shown at p. 144, it will be seen, that the brightness of the coronal bands

method of coincidences has ever yet been attempted. I had written further to the effect that, 'even when the auroral light is only ruddy to the eye, no red lines are seen, so that we may conclude that the excess of red is due to a peculiarity in the light of mixed refrangibility forming the continuous spectrum,' when I learned that, on the evening of October 24, Mr. J. R. Capron, with one of Browning's small direct-vision spectroscopes (adapted to star observation), had succeeded in observing a line in the red, 'very much like the lithium line, but rather more dusky. It was only well seen in the rosy patches of the aurora, but could be faintly traced wherever the rose-tint at all extended.'

or lines could not be increased in this way, though their *breadth*, and so the total amount of light from them, would be increased in precisely the same proportion as the opening of the slit. But the *brightness* of the continuous background would be increased in this same proportion. Hence the bright lines which Tennant could not see, on account of their fineness, were changed by opening the slit into broad bands of no greater brightness, and rendered invisible by reason of the increased brightness of the background. An intermediate amount of opening would in all probability have shown the lines. Now we see that Professor Harkness failed even to see a continuous spectrum when he used a narrow slit; and the fineness of the lines (not nearly so brilliant as the prominence-lines) caused them to escape his notice precisely as had happened with Tennant. But when he opened the slit 'a little,' he saw the continuous spectrum and one bright line. Had he opened it somewhat more, he would not have seen that bright line, but would have failed as Tennant had, and for the same reason. Had he opened it a little less, he would probably have seen the continuous spectrum and the three bright lines, as Professor Pickering did. With a somewhat smaller opening the continuous spectrum would disappear through excessive faintness; but the three bright lines seen by Professor Young would be even more distinctly visible. We see in fact that Professor Young, who succeeded readily in seeing three bright lines, failed to recognise the continuous spectrum.

It may be said that this is hypothetical; and so in a sense it is. What an observer would have seen under certain circumstances different from those which actually occurred must necessarily be hypothetical. But as to the matter of fact on which this hypothetical interpretation of the different results is founded, there can be very little question. Professor Young and Pickering *saw* three bright lines, Professor Harkness *saw* one such line, and Tennant, Pickering, and Harkness *saw* a continuous spectrum; while the conditions under which these different results were obtained are known. My interpretation accounts simply and naturally for all the observed spectra. I make the whole question one of slit-opening.

With sufficient dispersive power * we get, as might be expected:—With a very narrow slit, three bright lines (so fine as to be only recognisable on a close scrutiny, such as that given by Professor Young), and the continuous background too faint for recognition. With a slit not quite so narrow, we get a faint continuous spectrum and three lines, still so fine as to require very careful scrutiny for recognition. With a somewhat wider slit, we get a brighter continuous background on which the brightest of the three lines alone is visible. And lastly, with the jaws of the slit wide apart, we have a yet brighter continuous spectrum, and no visible bright bands. All this is precisely in accordance with what the theory of the spectroscope

---

* With insufficient dispersive power we have a continuous spectrum without bright lines, whether the slit be widely opened or not.

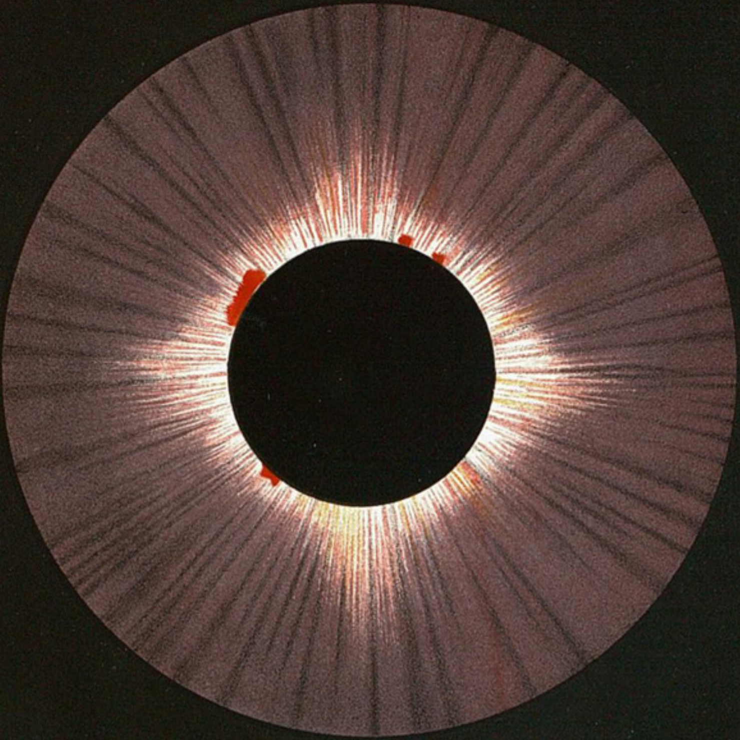
requires, and it accords perfectly well with all the observed facts, save one,—the failure, on the parts of Harkness and Tennant, to detect the faint bright lines of the corona, when these must have been very fine, owing to the narrowness of the slit. But this will surprise no one familiar with the very varying powers of observers, as respects the recognition of faint objects, or small objects, or objects which are both faint and small.

It is, however, always possible, or rather it is highly probable, that different parts of the corona may give different spectra.* The ascertained facts are these—that some parts of the corona do undoubtedly give a spectrum consisting in part of three bright lines; that these lines agree in position with bright lines belonging to the spectrum of the terrestrial aurora; and that, so far as observation has yet gone, the spectrum of the corona contains no dark lines.

The direct observations of the corona as seen during the American eclipse were numerous and important. Mr. W. S. Gilman, jun., from whose coloured drawing of the eclipsed Sun Plate VII. is taken, writes thus respecting the appearance of the corona:—'The general outline of the corona was a tra-

---

* This would accord well with what is observed of the spectrum of the aurora borealis. In the communication already referred to (note, p. 338), in which Mr. Birmingham describes his observation of three bright lines in the spectrum of the aurora, he says of the 'intense red of broad areas of light,' that 'there was here no line whatever to be detected.' And again he adds, 'the white light seen in some parts of the sky gave only the one principal line in the green.' We have seen also that the red line of the auroral spectrum is not commonly visible.



ECLIPSE OF AUGUST 7TH 1869.

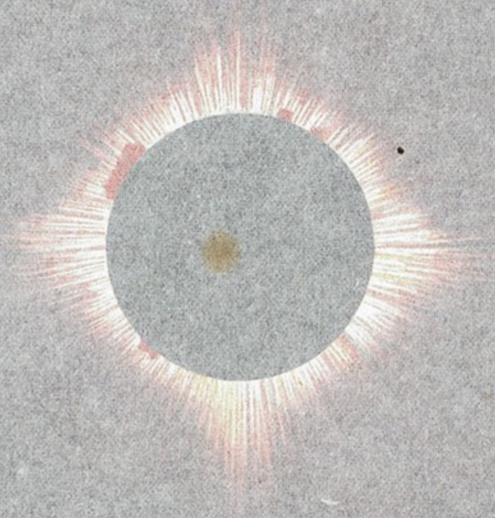
Drawn by W. S. Gilman Junr. Esqr.

WITH A 4 INCH REFRACTOR.

pezium, with the widest side to the south-east. There were also lesser projections on the four sides, as well as several small indentations. The longest masses of light coincided very nearly with the north and east points, at the middle of totality. Mr. Farrell also noticed the same peculiarity. The corona was composed of an infinitude of fine violet, mauve-coloured, white, and yellowish white rays, issuing from behind the Moon. I detected no clouds in it. The exterior edge was very jagged in appearance, but did not possess a harsh outline, having, on the contrary, a soft blurred look. This was quite contrary to my expectations, as, from all the drawings of eclipses I had ever seen, I was led to expect a well defined and truly circular halo of light, extending to an equal distance on all sides. Mr. Farrell described the appearance of the corona verbally, as follows: "It was a silvery-grey crown of light, and looked as if it was the product of countless fine jets of steam issuing from behind a dark globe. Near the Moon's disc, the light seemed almost phosphorescent."

'The small amount of light given by the corona,' he adds, 'is indicated by the remark that, "during totality, the seconds on our watch-faces could not be read without the assistance of the lamps placed in the windows of the house to aid us at this juncture. The time could with difficulty be told by the larger hands."'

Professor Simon Newcombe makes the following remarks: 'Looking directly at the corona, there was no actual appearance of striation, but it seemed to be of a jagged outline extending out into four sharp points,



ECLIPSE OF AUGUST 7TH 1869.

Drawn by W. S. Gilman Junr. Esqr.

WITH A [illegible] INCH REFRACTOR

pezium, with the widest side to the south-east. There were also lesser projections on the four sides, as well as several small indentations. The longest masses of light coincided very nearly with the north and east points, at the middle of totality. Mr. Farrell also noticed the same peculiarity. The corona was composed of an infinitude of fine violet, mauve-coloured, white, and yellowish white rays, issuing from behind the Moon. I detected no clouds in it. The exterior edge was very jagged in appearance, but did not possess a harsh outline, having, on the contrary, a soft blurred look. This was quite contrary to my expectations, as, from all the drawings of eclipses I had ever seen, I was led to expect a well defined and truly circular halo of light, extending to an equal distance on all sides. Mr. Farrell described the appearance of the corona verbally as follows: "It was a silvery-grey crown of light, and looked as if it was the product of countless fine jets of steam issuing from behind a dark globe. Near the Moon's disc, the light seemed almost phosphorescent."

'The small amount of light given by the corona,' he adds, 'is indicated by the remark that, "during totality, the seconds on our watch-faces could not be read without the assistance of the lamps placed in the windows of the house to aid us at this juncture. The time could with difficulty be told by the larger hands."'

Professor Simon Newcombe makes the following remarks: 'Looking directly at the corona, there was no actual appearance of striation, but it seemed to be of a jagged outline extending out into four sharp points, nearly in the horizontal and vertical direction; while midway between these points the serrated edge hardly seemed to extend beyond the body of the Moon. The greatest distance to which the extreme points seemed to extend did not exceed a semidiameter of the Moon, and there was nothing like long rays of light extending out in any direction whatever. When I turned my head the points did not seem to turn with it. Still I experienced a singular difficulty in judging accurately either of the number or direction of the jagged points, or of the extent to which they might be optical illusions, produced by the differences in the height and brilliancy of different parts of the corona.'

'Seen through green glass, the corona consisted simply of four or five prominences, extending around the Moon, smooth in their outline, shading off by imperceptible gradations, and rising to different heights, the greatest height not exceeding four or five minutes.'

Let us next consider Professor Eastman's account of the corona. 'I was considerably disappointed,' he says, 'with the appearance of the colour and brilliancy, as well as with the extreme contourof the corona. Most observers have described the colour as 'pure' or clear white, and the light as very brilliant, while nearly all the published sketches represent the contour as nearly circular and regular, and the coronal rays as radial, and equally distributed about the body of the Sun. The colour of the corona, as I observed it, both with the telescope and without, was a silvery white, slightly modified in the outer portions by an extremely faint tinge of greenish violet; and I could not detect the least change in the colour, or in the position of the rays during totality. The light of the corona was not brilliant—perhaps from the effect of haze—but appeared more like the pale light from the train of a meteor than anything else that I could recall at the time. The corona seemed to be composed of two portions, both visible to the naked eye; in which, with the small instrument which I used, I was unable to trace any similarity of structure. The portion nearest the sun was about one minute high, forming nearly a continuous band about the Sun, and appeared to be a mass of nebulous light, resembling in structure the most brilliant irresolvable portions of the Milky Way. Its colour was silvery white, and, like its density, appeared the same throughout its whole extent. The outer portion consisted of rays of light arranged in two different ways. In five places they were arranged into groups resembling star-points, composed of slightly convergent and radial rays, but elsewhere were disposed on radial lines. The colour of the bases of the star-points and of the radial lines was the same as that of the inner portion, while the outer portion of the points had a very faint greenish violet tint. The radial lines were the most prominent.' He adds that 'four of the star-points projected farther from the Sun than the ordinary radial lines, and gave the contour of the corona the form of a trapezoid.' Between two of the largest protuberances scarcely any corona was observed.'

The observations of Mr. J. Homer Lane are interesting, because he had more particularly charged himself, he says, 'with the duty of watching for any possible low atmospheric limit, marked by anything like a regular boundary and superior intensity of light very near the Sun's limb.' He does not appear to have succeeded in detecting any such signs of a boundary line limiting an atmosphere lying lower than the limits of the corona. 'I next turned my attention,' he proceeds, 'to the agglomerations of white light in the corona, and fixed upon two of those which were remarkable for their small size and the comparatively dense accumulation of light in them. These were situated about 80° from the vertex towards the right, as seen inverted in the telescope. In appearance they might well be compared to small telescopic comets, with tails of some length, but without a head, and with no distinct indication of a head at one end rather than the other. They were not far from radial in direction relatively to the Sun's centre—whether exactly so I did not remark at the time—but appeared completely isolated, and had their origin far above the limb of the Moon; so far at least that, though that part of the limb must have been approaching them, if their height above the Sun's limb remained constant, the approach did not attract my attention. I referred them two or three times to the profile of neighbouring lunar mountains, so far as to make it evident there was no such relative motion as must have been expected, had there been anything floating in our atmosphere.

They manifestly belonged to the heavens, and I made no doubt were to be classed with the other parts or aggregations of white light in the corona. These two bodies I scrutinised closely for some time, and, as I suppose, to the end of totality. The form, dimensions, and appearance of each, and their distance apart, were in constant review; but not the least change in either respect was seen. It would be in vain for me to try to estimate the length of time occupied with these objects. As to the distance between these two white comet-like objects, I judged, after the largest solar spot had been uncovered, that they would have included between their centres the nucleus of that spot, but not the penumbra. I had no means of taking any measures, nor would it have been easy to do so without sacrificing the scrutiny for the presence or absence of changes. In order, however, to reproduce as nearly as possible what I saw, I have laid down upon paper a circle of the same size as the Moon's outline in the focus of the object-glass—almost an inch—and holding this up to the light in the field of view of the same eye-piece used in the telescope, have tried to lay down with a pencil the dimensions and situation of the two objects. The following is the result I got in this way:

| | |
|---|---|
| Length of each cometoid light . . . . . | 130″ |
| Height of its origin above the Moon's limb . | 100″ to 80″ |
| Distance from centre to centre of the two . . | 50″ to 40″ |

'Perhaps the height above the Moon's limb should be taken at about half a minute, shortly before the end of totality. The estimate of 50″ to 40″ here made for the distance apart of the two objects, may be considered entirely independent of the one first given referring to the solar spot. In that case the judgment comparing with the spot was checked by selecting a pair of scratches on the blue screen glass in the field of view before the eclipse was over, whose interval was judged not far from equal to that of the two objects, and then bringing the solar spot between them.'

But perhaps the most important of all the observations made on the general aspect of the corona, during the eclipse of August 1869, were those made by General Myer, who watched the progress of the eclipse from the summit of White Top Mountain, near Abingdon, Virginia, 5,530 feet above the sea level. 'The point of observation,' he remarks, 'was sought with the view of placing ourselves as far as possible above the lower and denser strata of the atmosphere, and the smoke, haze, and obstacles to vision with which they are charged.' It is on this account, and because of the bearing of the evidence on the question of the effect which our own atmosphere produces on the appearance of the corona, that General Myer's observations are chiefly important. The telescopic observations are less interesting than they would otherwise have been, so far at least as the considerations we are now dealing with are in question, on account of the smallness of the field of view, which did not extend far beyond the prominences. General Myer remarks that, in the telescope, the corona or aureola exhibited a clear yellowish bright light closely surrounding the lunar disc, and fading gradually, with perhaps some tinge of pinkish green, into the line of the darkened sky. 'Upon this corona, extending beyond its brightest portion, the well defined rose-coloured prominences were projected at various points of the circumference.'* But it is when we turn to the description of the corona, as seen by the naked eye, that the characteristic peculiarities resulting from the position of the observer are recognised. 'To the unaided eye' says Myer, 'the eclipse presented, during the total obscuration, a vision magnificent beyond description. As a centre stood the full and intensely black disc of the Moon, surrounded by the aureola of a soft bright light, through which shot out, as if from the circumference of the Moon, straight, massive, silvery rays, seeming distinct and separate from each other to a distance of two or three diameters of the lunar disc, the whole spectacle showing as upon a background of diffused rose-coloured light. This light was most intense, and extended furthest at about the centre of the lower limb, the position of the southern prominence. The silvery rays were longest and most prominent at four points of the circumference, two upon the upper and two upon the lower portion, apparently equidistant from each other (and at about the junctions of the

---

* Since both the prominences and the corona are luminous, we cannot positively conclude, from this description, that a part of the corona really lay behind the prominences; yet it is well to observe how closely the description accords with this view, or, in other words, with the view that the corona is a solar appendage.

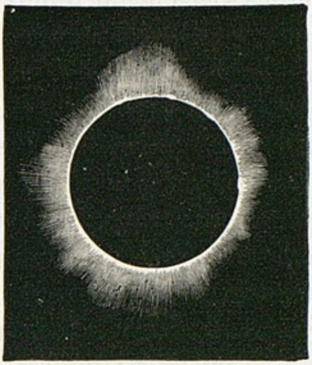
quadrants designated as limbs) giving the spectacle a quadrilateral shape. The angles of the quadrangle were about opposite the north-eastern, north-western, south-eastern, and south-western points of the disc. A banding of the rays, in some respects similar, has been noted as seen at the total eclipse of July 18, 1860. There was no motion of the rays; they seemed concentric.'*

---

* General Myer's description of the general aspect of the sky and air when the total eclipse was in progress deserves to be added:—'The approach of the Moon's shadow,' he says, 'did not appear to be marked by any defined line, or the movement of any dark column of shade through the air. The darkness fell gradually, shrouding the mountain ranges and the dim world below in most impressive gloom. Our guides had been instructed to watch for the shadow, and to call to us at the glasses. They saw nothing of which to give notice. At the same time, and in vivid contrast, the clouds above the horizon were illuminated with a soft radiance; those towards the east with lights like those of a coming dawn, orange and rose prevailing; those northward and westward, as described to us by Mr. Charles Coale of Abingdon, Virginia, who was present, with rainbow bands of light of varied hues. I quote, in his words, a description written by him, as of interest in reference to the dispersion of light:—"The grandest of all to us, who had no astronomical ambition, or astronomical knowledge, to gratify, was the effect upon the clouds during the total obscuration. Those who have had the privilege of being upon White Top, and enjoying the westward scene, will remember the grand panoramic view of mountains beginning on the northern and southern horizon, and stretching away to the west till they seem to meet, and will appreciate the scene that we now attempt to describe. Stretching along this semicircle of mountains in long horizontal lines, far below the Sun, lay light and fleecy clouds, as if resting upon their wings during the seeming struggle between the orbs above them. At the moment of the falling of the dark shadow, when naught was to be seen above but the stars and the circle of light around the Moon, these clouds became arrayed in all the colours of the rainbow, presenting an indescribable richness with their background of sombre mountain. To our vision, it was as if bands of broad ribbon, of every conceivable hue, had been stretched in parallel lines half round the universe."'

During this eclipse a more successful attempt was made to photograph the corona than on any former occasion. Fig. 85 represents the corona as photographed by Mr. Whipple, at Shelbyville, Kentucky. The four-cornered aspect is here distinctly recognised, and the

Fig. 85.



From a Photograph of the Solar Corona during the Eclipse of August, 1869.

---

In a letter subsequently written to General Myer, on the subject of the remarkable colour-scene described above, Mr. Coale remarks, 'I was probably bordering on the extravagant (though not more so than is allowable in country journalism) in giving to the clouds "all the colours of the rainbow." I clearly remember, however, that there were distinct bands of pink, purple, yellow, orange, and fiery red, and each slightly tinged with different shades of its own colour. One of the bands had, I remember, to my vision, a slight lilac tinge. I do not remember to have observed any green or blue, but I do remember that the lower edge of the purple had a very faint blue tinge. All these resting against a dark background gave them an indescribably gorgeous appearance, the lines of colour seeming to be divided by stripes of black. They all lay in horizontal lines one above the other. My impression is, that those colours appeared at the moment the shadow passed from the lower edge of the Sun, though I am not positive.'

probability may be inferred that, with a longer exposure, the rays would have been presented as seen by Gilman, Eastman, and others, if not as seen by General Myer. In the photograph there is, indeed, a sharpness of outline which might readily be interpreted by those unacquainted with the nature of photographic processes to imply the existence of a real boundary line separating this part of the corona from the part without. But, as a matter of fact, the sharpness of outline is due to peculiarities in the process of development. It may be recognised in the photographs taken at Ottumwa, although in them the corona has a much smaller extent. It is not noticed, however, in the photographs by Dr. Curtis, where the corona has about the same degree of extension; the reason being, that he employed special care in avoiding over-development of the negative. Hence no doubt whatever can remain that the sharpness of outline in the Ottumwa photographs, as also in Mr. Whipple's, implies no real limitation of the object photographed.*

---

* As some stress has been laid on this matter by those who advocate theories respecting the terrestrial nature of the corona, it may be well to present at length Dr. Curtis's statement respecting the erroneous interpretation of these photographic records. He says that he has read with surprise an extract from a letter, written by Dr. Gould to Professor Henry Morton, in which the former says, 'An examination of the beautiful photographs made at Burlington and Ottumwa, by the sections of your party in charge of Professors Mayer and Himes, and a comparison of them with my sketches of the corona, have led me to the conviction that the radiance around the Moon, in the pictures made during the totality, is not the corona at all, but is actually the image of what Mr. Lockyer has called the chromosphere.' 'Dr. Gould proceeds,' says Dr. Curtis, 'to specify the points at variance between the corona as photo-

Such is a sketch of the evidence adduced up to the present time respecting the solar corona. It appears

graphed and the same object as seen and sketched by him; and because the two representations do not correspond in feature, he infers that the objects depicted cannot be identical. This same argument would apply equally well to the "radiance" shown in my own photographs, since in them the phenomenon, though faint, agrees in outline with the similar object on the Burlington and Ottumwa pictures. Now, I cannot but believe that Dr. Gould is in error in imagining this aureola not to be simply the image of the more intense portion of the corona near the surface of the Sun. In the first place, the experience of this very eclipse has shown how guardedly all sketches and drawings of the appearances of totality should be received, as affording an accurate record of either the shape, size, or position of the various objects. This is evident upon comparing the various sketches made by eye-observers of the protuberances and corona, both with each other and with the photographs, and observing the very great discrepancies manifest. Of ccurse, it is not meant that accurate measurements made by a micrometer eye-piece in the telescope, or similar determinations of position-angle, cannot be relied upon, but, on the contrary, the argument is that *only* such are to be received as trustworthy, and that all *general* sketches and drawings made hastily during the few exciting minutes of totality, or from memory afterwards, form but a weak ground upon which to base an important scientific hypothesis. But positive proof in the question at issue is afforded by the very perfect photographs of the corona taken at Shelbyville, Kentucky, by Mr. Whipple, of the Cambridge expedition. Here we have a series of several negatives obtained by receiving the focal image of a six-inch object glass directly upon the sensitive plate, and taken, with a wide range of exposure, from five to forty seconds. Of these the one exposed the longest (fig. 85) yields a splendid and unmistakable picture of the corona, representing it, where the converging rays occurred, of a depth equal to a quarter of the Moon's diameter. Surely Dr. Gould cannot imagine the aureole of *this* photograph to be the chromosphere and not the corona; and yet *all* these pictures of Mr. Whipple's, and all of the Philadelphia expedition, and my own, agree perfectly in the features and position of the various irregularities in the outline of the corona, the difference in the representation of that object in the several photographs being solely one of extent and brilliancy. Dr. Gould adduces as an additional argument in favour of his assumption, the observation that the long coronal beams appeared to him to be "variable," while the "aureole" photographed was evidently "constant" during the time of totality. This argument, to me that although it does not suffice to answer all the questions of interest suggested by this imposing

however, loses some of its force when it is remembered that to other observers the corona appeared to the eye absolutely unchangeable, both in form and position, during the whole period of the total obscuration.'

Dr. Curtis then proceeds to consider how far Dr. Gould may have been led to found his opinion upon the circumstance that the 'aureole' in the Philadelphia photographs, 'while falling far short of the height above the Moon's limb attained by the corona as seen by the eye, yet appears of very great brilliancy, rivalling the protuberances in that respect, and comes to an almost abrupt termination a short distance above the solar surface.' He shows that these peculiarities must be regarded as in all probability simply 'photographic effects,' the prints of the photographs 'giving every indication that the negatives from which they were taken were strongly intensified after fixing.' 'This operation,' he adds, 'practised to give additional density to weak negatives, would have, in this case, precisely the effect of increasing on the photograph the apparent brilliancy of the corona without adding to its extent. Moreover, that this excessive photographic brilliancy of the under portion of the corona should not be taken as a proof of any physical or chemical peculiarity in the actual object is quite conclusively proved by my own photographs, which, while showing about the same extent of corona as those pictures of the Philadelphia party that received the least exposure, yet represent it as a very feeble luminosity, fading gradually and imperceptibly into complete darkness, and this while the same photographs show the *protuberances* of great brilliancy. If this peculiarity of the Burlington and Ottumwa photographs had indeed any influence in leading Dr. Gould into the misconception into which I cannot but believe he has fallen, the circumstance affords but another example among many that I have seen, of the necessity that a critic, before attempting to draw scientific inferences from photographic representations, should himself become something of a photographer, else he will be very apt to fall into this natural error of ascribing effects wholly produced in the dark-room to physical characteristics of the object pourtrayed. And by a singular coincidence, evidence that Dr. Gould has not a practical acquaintance with the art would seem to be afforded in this same published letter, by his total misinterpretation of another purely photographic effect, viz., the apparent encroachment of the prominences upon the disc of the Moon as seen in the photographs. This curious appearance, instead of being due to "specular reflection," is wholly a dark-room phenomenon.'

phenomenon, it yet leaves very little room for doubt as to the general characteristics of the corona.

We are fortunately able to dispose very briefly of some of the theories respecting the corona which were suggested in old times. We need no longer inquire with close scrutiny into the theory that the corona is due to a lunar atmosphere, because we now have abundant evidence that either there is no lunar atmosphere, or that at least no atmosphere competent to produce such a remarkable appearance surrounds our satellite. We know that two very definite results (to consider no others) must inevitably follow if the Moon had an atmosphere of even moderate extent. In the first place, the refractive power of such an atmosphere would cause somewhat more than one-half of the Moon's surface to be illuminated—precisely as, in the case of our own Earth, the Sun is apparently raised by atmospheric refraction above the horizon of places lying beyond the hemisphere turned directly towards him. It is easy to show that under these circumstances, when the Moon is nearly new, her horns should extend somewhat beyond a semicircle. The fact that no such extension has been noticed suffices to prove that she has either no atmosphere or one of very limited extent. Again, the occultation of a fixed star by the Moon could not fail to be accompanied by evidence of the existence of any lunar atmosphere. Instead of disappearing suddenly, the star would be slowly reduced in brilliancy, and would appear to cling for a few moments to the outline of the Moon's disc. Since no such appearances are noted, we must reject the conception that the Moon has an atmosphere of appreciable extent, and with it the theory, which to Kepler and Halley had seemed attractive,* that the corona is a phenomenon due to the action of a lunar atmosphere on the solar rays.

Nor need we dwell on the theory propounded by Delisle, that the corona may be an optical effect due to the diffraction of the solar rays as they pass by the Moon, because Professor Baden Powell and Sir David Brewster have abundantly demonstrated that the effects due to such diffraction could not be discernible from the Earth.

We may thus limit our attention to two general theories (each admitting of special differences) which at present divide attention. One is the theory that the corona is a solar appendage; the other is the theory that it is a phenomenon due to the passage of solar light though our own atmosphere.

It will be seen that somewhat important issues depend on the selection we have to make between these two theories. For, if the corona be but a phenomenon of our own atmosphere, it is not worthy of more attention than we might give to the rays which stream through openings between clouds and form vast beams of light across the heavens. But if it be a solar appendage, then it is one of the most imposing phenomena the

---

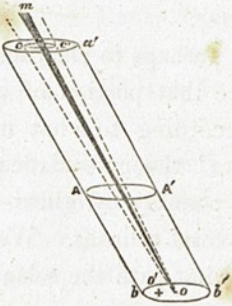
* Halley mentions that contrary sentiments were entertained 'by those whose judgments he should always revere.' It has been supposed that Halley here refers to Newton.

mind of man can dwell upon. Those long beams have (then) a real extension compared with which the volume of our Earth, nay even the volume of the Sun himself, sinks into utter insignificance; and that inner radiance which encloses the Sun on every side indicates a luminous region of inconceivably vast extent, while the problems suggested for our consideration by the aspect of this region, and by the physical state of the material distributed through it, are of the most interesting character.

It will conduce perhaps to clearness of conception if we consider where that portion of our atmosphere is situated which, according to what may be called the 'atmospheric glare' theory—advocated by M. Faye, Mr. Lockyer, and possibly by others—is illuminated by solar light during total eclipses. We can then inquire at our leisure by what path the solar light reaches this region of our atmosphere.

Let the observer be at O (fig. 86) in the centre of the Moon's shadow *b b′*, which forms an elliptical dark space on the Earth's surface. We need not concern ourselves with the shape of this ellipse, which will vary in different eclipses and at different stations. We need only note that in a considerable total eclipse the least diameter of *b b′* will be greater than 100 miles. Now, let A *b′* represent a portion of the Moon's shadow-cone, forming within our atmosphere a figure not differing greatly from an oblique cylinder. Assigning to the atmosphere a height of about 200 miles this cylinder will have a shape such as A *b′*; but if we assign to the atmosphere an elevation of 500 miles,* then we should have to assign to our shadow cylinder the figure *a b′*. Now, let lines drawn from the observer's eye to the boundary of the Moon's disc enclose the black cone shown in part in o *m*, while lines drawn to the boundary of a circular corona extending one degree on every side of the Moon's disc during totality form the cone shown

FIG. 86.



in part in o *c c′*. Both cones are shown well within the cylinder *a b′*, because as a matter of fact we find that the lines o *a*, o *a′* would contain an angle considerably greater than the angle *c* o *c′*.

---

* We are here considering, be it remembered, the atmosphere which is effective in reflecting solar light to the eye; and it will scarcely be admitted by most meteorologists that an atmosphere *of this sort* extends even to so great a height as 100 miles from the Earth's surface. Our best estimate (so far as this characteristic of the atmosphere is concerned) is undoubtedly that founded on the height of the twilight curve when observed from suitable stations, for this height depends on the very quality we are considering. Now Bravais, from a discussion of Lambert's observations of the crepuscular curve, deduced a height falling short of 100 miles, while his own observations made from the summit of the Faulhorn gave a height of about 66 miles. So far as the real extension

Now, if the atmospheric glare theory is true, all the cone $\mathrm{o}\,c\,c'$ in our atmosphere is illuminated at the time of central eclipse except only the core belonging to the cone $\mathrm{o}\,m$. This is certain, because we *see* the Moon dark and the corona bright; so that we require $\mathrm{o}\,m$ to be dark and the remainder of $\mathrm{o}\,c\,c'$ to be bright. Now, so far as undeflected solar rays are concerned, the whole region $a\,b'$ is in shadow. The light from the prominences can get into this region, and so perhaps can solar light deflected by some possible action at the Moon's surface. But the problem which the supporters of the 'atmospheric glare' theory have to solve is to get the light into the cone $\mathrm{o}\,c\,c'$, growing brighter and brighter up to the very boundary of the dark cone $m\,\mathrm{o}$ (to correspond with the increase of the corona's light up to the Moon's limb), and there suddenly ceasing. This done, they must show further that if another observer is stationed somewhere else

---

of the atmosphere is concerned, we may accept the opinion of Dr. Balfour Stewart that observations made on the aurora supply the best means of forming an opinion. From such observations made in 1819. Dalton estimated the extreme height of the auroral arch at 102 miles. Sir John Herschel estimated the height of the auroral arch seen on March 9, 1861, at 83 miles. Observations of meteors afford another means of solving the problem. A height greater than any of those above mentioned has been deduced from observations of this sort. Lastly, polariscopic observations have led Liais and others to the conclusion that our atmosphere extends to a height of more than 200 miles from the Earth. The rarity of the atmosphere at such an elevation, assuming the law of diminution of density which prevails lower down to continue unchanged, would be altogether inconceivable. A quantity ot air which a healthy person of average height could draw into his lungs at a single inspiration would suffice, when so reduced in density, to fill a sphere exceeding in diameter the orbit of Jupiter.

within $b\ b'$ as at $o'$, the cones formed by lines from *his* eye to the Moon's limb and the corona's boundary, are respectively dark and illuminated in exactly the same way—that is, they must show that the same regions of the air are at once illuminated and in darkness.

This may fairly be regarded as impossible.

Yet even if this could be demonstrated, much more would still remain to be done before the 'atmospheric glare' theory could be regarded—I will not say as established—but as worthy of consideration. Until something of this sort has been done—and nothing of the sort has yet been attempted—we need not inquire how far those spectroscopic observations can be explained away which Professors Young and Harkness justly regard as of themselves demonstrating the non-terrestrial character of the coronal light.*

It appears, then, that whatever view we are to form of the actual constitution of the corona, we can at least have no doubt that it is a true solar appendage. There may not be any closer bond of union between the

---

* It will be observed that the above considerations dispose of that modified form of the atmospheric glare theory which Mr. Lockyer has recently put forward. Without inquiring here whether (as he asserts) a possible lunar action formed a part of the theory as originally propounded (see preface to the second edition of my *Other Worlds*), I must point out that such action does not render the theory at all more satisfactory. We have, if the theory is true, a certain region of our atmosphere illuminated and a certain other region dark, and the theory gives no explanation whatever of how this comes about. Moreover, supposing it did explain the matter for one observer at one moment, the explanation would not avail to show how in the case of another observer that same illuminated region would be dark and that same dark region illuminated at the very same moment.

material substance (of whatever sort) which emits the light forming the corona than exists between the nucleus of a comet and the comet's tail. But certainly the evidence seems to force on us the conclusion that a relation as unquestionable associates the corona and the Sun as that which compels us to regard the tail and coma as real appendages of a comet.

The corona thus viewed becomes one of the most important and interesting of all the phenomena of the solar system. We no longer have to deal with sunbeams shining through our atmosphere, or with mirages in some lunar envelope, but with luminous spaces of inconceivably vast extent. Let us consider what evidence we already have bearing on the nature of this wonderful solar appendage. We shall then more justly appreciate the interest attaching to those efforts which are being made to gain fresh information.

The general aspect of the corona, as described in the preceding pages, does not suggest the idea that we have to deal with a solar atmosphere. Those radial projections are not the appearances we should expect to find in an atmospheric envelope. Nor again is it easy to understand how the irregular masses of light, the spots resembling hanks of thread in disorder, and other peculiarities of a like nature, can be accounted for on the theory that the corona is a solar atmosphere.

But this view of the corona may be regarded as disposed of completely by the nature of the lines seen in the spectra of the coloured prominences. We have seen

(p. 295) that the gases forming the prominences probably exist at a comparatively low pressure—that almost certainly the pressure near the summits of the loftier prominences falls very far below the atmospheric pressure at the summit of Dhawala Giri and Mount Everest. Now, if we supposed the extension of the corona limited to that shown in Whipple's photograph, we should yet, on the supposition that the corona is an atmosphere, deduce a pressure far greater than this, let our estimate of the tenuity of the upper parts of such an atmosphere be what it (reasonably) may. But when we remember that under favourable circumstances the corona has been seen to extend to a distance very far exceeding the diameter of the eclipsed Sun, so that its depth (still regarding it as an atmosphere) would be more than a million of miles, or exceed sixfold the height of the loftiest solar prominences, we find ourselves compelled to reject the idea that we have indeed to deal with a solar atmosphere properly so called.

We conclude, then, that the matter (of whatever sort) existing where we know that the coronal beams extend, does not form part of a solar envelope. This being the case, we have to account for its subsistence or continuance in these regions by some other conception than that of the combined forces of attraction and molecular or atomic repulsion which keep an atmosphere in equilibrium (statical or dynamical, as the case may be). Now, there *may* be, and very probably there is, in partial question here the action of repulsive forces exerted by the Sun. Most unquestionably, as Sir

John Herschel has pointed out, the Sun *does* exert repulsive forces, and those of a magnitude inconceivably enormous. But, in the absence of any exact knowledge of the condition of the coronal matter, or of the nature and mode of action of solar repulsive forces, we must for the present limit our consideration to those forms of solar action which we can measure and estimate. We must inquire how matter swayed wholly or principally by gravitation might remain in the Sun's neighbourhood without being brought to his surface. We know that this can only happen when that matter is in motion with suitable velocity. Place any particle at rest at the distance of Mercury, and that particle would move off towards the Sun, and in the course of about fifteen days and a half it would fall upon that luminary. But endow the particle with Mercury's velocity, no matter in what direction (except directly towards the Sun's globe), and it will revolve around the Sun in an orbit having the same mean distance as Mercury's. It has become at once an attendant on the Sun, or we may say it is now a solar appendage. Take, then, a million, or a million millions, of such bodies and give them adequate velocities, even though in a million different directions, and the Sun forthwith has as an appendage a cloud of cosmical bodies, which will continue for ever, or for an indefinitely long period, as a cloud-appendage. It will not be fixed—the relations even of its several parts will not be fixed; on the contrary, the cloud will shift and fluctuate, its members aggregating here and segregating there; but as a clustering solar appendage it will be permanent.

Now, if we regard the corona as consisting, not of one such clustering appendage but of countless millions, severally insignificant perchance, but combining to form a solar aureola of enormous dimensions and of inconceivable real magnificence, we are at least not imagining a new feature of the solar system. On the contrary, we have now for some time had abundant evidence that such an appendage must exist. In fact, we have simply been brought by the consideration of the corona to perceive an effect produced by a portion of the solar system which had already been recognised.

It is admitted that the Earth encounters each year more than a hundred meteor systems. It is known that each meteor system includes countless millions of meteors; it is known that, besides the meteors belonging to systems, the Earth encounters myriads of others which, because they have not yet been associated with any known systems, are called *sporadic*; and it is known that the total number of meteors actually encountered by the Earth in the course of a single year is upwards of 2,700,000,000 if we count only those visible to the naked eye, while the number mounts up to 146,100,000,000 if we include those shooting stars which can only be seen with telescopic aid.* In that fine hoop of space which the Earth traverses each year—

---

* These results have been simply deduced from Professor Newton's estimates for the hourly motion of the Earth, extended to include the Earth's motion round the whole of her orbit.

a hoop having a circular cross section of the relatively insignificant diameter of 7,900 miles—these amazing numbers of meteors are annually encountered and for the most part consumed as they pass through our air; and yet there is seemingly no diminution in the supply. If we suppose that the space between the Earth's orbit and the Sun is supplied with equal richness, then undoubtedly we already begin to have evidence of some such solar appendage as we have supposed the corona to be.

But this is not all. We know that the meteor systems which cross our Earth's orbit have paths of great eccentricity, so eccentric in some cases as to carry the members of these systems much farther out into space than the orbit of distant Neptune. We know certainly therefore that the intersection of any meteor-orbit with our Earth is a mere coincidence; a coincidence which would be so unlikely, if there were but a few millions of such systems, that the laws of probability force on us the conclusion that there must be millions of millions of meteoric systems for each encountered by the Earth. We know again that, according to the laws of motion, the existence of multitudes of eccentric systems implies necessarily the aggregation of meteors in the Sun's neighbourhood.*

---

* This is true, notwithstanding the fact that in any single meteoric system, elliptical in figure, the average condensation (in long intervals of time) must be least near the Sun, owing to the relatively swift motions of the meteors there. For if we conceive the case of a vast number of eccentric meteoric systems in every variety of position, the diminution of aggregation due to this cause would be as the distances; but the increase of aggregation due to the volumetric diminution of concentric

On this account alone the average density of meteoric aggregation would be twice as great at half the Earth's distance, three times as great at one-third the Earth's distance, and so on.

But further, meteors have been found to be associated with comets—in this way—that the only meteor systems whose orbits have been determined travel on the orbits of known comets. If we assume this relation, observed in the only instances we have yet had the opportunity of examining, to apply generally, then we must infer a yet greater increase of meteoric aggregation near the Sun. For it has been shown that cometic perihelia aggregate more and more densely the nearer we approach the Sun.

Yet further, it would seem from the researches of Leverrier into the motions of the planet Mercury, that within the orbit of that planet there must be an increased aggregation of matter. For he has shown that certain perturbations of Mercury's motion may be explained by the existence of several small planets travelling around the Sun within the orbit of Mercury; and as we have had no proof of the existence of even a single object of this class, and as the observed effects can be equally well accounted for by the supposition that myriads of much minuter bodies exist, we may fairly infer on this account alone that the neighbourhood of the Sun is richly peopled with minute cosmical bodies.

shells of equal thickness around the Sun with approach to him, would be as the square of the distance. Hence results on the whole an average increase of aggregation proportional to the diminution of the distance.

Yet once more, Baxendell of Manchester has proved that certain periodic meteorological phenomena, detected by him in the tabulated statements of the best observatories, can be explained by the theory that there exists around the Sun a ring or zone or spheroid of matter (his theory does not insist, he says, on any special form) at about that same distance from the Sun which Leverrier has assigned to the family of intra-mercurial planets.

Assuming that the meteoric families which undoubtedly exist, and undoubtedly become more densely aggregated with approach towards the Sun, do thus—as Leverrier's theory and Baxendell's would alike suggest—grow even yet richer at about that distance from the Sun to which the corona extends when most favourably seen, we have *this* also to further strengthen our belief in the resulting brightness of the solar appendage thus formed—that these meteors would be severally illuminated with inconceivable splendour on account of their nearness to the Sun. If we add to this that those approaching most nearly to him would be rendered incandescent, if not vaporised, by the intensity of his heat, and that most probably electric discharges would take place between them* on account of the intense

---

* I am aware that under ordinary terrestrial conditions the electric discharge will not take place through a vacuum. But we have no evidence that an actual vacuum exists where the corona is seen. On the contrary, it is probable that multitudes of the minute bodies travelling past the Sun become vaporised, and so combine to form a moving vaporous region, whose constituent parts are continually changing as fresh matter arrives and as portions pass away to distances where they

energy of the solar action, we have, I think, abundant reason for expecting that when the Sun undergoes eclipse an aureola of splendour would be seen around him.

We have, then, two distinct lines of argument. We have been led by the consideration of the phenomena actually presented by the corona to the conclusion that multitudes of bodies too minute to be separately visible exist around the Sun; while we have been led by the consideration of what we know respecting multitudes of minute bodies actually travelling around the Sun to the conclusion that a corona or aureole of light would be seen around him during total eclipse. It seems clear, too, that all those peculiarities of the corona which have seemed to oppose themselves so obviously to other theories accord most perfectly with this. Save, perhaps, one only. If the corona is really crossed by radiating dark bars such as are shown in Mr. Gilman's picture, and described by several observers of total eclipses, then most certainly that phenomenon is not accounted for by the theory here put forward. The meteoric theory will account for a radial dark bar—by which I mean a bar directed in a straight line from the Sun—as an occasional phenomenon. But that such bars should be a characteristic phenomenon of the corona, or that in any single case the corona should be seen

---

can resume their normal condition. Changes also occur, in all probability, which no physical researches yet made can explain, since we have proof of peculiar forms of action in the formation and rapid growth of cometary appendages.

streaked with several such bars, is a phenomenon which nothing in the meteoric theory considered *per se* is calculated to explain.

Now this is a difficulty which must be faced by some better means than a mere attempt to negative the evidence. That erroneous observations are made from time to time is unfortunately true ; but when a characteristic phenomenon which does not seem likely to be merely *imagined,* is attested by trustworthy observers, a theory begins to wear a most questionable aspect which can only be supported by assigning those observations to illusion.

The difficulty in this case is that, even setting aside the objections (overwhelming as I think) which have been brought against all other theories but the meteoric one, we do not find in any of these theories the means of explaining this particular phenomenon.

Certainly no theory involving the existence of a lunar atmosphere can aid us in this strait ; because if the radial bars were due to the passage of the solar rays straight through such an atmosphere in lines touching the Moon's edge (and no other path would account for the observed phenomenon), then the extension of the lunar atmosphere necessary to account for the observed appearances would be about 200,000 miles, or the lunar atmosphere would extend nearly to the Earth.* We cannot for a moment imagine this.

---

* It is easy to see this by making a diagram showing the Moon and Earth, with the paths of the imagined rays extending along the boundary of the Moon's geometrical shadow, carrying the rays on until, as seen from the station of an observer on the Earth (and within the shadow-

Nor again can the 'atmospheric glare' theory explain this phenomenon, unless we suppose the Earth's atmosphere to extend nearly to the Moon's distance,—which is altogether incredible. Indeed, even if we admitted this, the bright rays between which the dark rays are seen ought to grow brighter and brighter with increase of apparent distance from the Moon, which is the reverse of what is actually seen.

It remains, then, that we should account for the phenomenon by the theory to which we have been led by other considerations. In fact, since the meteoric theory has been shown to accord so well with other phenomena, while it derives a negative strength from the obvious flaws in all the other theories, we are justified in accepting with a certain degree of confidence any explanation of these dark radial bars which the meteoric theory may point to. In other words, instead of feeling bound to explain these dark bars before admitting the meteoric theory, we may employ the meteoric theory to supply the explanation we require.

Now, there are two phenomena—one belonging to the solar system, the other to the Earth's economy—which seem likely to aid us in this matter. One is the appearance of comets' tails; the other is the aspect of auroral streamers. As respects the former phenomenon, it is to be remarked that the directive forces, whatever they may be, which cause the tails of comets

---

cone), they shall subtend an angle four or five times as great as that subtended by the Moon's diameter.

to project from the Sun, reside undoubtedly in the solar globe, and act undoubtedly with very great energy on certain forms of matter near him. Hence, as we have abundant reason for believing that the corona is not free from a certain association with cometary matter, we need not be altogether surprised if we find in the corona evidences of the same sort of action that we recognise in the formation and projection of comets' tails. Yet again, as respects the second phenomenon, we have the striking evidence afforded by the spectroscope to show that a resemblance of some sort exists between the coronal light and that of our auroras; so that we are justified in finding some resemblance, and even in conceiving that some association exists, between the long straight streamers which form so remarkable a feature of the aurora borealis, and those straight radial bars,* with dark intervening spaces, seen in the solar corona.

There is a circumstance which seems to render this relation more striking in the fact that the only explanation one can readily conceive of the observed characteristics of the auroral spectrum seems to bring us again upon that subject of meteoric astronomy which has thus far stood us in such useful stead. For

---

* It is worthy of notice that a difficulty exists in the relatively small section of the radial bars, both bright and dark; for this phenomenon would imply that the centre of that action to which these bars are due cannot have the dimensions of the solar globe which we are able to measure. It seems far from unlikely (since indeed we have other evidence corroborating such a view) that the central and more condensed portions of the Sun's mass may be the real seat of this intense repulsive action.

among the bright lines seen in the auroral spectrum is one agreeing in position with a line of iron, and it has been thought probable by Stewart, Angström, and others, that the light of the aurora is due in part to electrical discharges taking place in the upper regions of our atmosphere. But how can iron reach those upper regions save from meteoric visitants? and what can be more likely than that iron does actually reach the upper regions of our air in this way, when we consider how largely iron enters into the composition of nearly all the meteoric masses which have been so far subjected to analysis?

But it may be reasoned that if this is indeed the case, if solar action in the upper regions of the Earth's atmosphere (or terrestrial action excited in some way by the Sun) can cause these electrical discharges, then solar action exerted directly on similar material in the other parts of the Sun's domain ought to excite a similar luminosity, and that therefore we ought at night to see some traces—faint, it may be, but still recognisable—of this particular form of phosphorescence.

This amounts, in fact, to the consideration, that the limits of the corona as seen during total eclipse ought not to mark the real limits of the Sun's light-exciting action. And even supposing that but a small proportion of the coronal light is really due to this form of action—that is, to electrical discharges—it would still be likely that some signs of those meteoric systems whose illumination by the Sun has been here regarded as the cause of the corona, should be seen beyond the observed limits of that aureola of light.

Here again it happens (and I know no surer test of the justice of a theory) that we have been led to see that a certain phenomenon *should* be manifested, which actually *is* a familiar phenomenon of the heavens, and which would most assuredly have required explanation if it had not thus been led up to.

For precisely in that region where we should expect to find a faint gleam of light—precisely where the known relations of the planetary scheme would lead us to look for an abundance of meteoric material, there appears that mysterious luminosity known as the Zodiacal Light. And just as our reasoning has led us to regard the meteoric appendage of the Sun—an appendage really extending far beyond the orbits of the most distant planets—as variable in configuration, however constant when regarded as a whole, so we find the zodiacal light varying from year to year in brightness, and extent, and position. Its light, again, presents that faint tinge of pink which has been recognised in the corona and forms so marked a phenomenon of the aurora. It has even been observed to fluctuate in brightness and to be traversed by flickerings and coruscations—to thrill, as it were, responsive to mysterious influences, precisely as we should expect on the supposition that it is analogous to the aurora. *But lastly, as if to remove all doubt, comes the fact* *that the light of the zodiacal gleam gives the very same spectrum as the aurora, a spectrum which, as we have already seen, resembles closely that of the corona.*

I have said that if we were not led by our consideration of the corona to anticipate as it were the existence of the zodiacal light, we should have to explain this latter phenomenon. Let us view the zodiacal light apart for a moment.

We have a glow or radiance which is commonly seen along the zodiac,—that is, in the region of the sky where planets are to be looked for. This glow obeys all the usual laws observed in the motion of celestial bodies. It rises and sets precisely as the fixed stars and planets are observed to do. If we travel towards or from the equator, it is seen higher or lower, precisely as the part of a planet's path near the Sun's place would shift. It presents all those peculiarities, in fine, which force on the astronomer the conclusion that he has to do with an extra-terrestrial phenomenon, and a further peculiarity showing that it is a phenomenon specially associated with the planetary scheme.*

---

* Space forbids my entering here into a consideration of the arguments by which all other theories of the zodiacal light may be negatived. In the *Monthly Notices* for November of the present year there will be (I write this in October) a paper of mine, showing by mathematical considerations of a very plain kind that the only admissible theory of the zodiacal light is that same theory which I have here urged in explanation of the corona,—the theory, namely, that there exists around the Sun a region of meteoric matter continually changing in configuration and constitution, owing to the continual arrival and departure of individual meteors. Every peculiarity of the zodiacal light is in accordance with this view, and many of its features, as also many features of the

It has therefore been regarded by every astronomer who has studied the subject with due attention, as indicating the existence of a lens-shaped region around the Sun within which cosmical matter is strewn with considerable profusion.

Now, regarding the zodiacal light in this way, and considering its general aspect when seen under favourable conditions, the conclusion is forced upon us that the density of aggregation of this cosmical material increases with proximity to the solar globe. For we see that the borders of the zodiacal light are very much fainter than the central part or core of the gleam. We see, again, that the light grows brighter and brighter towards the horizon,—that is, with proximity to the place of the Sun. And these relations are observed even in those countries where at certain seasons the zodiacal light is vertical, and where therefore the actual arc separating its base from the Sun's place is least at the time when the light is first visible after sunset or before sunrise.

The obvious conclusion is, that if the zodiacal light could be traced yet farther towards the Sun's place this increase of lustre would continue, and that therefore all round the Sun there would be seen a luminosity corresponding precisely with the observed aspect of the corona. So that *again* we are led by the con-

corona, seem individually explicable on no other hypothesis; while assuredly no other theory can account for *all* the observed peculiarities of these remarkable phenomena of our system.

sideration of a well-recognised feature of the solar system to the conclusion that the corona is a phenomenon to be expected when the Sun is totally eclipsed, rather than one whose appearance should be regarded as surprising and perplexing.*

In conclusion I would remark that while the exact nature of the corona remains—and perhaps may long remain—a mystery, I know of few instances in which the general nature of a phenomenon has seemed more satisfactorily exhibited than in the case of the corona and zodiacal light. We have the strongest negative

---

* To the considerations above adduced, I may add some which are touched upon in a paper of mine which appeared in *Fraser's Magazine* for February last:—'There is one feature of comets' tails,' I there point out, 'which has long since attracted attention, and will remind the reader of the peculiarities common to the zodiacal light and the aurora. I refer to the sudden changes of brilliancy, the flickerings or coruscations, and the instantaneous lengthening and shortening of these mysterious appendages. Olbers spoke of "explosions and pulsations, which in a few seconds went trembling through the whole length of a comet's tail with the effect now of lengthening now of abridging it by several degrees." And the eminent mathematician Euler was led by the observation of similar appearances to put forward the theory "*that there is a great affinity between these tails, the zodiacal light, and the aurora borealis.*" The late Admiral Smyth, commenting on this opinion of Euler's, remarks that "most reasoners seem now to consider comets' tails as consisting of electric matter" (that is, I suppose, indicating the occurrence of electric discharges), adding that "this would account for the undulations and other appearances which have been noticed—as, for instance, that extraordinary one seen by Chladni in the comet of 1811, when certain undulatory ebullitions rushed from the nucleus to the end of the tail, a distance of more than ten millions of miles in two or three seconds of time." To this may be added the theory suggested by Sir John Herschel, that the matter forming the zodiacal light is "loaded, perhaps, with the actual materials of the tails of millions of comets, which have been stripped of these appendages in the course of successive passages round the immediate neighbourhood of the Sun."'

evidence against all other theories but one, and that one theory is confirmed by line after line of positive reasoning. To doubt what general view we should form of the corona and zodiacal light under these circumstances seems to me to savour—*not* of that wise caution which prevents the true philosopher from overlooking difficulties, but rather—of an inaptitude to estimate the value of evidence. As to details we may be doubtful. Other matter than meteoric or cometic matter may well be in question; other modes of producing light, save heat, electricity, or direct illumination, may be in operation in this case; and lastly there may be other forces at work than the attractive influence of solar gravity, or the form of repulsive force evidenced by the phenomena of comets. As regards, also, the true shape and position of the coronal and zodiacal appendage—and yet more as regards its variations in shape—we may still have much to learn. But of the general fact that the corona and zodiacal light form a solar appendage of amazing extent and importance, that they are not merely terrestrial phenomena, but worthy of all the attention astronomers and physicists can direct to them, it seems to me that no reasonable doubts can any longer be entertained.

## CHAPTER VII.

### PHYSICAL CONDITION OF THE SUN.

In the course of the last four chapters a number of facts bearing on the physical condition of the Sun have been dealt with at greater or less length. Considered separately these facts are full of interest and seem to afford somewhat satisfactory information on the points they severally relate to. We feel little difficulty, for instance, in giving a general interpretation to the dark lines of the solar spectrum, regarding them as undoubtedly due to the existence of the vapours of certain metallic and other elements in the solar atmosphere. We regard with a certain confidence, again, the conception that the spots are depressions of greater or less depth, and further that the light received from the umbra of a spot shines through absorbing vapours, some of which exist at a greater pressure and at a lower temperature than over the rest of the photosphere. We are able, also, to form certain sufficiently definite opinions respecting the prominences, more particularly as regards the pressure at which their substance exists and the motions to which they are subjected. While, lastly, the corona has been studied with results which cannot but be regarded as trustworthy.

But when we attempt to combine these several results, and further to determine what the general condition of that orb may be which presents these several features, we recognise at once that a problem of enormous difficulty lies before us. The more we have learned respecting the Sun, nay the more we have learned respecting those physical laws by which we are to interpret solar phenomena, the more insuperable have our difficulties become. It was easy to theorise when as yet but little was known. It was easy to suppose that the few physical laws we imagined we understood sufficed to account for all the phenomena presented by the solar orb. But as one fact after another has been discovered, the true complexity of the problem has been revealed to us; and as the physical laws which it is in our power to discuss and experiment on have been more carefully studied, we have begun to recognise how very limited our experience has hitherto been. It is not too much to say that theories respecting the Sun's physical condition which would have been regarded twenty years since as deserving of careful study, have now no worthier standing in science than the idea of Anaximander that 'the Sun is a great vessel filled with fire, at the top of which is an opening through which the fire escapes.'

It is easier to consider those facts which have revealed to us the enormous difficulty of the problem we are upon, than to present any considerations tending to render our conceptions clearer.

In the first place, we recognise the fact that in the Sun the elements exist in conditions altogether different from those we are familiar with. This is true of all orders of elements, from those whose normal condition (as recognised by us) is gaseous, to those which at ordinary or even very high temperatures remain solid or liquid. For instance, metals which we can volatilise indeed in small quantities, and by the aid of special contrivances, but which yet we can only experiment upon (properly speaking) when they are in the solid or liquid state, are present in the Sun as glowing vapours. But also gases which no amount of pressure or refrigeration we can command will cause even to show signs of approaching the liquid condition, probably exist in certain portions of the solar globe as liquids or even as solids. On the one hand, then, we have an inconceivably high temperature volatilising our most fixed elements—nay, for aught we know, perhaps dissociating substances which we regard as elements into their true primary constituents; on the other we have an inconceivably high pressure at a relatively inconsiderable distance beneath the photosphere, reducing our so-called perfect gases into the liquid or solid form.*

---

* I do not know that we have any sufficient evidence that hydrogen, oxygen, nitrogen, and other gases which we call perfect, are really exceptions to the general rule that all substances are capable of assuming —under suitable conditions—the solid, liquid, and gaseous form. It is true that no pressure or refrigeration we can apply causes these gases to exhibit the least trace of those qualities which the imperfect gases exhibit at ordinary temperatures and pressures. In other words, the perfect gases, as their title implies, obey perfectly (1) the law associating density and pressure; and (2) the law according to which the relation of specific

In the next place, we have learned of late to recognise on how very doubtful a basis many of the received axioms (almost) of physical science have been placed. No laws of science were perhaps more thoroughly accepted than those which were supposed to distinguish the solid, liquid, and gaseous states from each other. All physicists believed that a definite and well-marked change of condition necessarily accompanies the process by which gas passes into the liquid state, and very few were disposed to doubt that a correspondingly distinct line of demarcation separates the liquid from the solid state. Yet the researches of Dr. Andrews have recently shown that under certain conditions carbonic acid gas may be made to pass by absolutely insensible gradations from an undoubtedly gaseous to an undoubtedly liquid state; and it is recognised that what has been proved in the case of a single gas is in all probability true of all gases and vapours. It has been also rendered highly probable that under suitably

heats at constant pressure and constant volume remains constant. But it is known that imperfect gases at ordinary temperatures and pressures seem very closely to obey these laws, though as they approach more and more nearly to the circumstances under which they become liquid (whether through increased pressure, simple refrigeration, or both combined) they depart in a marked way from these laws. And it is probable that if the imperfect gases could be experimented on at enormously high temperatures and low pressures, they would be found to obey the above-named laws as perfectly to all appearance as the perfect gases do. All we can assert respecting the perfect gases is that none of the processes of combined pressure and refrigeration hitherto applied—perhaps none we are capable of applying—bring them even so far towards the state under which they would become liquid as the imperfect gases are brought under ordinary pressures and temperatures.

great pressure the passage of a molten metal to the solid form, or conversely the melting of such a metal, may take place in a gradual manner, so that at a certain stage of the process it shall be impossible to say whether the metal or certain portions of it be liquid or solid.

Another physical law had been thought to be thoroughly established. It had been supposed that the whiteness (or true incandescence) of flame was due to the presence of minute particles of incandescent matter. But Frankland has shown that with increase of pressure the faintly luminous light of burning hydrogen may be rendered bright, and that by sufficiently increasing the pressure the spectrum of the light becomes continuous. So that what had been supposed the most marked characteristic of incandescent solid and liquid bodies, is thus shown to be a possible characteristic of the light of glowing gas. Thus the whole basis of our reasoning (which had been thought so sound) respecting the actual condition of the solar photosphere, whether as evidenced by direct observation or by analysis with the spectroscope, has been shaken.

Yet again we have learned to recognise the fact that the imagined limits to the rarefaction at which gases may subsist, as such, had been placed far too low. Our experimenters have gone indeed very far towards the production of an actual vacuum, without obtaining any evidence that the almost infinitesimal quantity of gas which can alone remain in the so-called vacuum-tubes, behaves otherwise than at appreciable pressures.*

It will be seen at once how importantly this bears on the subject of solar physics, since it compels us to reconsider all our ideas respecting the probable limits of atmospheric envelopes; and the most difficult questions of solar physics are precisely those which depend on this matter. Unfortunately the observed fact gives us no new means at present of forming a satisfactory opinion as to atmospheric limits. Even if it should be regarded as demonstrating that there is no real limit to atmospheric extension, we should still be no nearer than before to a determination of the atmospheric pressures at the surface of the Sun or of any planet except our own Earth; and this is the problem which chiefly concerns us here.†

---

* They have carried the rarefaction so far that the electric spark will no longer traverse the tenuous medium; and some have been led to suppose that when this state of things has been brought about there must be an actual vacuum. No such conclusion can be regarded, however, I will not say as demonstrated, but as probable or even conceivable. All the evidence we have tends to show that an absolute vacuum is as imaginary a conception as the philosopher's stone or the perpetual motion. Take the most stable and dense metal, platinum, and conceive that for a moment in the heart of a mass of such metal there was a vacuous space; then in an instant that space would be occupied. For, even setting aside the probability that the most solid metals undergo an indefinitely minute but still real dissipation at their surfaces, corresponding to that dissipation which ice undergoes at the lowest temperatures yet experimented on—setting aside, I say, this probability, there yet remains the certainty that in the intimate molecular structure of the platinum a perfectly free communication exists between the imagined space within and the space without. The communication is not, indeed, such as suffices for the conveyance of certain forms of matter or of motion; but that it is none the less real on that account cannot be questioned.

† It would, indeed, be pleasant to theorise on this matter,—to conclude,

Still further, in considering the Sun's physical condition, we have to discuss the effect of motions wholly surpassing in velocity any that we are familiar with on Earth, and therefore *à fortiori* any that we can experiment upon. Setting aside the fact that matter from without must continually be falling upon the Sun (in whatever condition) with velocities such as have been dealt with in Chapter II., we have the observed fact that movements fairly comparable with these have been recognised in the very substance of the Sun by the aid of spectroscopic analysis. Now, what means have we for determining the probable effect of motions of 100 miles per second taking place even among substances in conditions such as we are familiar with? We know the effects of certain velocities; we see the bullet melted as it reaches the target, the meteor vaporised as it speeds through the air. But we have no means whatever of determining what effects would be produced by velocities enormously exceeding even the inconceivable velocity of many meteors. When to this we add that the swift motions referred to take place amid depths of vaporised metals, and, for aught we know, over seas and continents of liquefied and solidified gases, we may well shrink from the task of attempting—at least in the

---

for example, that every planet could have just so much atmosphere as corresponded to the range of its attractive influences; and consequently to deduce the atmospheric pressure upon the Sun. But no confidence could be placed in such theories. For, on the implied supposition, quite other forces than attraction would have to be considered. And, further, it would be an inevitable consequence of such a state of things that as the planets changed their relative positions, the atmospheric pressure at the surface of each would vary.

present state of our knowledge—to estimate their probable effects.

I confess, therefore, that at this stage of my subject I am very far from sharing that confidence which I find some men possess in dealing with problems of solar physics. I shall not pretend to place all the phenomena in that due order in which they appear in the theories hitherto propounded. I can only look on with a sense of bewildered admiration while the professors of rival theories exhibit the physical habitudes of the Sun as obviously explicable according to contradictory hypotheses. I must admit that it seems to me that only a very energetic forgetfulness of a large proportion of the evidence can account for the adoption of these theories. I must content myself, therefore, with an exceedingly brief statement of certain general relations, which are all that I find satisfactorily exhibited by what has as yet been learned respecting the Sun.

We have in the Sun a vast agglomeration of the elements we are familiar with on Earth; and this vast agglomeration is subject to two giant influences, producing in some sort opposing effects—viz., a temperature far surpassing any we can form any conceptions of, and a pressure (throughout nearly the whole extent of the solar globe) which is perhaps even more disproportionate to the phenomena of our experience. Each known element would (beyond all question) be vaporised by the solar temperature at known pressures; each would (there can be little question) be solidified by the vast solar pressures, did these occur at known temperatures. Now, whether under these circumstances the laws of gaseous diffusion prevail where the elements *are* gaseous in the solar globe; whether where liquid matter exists it is in general bounded in a definite manner from the neighbouring gaseous matter; whether any elements at all are solid, and if so under what conditions their solidity is maintained and the limits of the solid matter defined—all these questions are such as we *must* answer before we can form a satisfactory view of the solar constitution; and yet they are questions which we have at present no means of answering. Again, we *must* learn how far combustion, properly so called, can take place within the Sun's mass, and whether those processes which we recognise as combustion are the only processes of combustion which can actually take place there. For aught that is yet known, the intensity of the forces at work upon and within the Sun may wholly prevent the occurrence of any processes of combustion familiar to ourselves; while other processes of true combustion altogether unthought of by us may be in continual action.

Assuming, however, that some general resemblance exists between the processes at work upon the Sun and those we are acquainted with (the wildest assumption possible), we may imagine that the various elements in the solar substance ordinarily exist down to certain definite levels in the gaseous form, at lower levels (definite for each) in the liquid form, at yet lower levels in the solid form. That part of each element which is gaseous must again be divided into two portions—that whose light is capable of giving characteristic spectra of lines or bands (these spectra being, however, different for portions lying at different depths), and that lower portion whose light is capable of giving a continuous spectrum.

Now, here we approach the great difficulty of interpreting the results of the spectroscopic analysis of the Sun. We have no means of learning whence that part of the light comes which gives the continuous spectrum. When we recognise certain dark lines, we know certainly that the corresponding element exists in the gaseous form at a lower temperature than the substance which gives the continuous spectrum. And so, also, we can interpret the appearance of bright lines. They show beyond question that the corresponding element exists in the gaseous form at a higher temperature than the substance which gives the continuous spectrum. But as regards that continuous spectrum itself we can form no such exact opinion. It must be remembered that a substance giving a continuous spectrum is not necessarily opaque to light from a substance at higher temperature also giving a continuous spectrum. It is capable of exercising a general absorption, but not necessarily (nor probably, under such conditions as exist in the Sun) of exercising an absorption at once general and complete. Hence we have no means of determining how great a depth of the solar substance is concerned in sending out the light which gives the continuous background of the spectrum. This light may come from the surface layers only—but it may be a shell whose thickness forms no inconsiderable aliquot part of the Sun's diameter.* And the reversal of the lines of certain elements, although it cannot take place at such excessive depths, may yet take place very far below the visible limits of the photosphere. For, as I have already shown, a depth of a few hundred miles would be wholly inappreciable in the most powerful telescope (spectroscopically armed), and so no peculiarities would be recognised as the result of processes taking place within such distances of the solar surface, however diligently the edge of the solar disc might be examined with the spectroscope. And, further, as respects the examination of the Sun's edge, on which so much stress has been laid, it is far from unlikely—if, indeed, it is not to be regarded as certain—that the visible edge of the solar disc lies considerably above the true limit of the photosphere. That light at the Sun's edge which seems to belong to the hemisphere of photospheric matter turned towards us comes probably from parts of the photosphere which lie really beyond the borders of that hemisphere, and are simply brought into view through the refractive power of the lower layers of the solar atmosphere. I am not here venturing a mere opinion or conjecture,—though I profess no certainty of conviction in the matter. We have the evidence of

---

* This is in no way opposed to the evidence adduced by Sir John Herschel from the apparent uniformity of light derived from a glowing and transparent liquid not uniformly deep; for every part of such a liquid (at least in Sir John Herschel's experiment) is at appreciably the same temperature. If the lower layers could be heated to a higher temperature, the effect of depth would become apparent.

facts—observed by Carrington, Secchi, and others—showing that the motions of the spots across the solar disc are really affected, as respects apparent *rate*, by the refractive power of the solar atmosphere. And it is impossible to doubt that if the apparent place of a spot can be affected in this way, then the solar regions beyond that hemisphere which is turned towards us at the moment, must be brought into view and form the real limits of the solar disc.*

I am sensible that I am not making definite statements as to the Sun's condition, but only stating difficulties. The difficulties are real, however, not imaginary, and ignoring them can serve no useful purpose.

When we turn to the details of the solar orb and its surroundings, we find the evidence slightly more definite; but still great difficulties surround us. In the course of the several chapters bearing on these matters nearly all the known facts which bear directly on the views we are to form respecting the Sun's physical constitution have been discussed as they have been described, so that but few words about the several solar features are here called for.

Taking the actual telescopic aspect of the solar spots

---

* This really amounts to saying that the Earth, if viewed from the solar photosphere, would be visible above the solar horizon (as our Sun is after real sunset and before real sunrise) when the geometrical line to her passed below that horizon. We cannot doubt that the solar atmosphere would exert this refractive effect at least as powerfully as our own; and if the Earth could be seen in this way from parts of the Sun really turned away from her, then certainly those parts of the Sun must be visible from the Earth; for a visual ray passes along the same path from whichever of its extremities we suppose it to travel.

and their surroundings, a perplexing series of problems is suggested. These problems are indeed so perplexing as abundantly to justify the disagreement hitherto found among theorisers. Admitting the spots to be depressions, what is the real disposition of the matter which produces the appearances we call faculæ—penumbra—umbra—nucleus? The penumbra may belong to a lower layer; but the general arrangement according to which the willow-leaves on the penumbra point inwards towards the umbra seems to indicate a real connection between the penumbra and the faculous bordering. This arrangement is indeed sometimes so marked that one is led to imagine that the so-called willow-leaves are filamentous bodies which usually hang in a nearly vertical position and so appear nearly round, but when thrust aside during the formation of a spot hang nearly horizontally, the ends which had been lowest floating like streamers towards the region whence they had been removed. If we could but conceal from ourselves a large portion of the evidence we have (or else explain it away) this view might be insisted upon with pleasing confidence; but as a matter of fact it merely serves to indicate the impression produced by certain phenomena, and has at present no value whatever.*

A great difficulty lies in the fact that we have no

---

* Lest I should here be supposed to be too curtly criticising the views of others, let me hasten to say that the fancy thus summarily rejected is my own, and, so far as I know, as original as it is probably valueless. Yet I have not introduced it without a purpose. There is at least as much evidence in its favour as in favour of many theories which have been very confidently put forward.

clear evidence to show whether the Sun-spots are formed by forces acting from without or from within. Here I set on one side the theory that in a spot we see a region where a great heat has dissolved solid or liquid or cloudlike matter forming the photosphere, and that thus the intensely hot, but feebly radiating gaseous nucleus of the Sun (according to this theory) is disclosed. Kirchhoff has fairly disposed of this theory by showing that this intensely hot nucleus would be transparent to the light from the farther side of the Sun, and that therefore no spot could appear unless two openings on opposite sides of the Sun happened to be in the same visual direction.* I refer now, not to this or similar theories, but to the definite problem, whether the seat of that action which leads to the formation of a spot lies below or above the level of the photosphere. The spectroscope shows that a spot is a region where certain gases exist at a lower temperature than in other parts of the Sun. But whether this low temperature results from the expansion of compressed gas erupted from the Sun, or from the fact that matter has reached the Sun from outer space, remains as yet altogether unknown.

---

* Fr. Secchi was the original propounder of this theory (not M. Faye, to whom it is usually ascribed). The theory really does account for many observed features of the solar spots, but it is none the less untenable. Fr. Secchi's answer to Kirchhoff's objection would seem to indicate that he has not recognised the exact force of that objection. He says it is not true that a gaseous nucleus would be perfectly transparent to rays from the further side of the Sun, for we see that our own atmosphere absorbs light as well as heat. Kirchhoff's argument is that the solar nucleus would be transparent on account of its existing at a higher temperature than the photosphere—according to the theory at least which he deals with.

Again, as to the prominences, it seems to be demonstrated that they are due to some form of eruption and only assume the cloud form after the eruption which gave them birth has ceased. But what are the circumstances which give birth to these eruptions, what the nature of the layer (Zöllner's 'Trennungschicht') beneath which the eruptive action is prepared, and what the actual depth whence the erupted matter springs, we have very little to show.

And lastly, as to the corona and the general relations involved in the access of external matter from the interplanetary and intersidereal spaces to the neighbourhood of the Sun's globe, we have, I apprehend, small means of forming an opinion. The condition, indeed, of the space which lies immediately around the Sun is very little understood by us. It may be that in the study of the corona during total eclipses we may find a means of answering the many perplexing questions associated with this matter. It may even be that new appliances may enable us to study the corona when the Sun is not eclipsed, and so to learn whether systematic processes affecting the Sun's economy are at work in the region immediately surrounding him. At present our information on this subject is meagre in the extreme; and our means for acquiring information are far from promising. Here, as in so many matters related to the physical constitution of the Sun, we must perforce wait until our experimental knowledge and our instrumental means have been very largely increased.

# CHAPTER VIII.

## *THE SUN OUR FIRE, LIGHT, AND LIFE.*

FEW of the results of modern scientific research are more remarkable than the recognition of the real extent of the influence which the Sun exerts upon the Earth. Of old the Sun's power as ruler over the seasons, his action upon vegetation, and other like influences, were recognised in a vague and general way. But men were far from regarding the Sun as the true source of many forms of force which seem almost equally important. Still less were they prepared to trace his influence in nearly every kind of action or mode of motion taking place upon our globe. It is the most striking feature of recent scientific research that it has taught us to see in nearly all terrestrial phenomena the action of a certain proportion of Sun-force.

We owe to the greatest astronomer of our time—Sir John Herschel—the first definite enunciation of this great principle. 'The Sun's rays,' he wrote in 1833, 'are the ultimate source of almost every motion which takes place on the surface of the Earth. By its heat are produced all winds, and those disturbances in the electric equilibrium of the atmosphere which give rise to the phenomena of lightning, and probably also to terrestrial action and the aurora. By their vivifying action vegetables are enabled to draw support from inorganic matter, and become in their turn the support of animals and man, and the source of those great deposits of dynamical efficiency which are laid up for human use in our coal strata. By them the waters of the sea are made to circulate in vapour through the air, and irrigate the land, producing springs and rivers. By them are produced all disturbances of the chemical equilibrium of the elements of nature, which by a series of compositions and decompositions give rise to new products, and originate a transfer of materials. Even the slow degradation of the solid constituents of the surface, in which its chief geological change consists, is almost entirely due—on the one hand to the abrasion of wind or rain and the alternation of heat and frost, on the other to the continual beating of sea-waves agitated by winds, the results of solar radiation. Tidal action (itself partly due to the Sun's agency) exercises here a comparatively slight influence. The effect of oceanic currents (mainly originating in that influence), though slight in abrasion, is powerful in diffusing and transporting the matter abraded; and when we consider the immense transfer of matter so produced, the increase of pressure over large spaces in the bed of the ocean, and diminution over corresponding portions of the land, we are not at a loss to perceive how the elastic force of subterranean fires, thus repressed on the one hand and released on the other, may break forth in points where the resistance is barely adequate to their retention, and thus bring the phenomena of even volcanic activity under the general law of solar influence.'

Since this was written men of science have learned to enounce the complete law of the 'conservation of solar energy,' as applied to the organic and inorganic world. That which was put forward in a general way by Sir John Herschel has been made the subject of special scrutiny. We have learned how to weigh and measure the Sun's action and the force-supplies which we derive from it.

Let us take first the supply of heat the Earth derives from the Sun. We shall have much to excite our wonder, whether we regard the real vastness or the relative minuteness of this supply.

From the researches of Sir John Herschel it appears that the direct heat of the Sun, if 'received on a surface capable of absorbing it and retaining it, would suffice to melt an inch of ice in thickness in 2h. 13m.;' and he thence calculates that no less than 26,000 tons of ice would be melted per hour by the heat actually thrown on a square mile exposed at noon under the equator. This amount must be multiplied fifty million times to correspond to the heat actually received by the Earth's globe during a single hour. Pouillet obtained results not differing very greatly from these. He calculated that an interval somewhat greater than 2h. 13m. would be required to melt a layer of ice one inch thick.* Expressing his result according to a somewhat different method, he states, again, that if the Sun's heat were distributed uniformly over the Earth's surface 'it would in one year suffice to liquefy a layer of ice 100 feet thick, or to heat an ocean of fresh water sixty-six miles deep from the temperature of melting ice to the boiling point.'

Yet this enormous annual supply of heat is but the 1-2,138,000,000th † part of that which the Sun actually radiates into space in the course of a year. All the planets of the solar system are able to intercept but about the 227-millionth part of the heat actually emitted by the Sun. There is a fine passage in Herschel's 'Outlines of Astronomy' which shows how enormous is the amount of heat deduced by increasing in the ratio above indicated the supply of heat actually received from the Sun by the Earth:—'Supposing a cylinder of ice forty-five miles in diameter to be continually darted into the Sun *with the velocity of light*, the heat now given off constantly by radiation would then be wholly expended

---

* The actual relation between Pouillet's and Herschel's results may be thus expressed. Sir John Herschel deduced 43·39 feet as the thickness of ice which the Sun is capable of melting per minute, supposing the ice continually applied to the Sun's surface (and the water produced by its fusion continually carried off). Pouillet deduced 38·7 feet per second. Sir John Herschel says that 40 feet may be regarded as a probable mean. (It will be noticed that, with characteristic modesty and generosity, he places the mean much nearer to Pouillet's value than to his own.)

† It is singular how persistently the number 2,300,000,000, calculated by Mayer, maintains its ground in scientific treatises. This number was correctly deduced from the old value of the Sun's distance. But the above is the true value, according to the best modern estimates of the solar parallax.

in its liquefaction on the one hand, while on the other the actual temperature at the Sun's surface would undergo no diminution.'

The luminosity of the Sun's surface is more readily estimated than the heat, since the intrinsic brilliancy of a self-luminous substance is in no way affected by distance; and we have only to take into account the effect which our own atmosphere may have in diminishing the apparent brightness of the Sun in order to form an accurate estimate of the intrinsic brilliancy of the Sun's light. Comparisons have been instituted directly between the light of the Sun and that of known terrestrial lights. It has been found that the most intense light we can produce appears absolutely black by comparison with the brightness of the solar orb. It has been estimated that the intrinsic brilliancy of the Sun's surface exceeds more than 146 times the brilliancy of the lime-light, and 32,700 times that of a sperm candle. In order to conceive the real amount of light to which a body close by the Sun—within a foot, say, of the photosphere—would be exposed, we must conceive the amount of light we receive in the full splendour of a summer's day increased in the same proportion that the whole hemisphere of sky exceeds the solar disc,—besides, of course, a further addition corresponding to the proportion in which the brilliancy of the solar disc, if viewed without the interposition of any atmosphere, would exceed the actual brilliancy observed on a summer day.

Of the chemical activity of the solar rays, it is not in our power to speak with so much confidence, since we have not as yet measured the Sun's power in this respect in a way which enables us to pronounce on its real extent. We can compare the intensity of the Sun's chemical action with that of terrestrial lights; but we have not yet found a means of determining its value as compared with those forces which the chemist more ordinarily employs to produce chemical changes.

It is worthy of notice, as respects the last two forms of solar activity, how large a share of the force we derive from the Sun is obtained through their action. This will be apparent when we remember the important bearing of the processes of vegetation on the wants of the human race. 'Nature,' says Mayer, 'has proposed to herself the task of storing up the light which streams earthward from the Sun—of converting the most volatile of all powers into a rigid form, and thus preserving it for her purposes. To this end she has overspread the Earth with organisms, which, living, take into them the solar light, and by the consumption of its energy incessantly generate chemical forces. These organisms are *plants. The vegetable world constitutes the reservoir in which the fugitive solar rays are fixed, suitably deposited, and rendered ready for useful application. With this process the existence of the human race is inseparably connected.*

And even if we regard the effect of the Sun's heat as exerted upon the oceans and continents of our globe, we find that a large proportion of that which is eventually utilised by man, in one way or another, is first made subservient to the processes of vegetation. When the Sun's rays are poured down upon the ocean, or on parts of the Earth's surface in which water is abundant, the heat raises into the atmosphere large quantities of aqueous vapour. And this vapour, rising by reason of its extreme lightness, reaches eventually a region where it is condensed into *clouds.* Again, the heat of the Sun producing various effects, according to the nature of the regions on which it falls, gives rise to those differences of temperature which result in the generation of *winds.* By the agency of winds the clouds are transferred from the place of their formation to regions which require to be nourished by copious showers. And thus winds and clouds combine to support vegetation. The winds convey the clouds from place to place, and the clouds themselves, in the expressive language of Scripture, 'drop fatness on the earth.'

It is worthy of notice, too, that besides the action of the Sun in supporting vegetation at the present time, it was the same form of action exerted in long-past ages which resulted in storing up for our use those vast supplies of energy which are contained within our coal-mines. In other words, what may be called our *force-principal* is as fully due to the Sun's action (direct or indirect) in promoting vegetation as that *force-interest* which we derive each year from the Sun's seasonal action.

And here I may be permitted to dwell on considerations which, though bearing rather on the economy of our Earth than on the general subject of solar physics, yet illustrate in a significant manner the work which the Sun has been appointed to do. I may premise, indeed, that we have no means of determining what the Sun's influence on the other planets may be, however clear it may appear to us that we are not the only, nor even the chief, recipients of those stores of force he lavishes so abundantly. It is on this account that while I give to this treatise a title indicating the Sun's position in the solar system, I deal only in this chapter—the sole one bearing on the Sun's office—with his position as *our* fire, light, and life. If in the considerations I am about to urge the Earth only seems concerned, it is none the less probable that results affecting the economy of the whole planetary scheme are in truth illustrated.

We are accustomed to look upon the Earth as an inexhaustible storehouse whence all our wants may be supplied. Year after year we till the soil, and still there is no lack in the growth of all the vegetable productions needed by man; nor do our flocks and herds diminish, notwithstanding the enormous supplies of flesh-meat we are continually consuming. Taking the whole Earth, it is probable that the yearly produce of agricultural and pastoral labours increases at even a higher rate than that at which the human race is increasing, so that were man content, as in old times, to draw upon the Earth's stores for the supply of his ordinary wants, there would be little fear of that store being ever exhausted.

But of late a change has passed over the aspect of the world. On every side a multitude of new inventions, and with them a multitude of new wants, are making their appearance. The stores which had been garnered up during long past ages of the Earth's history are being consumed with a rapidity which has already begun to alarm our men of science. It is true, indeed, that there is as yet little room for fearing that the terrestial storehouse will soon be cleared of its contents. Even if the coal-mines of the world should be exhausted, there are still other force supplies; and doubtless the present rate of consumption might be continued, or even an increased rate maintained, for a period which seems indefinitely long when compared with the short span of life allotted to man.

But, after all, what are a thousand years, or even several thousand years, when viewed with reference to the history of the globe on which we live? If it could be shown that within two or three thousand years man will have exhausted all the stores of force which exist within the Earth, it surely might be urged with fairness that the present rate of consumption is unduly —selfishly great; that the wants of future races should be considered, and that a check should be put upon those processes of over-rapid advance on which we are in the habit of priding ourselves. Precisely as we should hold it to be blameworthy that a rich man should use, merely for purposes of luxury or convenience, that which could be shown to be absolutely essential to the existence of a large number of his fellow-men, so it might fairly be held to be wrong for the present inhabitants of the Earth to exhaust, in contrivances intended to add to the luxuries or conveniences of life, those stores which are absolutely necessary to the well-being of future races.

In dealing, for example, with the question of terrestrial coal supplies, it will not suffice to point out that for a thousand or several thousand years they may be drawn upon as at present, or even more largely, without exhaustion. The thousand or thousands of years will pass as surely as those which have already passed, and the wants entailed by our wastefulness will be felt none the less, that for so many years there had been no failure in the supplies contained within the great terrestrial storehouse. What must be done, then, is to show that by the progress of that very course of events which results in the rapid use of those stores, the means will spring into existence of obtaining fresh and inexhaustible supplies. This is no idly speculative view, but the plain and obvious duty of the scientific world. Precisely as the superiority of civilised races over barbarous tribes is shown in nothing more clearly than in the fact that the former are not content, as the latter are, merely to supply the wants of the moment, or of a few days, but seek to make provision, not only for future years, but for the wants of their immediate descendants, so it behoves the leaders of the great movement which during the last few years has so greatly changed the aspect of the human race, to show the superiority of the new order of things by a careful provision for, and anticipation of, the wants of the races which will inhabit the Earth thousands of years hence.

Without discussing the various forms of work which are being done upon the Earth, or considering the various agents employed in producing the motive power by which those forms of work are set in action, it may be simply stated that at present nearly all our motive force is obtained from *stored* Sun-force. It would be difficult to point to a single work accomplished by the aid of modern scientific appliances which has not resulted in exhausting to a greater or less degree the force which the Earth has been garnering up in long-past ages for our use. It is in this all-important respect that the more modern forms of machine-work differ from other forms of work. I refer, of course, to machines driven by inanimate motive powers, and not to those worked by the direct action of animal force. The machine draws upon the Earth's *garnered* stores,* while the living worker draws upon the Earth's *periodical supplies* of force. In the former case, that is being used up which cannot be replaced; in the latter, what is consumed will be restored in the ordinary course of nature. In one case it is our 'force-principal,' in the other it is our 'force-income' we are consuming. The distinction is all-important.

---

* Those appliances in which advantage is taken of the action of the wind, rainfall (rivers), tidal action, and a few other natural processes, are to be excepted. Modern invention, however, is but seldom directed to the utilisation of these old-fashioned force-supplies.

This is not the place to enter into a discussion of the methods by which the great problem—a problem not requiring immediate solution, but which in the long run will surpass all others in interest and importance—is to be solved. But I may indicate what is, I take it, the direction in which a solution will be found. We are now utilising what Professor Tyndall calls the Sun of the Carboniferous Epoch: our descendants will have to employ the Sun of their own epoch. The heat of the solar rays—mayhap also their light and their actinic energy—must one day be applied to work our machinery. Already men have felt the advantage of thus employing solar energy. They have not, indeed, as yet applied the direct action of the Sun systematically to their purposes.* But in an indirect manner they have utilised solar energy. The ships which sail upon our seas, the mills which are turned by water or by wind—these and many other devices of man have been contrived to utilise a portion of the Sun's heat. But the proportion thus utilised is almost indefinitely small by comparison with that which is actually available. It is only necessary to translate some of the ordinary phenomena of nature into the language of the familiar forces in order to see that this is so. For instance, the amount of energy involved in the production of rain is startlingly great

---

* Ericsson has constructed a machine in which the solar rays supply the primary motive force. It has not, however, yet been demonstrated (though I have not the least doubt it will be at some future epoch) that the solar heat can be employed in a profitable—that is, a 'mechanically advantageous,' manner.

when compared with our ordinary estimates of force. I have calculated that the force expended in the production of a day's steady rain over an area equal to that of the county of Middlesex would be equivalent to a mechanical power competent to raise 1,000,000,000 tons to a height of three miles!

Professor Tyndall has put in a striking form the relation which exists between the simpler processes of nature and those effects which seem to us the most apt exponents of power. 'I have seen,' he says, 'the wild stone-avalanches of the Alps, which smoke and thunder down the declivities with a vehemence almost sufficient to stun the observer. I have also seen snowflakes descending so softly as not to hurt the fragile spangles of which they were composed;—yet to produce from aqueous vapour a quantity which a child could carry of that tender material, demands an exertion of energy competent to gather up the shattered blocks of the largest stone-avalanches I have ever seen, and pitch them to twice the height from which they fell.'

And when we have thus seen what a tremendous amount of energy is involved in such processes as the formation of snow or rain in comparatively small quantities, we begin to recognise, though we are far from being able to conceive, how enormous is the potential energy which supplies the rainfall of the whole Earth. We must remember, too, that a large amount of rain falls where it is not wanted, and that the energy of the Sun expended in the production of wind is in large part wasted. Clouds are raised by evaporation from the sea surface to fall on another part of the self-same waters. Storms are roused which blow with vehemence for awhile, and then sink into rest without having accomplished any purpose necessary to the wants of terrestrial races. Here at once we see a large amount of energy not fully utilised. I do not indeed say that this apparently useless expenditure of force has no purpose in the economy of nature. Doubtless, every natural event has its end and object. What I would dwell upon is that if the energy which thus seems wasted could be made available to subserve human wants, it might be used without any fear that the economy of nature would suffer from such an application of her energies. And if this is true of the application of the indirect effects of solar energy, it is *à fortiori* true of the utilisation of the Sun's direct action—that is, of those solar rays to which the winds and the rains are due.

Now, if we assume that with the progress of science the power of thus employing to the full—or much more fully than at present—the forces which the Sun really expends upon the Earth will be acquired by man, we recognise the probability that science viewed generally is one of the means by which the efficiency of the solar energies is enlarged and extended. What is true of our Earth may be regarded as in all probability true of other worlds than ours. As on our Earth so probably in other worlds there are or have been eras during which the beneficent power given to our great luminary is used without any consciousness of its value. There are or have been more advanced eras when the return of day and night, the progress of the seasons, the nourishment of the lately sown seed by spring rains, and the whitening of the fields into harvest under the summer Sun, are watched with anxious interest. Then later follow the eras when the annual supply of truly vital energies seems to become insufficient, and when garnered stores of force are utilised by the thoughtful or ransacked by the too eager. And, lastly, it may well be that in other worlds, as one day doubtless on our Earth, there will be eras when a more advanced degree of science will enable intelligent beings to derive from direct solar action the means of obtaining even larger supplies of force than they had been able to gather from the Sun-work of past epochs garnered by nature for their benefit.

The ideas of the Sun's true position in the solar system thus suggested lend an enhanced interest to the question whence the Sun himself derives and recruits his energies. As Tyndall has finely written, 'How is the perennial loss made good? We are apt to overlook the wonderful in the common. Possibly to many of us—and even to some of the most enlightened among us—the Sun appears as a fire, differing from our terrestrial fires only in the magnitude and intensity of its combustion. But what is the burning matter which can thus maintain itself? All that we know of cosmical phenomena declares our brotherhood with the Sun—affirms that the same constituents enter into the composition of his mass as those already known to chemistry. But no earthly substance with which we are acquainted—no substance which the fall of meteors has landed on the Earth—would be at all competent to maintain the Sun's combustion. The chemical energy of such substances would be too weak, and their dissipation too speedy. Were the Sun a block of burning coal, and were it supplied with oxygen sufficient for the observed emission, it would be utterly consumed in 5,000 years. On the other hand, to imagine it a body originally endowed with a store of heat—a hot globe now cooling—necessitates the ascription to it of qualities wholly different from those possessed by terrestrial matter. If we knew the specific heat of the Sun* we could calculate its rate of cooling. Assuming the specific heat to be the same as that of water--the terrestrial substance which possesses the highest specific heat—then, at its present rate of emission, the entire mass of the Sun would cool down 15,000 degrees in 5,000 years. In short, if the Sun be formed of matter like our own, some means must exist of restoring to it its wasted power.'

We have not as yet the means of satisfactorily answering the question thus suggested. Answers have

---

* The absolute quantity of heat necessary to raise the average temperature of the Sun by any given amount—say one degree, and therefore the absolute quantity of heat which corresponds to the loss of say one degree of temperature from the average temperature of the Sun—will depend on the physical constitution of the Sun. To say merely that the Sun's substance subsists at such and such a temperature, is by no means sufficient to indicate the amount of heat which the Sun is capable of imparting. A mass of iron, for example, may be at a temperature precisely equalling that of an equal mass of boiling water; yet the boiling water will give out far more heat while passing to any given lower temperature than the heated iron will.

been suggested, but no answer has yet seemed so satisfactory that men could regard the problem as disposed of. Indeed 'the facts are so extraordinary,' as Tyndall has said, 'that the soberest hypothesis must appear wild.' Whether we conceive, with Mayer and Thomson, that the Sun's heat is maintained by the incessant downfall of cosmical bodies gathered out of space by the Sun's mighty attractive energies; or whether we follow Helmholtz in supposing that the gradual contraction of the solar orb is the mainspring of the solar energies; or whether we believe, with Secchi, that the dissociation of compound bodies in the Sun's substance is a fund of force to be gradually exhausted only as the dissociated elements unite in chemical combinations; or, lastly, whether we prefer the idea thrown out by Sir John Herschel that mayhap the vital energies of monstrous creatures—the willow-leaves of Nasmyth—are the true source of the great luminary's might, we have not overpassed by a step the amazing field of conjecture appertaining to our subject. Startling as these theories appear, they are not a whit more startling than the known facts which they are intended to interpret.

I have no wish to enter here into a detailed consideration of any of the theories above referred to; but I wish to make a few remarks respecting those of Helmholtz and Thomson.

When we consider the evidence forced upon us by the present condition of the solar system, and by the nature of the observed motions taking place within it, we find it difficult not to believe that two great processes have for uncounted ages been at work within its limits. On the one hand, the evidence is very strong in favour of the view that a process of contraction from a nebulous condition has taken place not only in the case of the Sun, but in that of the planets and other members of the solar system. On the other hand, the evidence is absolutely demonstrative that at the present time uncounted millions of minute cosmical bodies are streaming in upon the Sun. We have, then, clear evidence that at least some portion of the Sun's energy is derived in each of the two methods now dealt with. We can hardly conceive that the process of solar contraction has come to an end; and certainly we have no proof in the apparent constancy of the Sun's volume that the process has ceased, since Helmholtz has shown 'that the shrinking of the Sun's diameter by one-10,000th part of its present length would generate an amount of heat competent to cover the solar emission for 2,000 years.' Nor can we question that whatever energy may correspond to the velocity, mass, and distance of a meteoric body at any epoch must have been transferred to the Sun if at some later epoch the mass of the meteor has come (after whatever processes) to form part of the solar globe. Now, without committing myself to the opinion that the whole solar emission can be accounted for by combining these two causes, I must yet express the conviction that to forget the reality of these causes, their competence to account for some aliquot portion (let its amount be what it may) of the solar energies, would be a mistake. Nor can I see any valid reasons for asserting positively that the two causes combined may not account for a very large proportion of the Sun's activity. The irregular and perhaps intermittent supply of meteoric matter affords doubtless but an insufficient explanation of the Sun's copious and steady emission of heat; but the process of contraction would act the combined part of a 'governor' and an independent source of heat. Checked during the arrival of large meteoric supplies, and proceeding more rapidly when those supplies were temporarily diminished, it would account for that observed steadiness of emission which forms so important a characteristic of solar action.

I feel that I cannot bring this chapter more aptly to a conclusion than by quoting that noble passage in which Tyndall closes his discussion of the same subject:—'Presented rightly to the mind,' he says, 'the discoveries and generalisations of modern science constitute a poem more sublime than has ever yet been addressed to the imagination. The natural philosopher of to-day may dwell amid conceptions which beggar those of Milton. So great and grand are they, that in the contemplation of them a certain force of character is requisite to preserve us from bewilderment. Look at the integrated energies of our world,—the stored power of our coal-fields, our winds, and rivers; our fleets, armies, and guns. What are they? They are all generated by a portion of the Sun's energy which does not amount to the twomillionth of the whole. This is the entire fraction of the Sun's force intercepted by the Earth, and we convert but a small fraction of this fraction into mechanical energy. Multiplying all our powers by millions of millions, we do not reach the Sun's expenditure. And still, notwithstanding this enormous drain in the lapse of human history, we are unable to detect a diminution of his store. Measured by our largest terrestrial standards, such a reservoir of power is infinite; but it is our privilege to rise above these standards, and to regard the Sun himself as a speck in infinite extension—a mere drop in the universal sea. We analyse the space in which he is immersed and which is the vehicle of his power. We pass to other systems and other suns, each pouring forth energy like our own, but still without infringement of the law which reveals immutability in the midst of change, which recognises incessant transference or conversion, but neither final gain nor loss. The law generalises the aphorism of Solomon, that 'there is nothing new under the Sun,' by teaching us to detect everywhere, under its infinite variety of appearances, the same primeval force. To Nature nothing can be added; from Nature nothing can be taken away; the sum of her energies is constant, and the utmost man can do in the pursuit of physical truth, or in the applications of physical knowledge, is to shift the constituents of the never-varying total. The law of conservation rigidly excludes both creation and annihilation. Waves may change to ripples and ripples to waves—magnitude may be substituted for number and number for magnitude—asteroids may aggregate to suns, suns may resolve themselves into floræ and faunæ, and floræ and faunæ melt in air—the flux of power is eternally the same. It rolls in music through the ages, and all terrestrial energy—the manifestations of life, as well as the display of phenomena—are but modulations of its rhythm.'

# CHAPTER IX.

## THE SUN AMONG HIS PEERS.

We have hitherto regarded the Sun with reference to his position in the solar system—as ruler, fire, light, and life of that wonderful scheme whose real magnificence and complexity has but recently begun to be recognised by astronomers. We have now—but very briefly, for already the space allotted to our subject has been exceeded—to consider him as a member of the sidereal system. What he is to the scheme of dependent worlds we have seen; it remains that we should endeavour to form some conception of his position among his peers. We have to contemplate him as a Sun among many suns, exerting an influence indeed over his fellow orbs, but, swayed in like sort by their attractions, still surrounded—as when we considered him with reference to the solar system—by orbs travelling with enormous velocity, but no longer at rest, or almost at rest, amidst a scheme of moving worlds. We are to see him taking part in a scheme of movement too wondrously complicated to be as yet interpreted by astronomers.

We must not pause here to consider the processes—interesting though their history may be—by which astronomers have been enabled to determine the distances of certain stars, and so to form a general estimate of the scale on which the sidereal system is constructed. Let it suffice to mention that the fundamental fact on which our estimate of the distances of the fixed stars from us and from each other has been based, is the circumstance that while our Earth sweeps round the Sun on an orbit more than 180,000,000 miles in diameter, the stars remain all but unchanged in their apparent position, all the powers of our modern instruments only revealing in a very few instances the minutest conceivable displacement. Setting this fact clearly before us, the grandeur of the sidereal system becomes more real and present to our minds. From the nearest fixed star, the vast orbit of our Earth is reduced to little more than a point,—to a circle so minute that 2,000 such circles could be placed side by side along the apparent diameter of the Sun or Moon. But the great majority of the stars lie at distances far vaster: so vast indeed, that the Earth's orbit is reduced to a mere point as viewed from beyond the vast abysms which separate us from those orbs. Nor is it likely that in general, the distance of star from star, of any star in the heavens, for instance, from the nearest of its neighbours, falls short of the distance by which our Sun is separated from the nearest of his fellow orbs.

We are thus brought face to face with a problem full of interest but enormously difficult—a problem which belongs perhaps rather to the astronomy of the future than to that of our day. How are men to determine the figure and dimensions of the sidereal system, to understand its structure and complexities, to trace out the motions taking place within its limits, when as yet they seem to have scarce any means of even attempting to solve these problems? Yet here, unless I mistake, is a work from which future astronomers will not shrink, a problem whose solution (for it *will* be solved) cannot but reveal results altogether surpassing in interest any which astronomers have yet obtained. It is true that if we consider the means we have for attacking this noble problem, they seem ineffective indeed; if we look at the results of past research we find little to encourage present confidence. Yet it is only necessary to consider the amazing interest of the problem to set doubt and irresolution on one side, and at least patiently to test the means we have at our disposal.

I have elsewhere* pointed out reasons for regarding the views hitherto accepted respecting the sidereal system as unsatisfactory. The results obtained by Sir William Herschel, and apparently confirmed by the labours of Sir John Herschel, the elder Struve, and others, seem, according to the evidence I have adduced, to be self-contradictory and not accordant with other equally reliable researches. I confess I can no longer entertain any doubt that there is an error in the

---

* In *Other Worlds than Ours*, and more especially the second edition, where additional, and, I think, conclusive arguments are brought forward.

hypothesis which underlies Sir William Herschel's reasoning. It is absolutely essential, if we would form any adequate conceptions at all respecting the nature of the scheme to which the Sun belongs, that that hypothesis and its results should be re-considered. I must avoid here, however, all reference to arguments already enforced, and indeed I propose but to sum up here the results I have exhibited elsewhere, and then to pass on to consider one special circumstance connected with the Sun's relation to his fellow suns—the *proper* motion by which he speeds through interstellar space.

Sir William Herschel, fired with the noble thought of gauging the celestial depths, took as the fundamental hypothesis on which his gaugings were to rest, the conception that the stars are spread with a certain general uniformity within a definite region of space.* If this one hypothesis be admitted, it becomes possible, by means of a telescope powerful enough to reach the most distant and the smallest stars of the system, to gauge the extent of the system. All that is necessary is to count the number of stars seen in the telescopic field of view when the instrument is directed towards different parts of the heavens. Where many stars are seen, there the system must necessarily have its greatest

---

* It is sometimes added that Sir William Herschel supposed a certain general uniformity of size and brilliancy to exist among the stars. This, however, is a mistake. The only general hypothesis made by Sir William Herschel was the one stated above; he added, as necessary to the admission of his own star-gaugings, the theory that his telescope reached—at least in most directions—the limits of the sidereal system.

extension; where there are few stars, there the limits of the system must be nearest to us.

It is well known that by this process of star-gauging Sir William Herschel was led to the conclusion that the sidereal system has the form of a cloven disc. The extension of this disc is towards the region of the heavens occupied by the Milky Way, the parts of the heavens where no milky light is seen corresponding to the flattened sides of the disc. According to the essential principle of this method of star-gauging, applied to the observed numerical relations, it follows inevitably that the stars visible to the naked eye lie far within the limits of the cloven disc. The same conclusion follows, also, from Sir John Herschel's gauges of the southern heavens; though his view of the sidereal system differed in *this* respect from his father's, that he considered the stars visible to the naked eye, and others down to about the tenth magnitude, to be less richly spread through space than those whose united lustre produces the milky light of the galaxy. But whether the richer parts of the sidereal system form, as Sir William Herschel thought, a cloven disc in space, or, as Sir J. Herschel supposes, a cloven ring, surrounding the lucid stars,—in either case, accepting only Sir William Herschel's fundamental hypothesis, we are bound to admit that the lucid stars lie far within the limits of the sidereal system.* In

---

* For convenience, astronomers speak of the stars visible to the naked eye as the *lucid stars*. The title has no reference, it will be understood, to the intrinsic brilliancy of the light of the visible stars.

whatever direction we turn our eyes to look upon the lucid stars, we may be quite certain, if only this hypothesis be true, that the bounds of the star-system lie far beyond the constellations we are regarding.

Now it is at this point that my study of the stars has led to the recognition of evidence which opposes itself in the most striking manner to the views usually accepted. I find among the lucid stars the most convincing signs of aggregation along certain definite regions, and of segregation from others. I have applied to these signs the strictest principles of mathematical calculation, in order to determine whether they can by any possibility be due to chance distribution, and I find that it is wholly impossible so to interpret them. But in this result, regarded by itself, there is in truth nothing opposed to the accepted theories. It is indeed an interesting circumstance that such traces of aggregation and segregation should be recognisable, and perhaps it may seem to many a perplexing circumstance that these signs should so long have escaped recognition.* But, apart from the interest thus attaching as I think to the discovery, there is nothing which may not be conceived to accord very well with the views of the Herschels. For such peculiarities of structure, if one may so speak, within the sphere of

---

* This may be ascribed wholly to the strange nature of the star-atlases hitherto constructed, in which the authors seem to have studied how they might best (by distorting the celestial spaces and by covering them over with monstrous figures of men and animals) conceal altogether from view any laws of association which may really exist among the stars.

the lucid stars might extend thoughout the whole of the 'cloven-disc star-system' conceived by Sir William Herschel, and yet the averages on which he based his conclusions might not be disturbed. It is when another and most unexpected relation is mentioned that the accepted theories are found to fail. The aggregation of stars distinctly recognised in some regions and very marked in others is most marked of all along the Milky Way. Not only are lucid stars so richly strewn on the Milky Way that for the whole heavens to be as richly spread 6,000 new lucid stars would be wanted, but the gaps and lacunæ in the Milky Way are so bare and vacant that were the whole heavens no richer 4,600 stars now visible would have to be blotted from our view. Such, briefly stated, is the statistical evidence on this point. There can be no question that it is of the most convincing character. The probabilities against such a result if chance distribution were alone in question—that is, if no real relation existed between the lucid stars seen amid the milky light of the galaxy and the clustering groups of telescopic stars which produce that light—may be readily shown to be so overwhelming that no illustration can be devised to convey an adequate idea of their immensity. The chance that the Sun will rise to-morrow is ridiculously small (at least as Quêtelet calculates it) by comparison. So that as long as the laws of probability are to be our guide in such matters (and in every scientific conclusion ever yet adopted we have had no other evidence) it must be regarded as certain that the lucid stars seen on the Milky Way are for the most part immersed among the crowds of minute stars forming the diffused light of the galaxy. These galactic stars then are much nearer than had been supposed, and they are *really* minute, not reduced merely to apparent minuteness by the vastness of their distance.

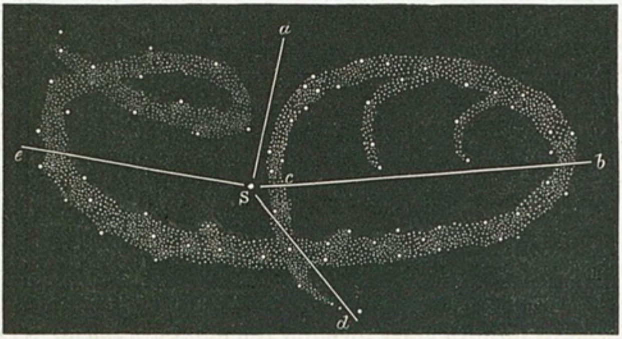
When we add to the considerations thus suggested that the nebulæ have been shown by unmistakable signs of association to form a system intimately connected with the system of stars, we begin to see that the sidereal system regarded as a whole is very different from that scheme of suns pictured in the accepted theories. Our Sun and his fellow suns are associated with groups of minor suns, with clusters of star-dust, with masses of star-mist. We trace amid the complex system thus disclosed the signs of as yet unthought-of laws. Here the large suns gather into well-marked clusters; here they form streams amid the celestial depths. In one region we find them associated with that strange spiral of minute stars forming the galaxy* (see fig. 87); in another they are grouped with discrete nebulæ; and yet elsewhere they are immersed amid the whorls and convolutions of nebulous matter. Lastly, in two regions we see suns and minor stars, star-clusters and discrete nebulæ, and masses of nebulous matter, combined into vast spherical aggregations—the Magellanic Clouds of the seaman—and these aggregations them-

---

* Such is at least a figure which (as shown in my *Other Worlds*) accounts in a satisfactory manner for all the observed peculiarities of the Milky Way.

selves forming the centre of a remarkable group of lucid stars. This group, numbering more than 2,500 orbs, covers one half of the southern heavens. It sweeps in a mighty spiral* around the greater Magellanic Cloud. It gathers its host of lucid orbs so densely along one part of its course that that region of the heavens alone suffices to light up the southern skies as with the light of a young moon. It presents, in fine, phenomena which leave little room for question that it forms a great and distinct system, within whose bounds are included all the characteristic features of the sidereal system itself, if indeed we are not to regard it as forming the noblest half of that portion of the universe of which we have hitherto become cognisant.

Fig. 87.



The Milky way regarded as a Spiral.

---

* The features here referred to are very strikingly exhibited in the isographic maps of the northern and southern heavens accompanying the second edition of my *Other Worlds*. These maps show all the stars visible to the naked eye (in white on a black ground) truly distributed area for area.

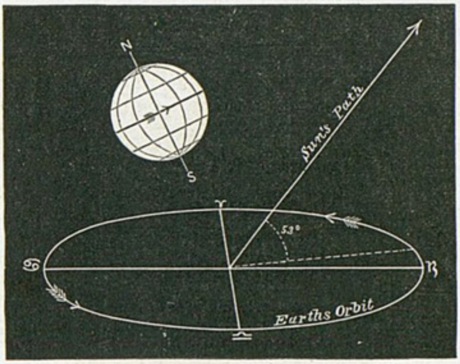
It is not without a purpose that I have thus directed the reader's attention to the vast southern star-system which constitutes the most striking and instructive feature of the heavens. If there is no feature of the northern heavens which to ordinary vision seems to correspond to this southern star-system, yet statistical research reveals the fact that the southern region has its true analogue in our northern heavens. The widely extended group of stars surrounding the projection of the Milky Way in Cepheus, and including within its limits the singularly rich portion of the Milky Way in Cygnus, has not only well-defined limits, but presents a well-marked superiority to the rest of the northern heavens as regards richness of star-distribution. Though smaller in extent, it is not less rich on the average than the great southern rich region. It corresponds also with that region in some other and rather peculiar respects. It covers a region where the Milky Way throws out projections, and shows vast vacuities. The Milky Way reaches it on one side as a single stream, on the other as a double stream; and, further, the brightest portions of the Milky Way, in the northern and southern heavens, lie near these two rich regions, and both also towards that edge of the rich region whence the double stream of milky light extends.

Now, I would invite attention to the circumstance that the Sun's proper motion, according to the best estimates hitherto made, is carrying him from the borders of the southern rich region towards the borders of the northern rich region. He is passing away from the neighbourhood of Canis Major, Columba, and Lepus (not to define too precisely the as yet scarcely determined path along which he travels), and he is urging his way with inconceivable velocity towards the region between Hercules and Lyra. Of the true habitudes of those regions of space through which he is bearing, and has lately borne, his family of planets we know little. But as we look back along the extended track he has pursued, and see the richness of those regions he has left, and as we look onwards and trace his course in imagination towards the borders of that rich region whose glories gather into their chiefest splendour in Cygnus, the conception is suggested that he is now winging his way through a relatively barren region, that he has left and will again visit more glorious star-depths than those through which he now pursues his course.

And here we may pause for a moment to consider the nature of that path along which we ourselves are borne as the Earth sweeps on her course round the Sun.

Let the foreshortened circle in fig. 88 represent the path of the Earth about the Sun, the globe N S representing on a large scale the slope of the Earth's axis throughout her annual revolution. When the Earth is at ♋ it is mid-winter; when she is at ♎ it is spring; at ♑ it is summer; and lastly, when she is at ♈ it is autumn. Then the path of the Sun has the position indicated in the figure, being inclined some 53 degrees to the plane of the Earth's orbit, and some 15 degrees in advance of ♑.[*] Along this path the Sun pursues his course at a rate which has been estimated at about 150 millions of miles, or five-sixths of the diameter of the Earth's orbit in each year. Hence, since the Earth's orbit-plane is carried along at this rate, while the Earth circles around that orbit once in each year, it follows that the actual path pursued by the Earth in space[†] is such as is indicated in fig. 89. It is in fact a skew spiral or helicoidal path.[‡]

FIG. 88.



Illustrating the motion of the Earth's orbit through space.

---

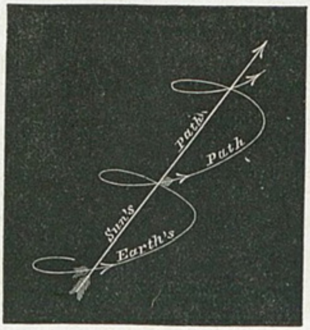
[*] Its projection on the ecliptic, that is, lies in longitude 285°, or thereabouts.

[†] Or rather within the sidereal system, which itself doubtless has some motion—perhaps an inconceivably rapid motion.

[‡] Some persons have expressed great anxiety lest, if the Sun is really travelling so swiftly through space, he should leave the Earth behind. There is not the least fear of this, any more than there is that the Earth should leave the Moon behind. Whatever forces have caused the Sun to follow his present career have acted upon the Earth and all the planets with equal effect. And as the Sun's course through

The other members of the planetary system pursue paths of different figures. The coils of the helicoids traversed by Mercury and Venus lie relatively close together; those of Mars are not so near as the coils of the Earth's path. The asteroidal helicoid paths—amazingly complicated they must be—are yet more drawn out (so to speak). But it is when we pass beyond the asteroidal orbits that we get the most extended helicoids. Jupiter is carried some 1,700,000,000 miles onward with the advancing Sun, while he circuits once around his orbit of less than 1,000,000,000 miles in diameter; Saturn sweeps on through some 4,400,000,000 miles, while circuiting his orbit, less than 1,850,000,000 miles in diameter; and the paths

Fig, 89.



The Earth's motion through space.

space becomes modified by the varying attractions to which he may be subjected, every planet in the solar system, every satellite, meteor, planetary comet, and asteroid, will experience the same influences, and accompany the Sun just as faithfully as at present.

traversed by Uranus and Neptune amid the depths of sidereal space are even more remarkably drawn out, regarding them in their helicoidal character.

As the Sun travels through space the planets sweep onward with him.* But has he, besides his planetary dependants, any companions on his voyage? Do any of his brother suns travel along with him? As yet we have no means of knowing, for a strange difficulty arises. If the Sun has companions, these must, of course, be relatively near to him. It does not follow that they will appear brighter than other stars, because if they are no larger than the Sun, we know that other orbs (as Sirius and Arcturus) must largely exceed them in real size, and so their relative proximity may not be rendered apparent. But this is not all. The stars which astronomers select as most likely to afford measurable indications of proximity are those whose apparent motions on the heavens are exceptionally large. Now the companions of our Sun on his voyage through the sidereal system doubtless travel on a nearly parallel course; and therefore, setting aside their orbital motions around each other, or around the common centre of gravity of the family, they must appear, as viewed by us, to be almost at rest. They would of course indicate in a more marked manner than any other stars the effect of the Earth's annual

---

* It is worthy of notice that the Sun's northern hemisphere travels forwards. Is it not conceivable that in this peculiarity we may find some explanation of the greater heat which has been said to be emitted from the northern solar hemisphere? See also p. 210, footnote.

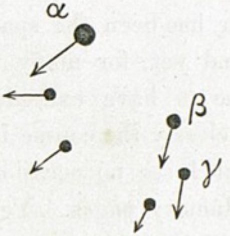
motion (or, technically, they would have a large annual parallactic displacement); but then astronomers would not be led to look for such effects, since we know as a matter of fact that the stars hitherto examined for signs of annual parallax are those which, either through exceptional brilliancy or through exceptionally large proper motion, seem likely to be near to us.

FIG. 90.

Observed proper motions in Ursa Major and neighbourhood.

We find signs in the heavens leading us to regard the existence of such 'companions of the Sun' as at least not wholly improbable. Here, for example, is a picture (fig. 90) borrowed from my 'Other Worlds,' in which the chief stars of the constellation Ursa Major are depicted, and some few others belonging to Draco and Bootes. To each star is attached a small arrow indicating the direction of its motion, and the amount of such motion in 36,000 years. We see here decided signs of star-drift. We can scarcely doubt that the five principal stars of Ursa Major included within the dotted line are travelling together though space; while the four stars above, belonging to Draco,

Fig. 91.



Observed proper motion of Stars in head of Aries.

seem similarly to be companion suns. The remaining stars may also not improbably form a single family.

The group of stars shown in fig. 91 seem to form a system within which probably there are orbital motions of considerable magnitude.

It is worthy of notice that in the two instances here referred to there are evidences of association apart from the observed proper motions. For in the second the stars forming the system seem segregated in a somewhat marked manner from neighbouring star-groups. The stars in Ursa Major, again, have been noted by Fr. Secchi as having very similar spectra—in other words, as resembling each other very closely in structure and condition. In this circumstance we have a peculiarity which may one day enable us to select with some confidence those stars which are our Sun's special companions, which voyage along with him through the sidereal system, and share with him perchance in a reign over some special domain of space.

And here I have to draw this work to a conclusion. Too limited by far has been the space I have allotted to my subject, and yet, for many reasons, it would have been unwise to have exceeded this space. I have followed so closely the course I marked down in the beginning that I see no occasion to change one word in the introductory pages. Yet at many stages where I had promised myself a pause for survey and reflection I have been compelled to pass on without stay. Many inviting paths opening out on one hand and the other have been perforce left unexplored. As I have proceeded I have become more and more sensible of the vastness of the subject I have undertaken to discuss. Not a book, but a library of books, is needed to deal adequately even with only what is at present known about the Sun; not a few students of science, but all the astronomers and physicists now living, might devote their powers to the study of solar phenomena, and yet find that the army of labourers needed to be largely recruited. Only in the coming time, when the students of science will be the majority instead of a minute minority of mankind, will this subject, and those countless other kindred subjects which await investigation, be adequately dealt with. Let us hope that time is not far off.

# APPENDIX A.

## *THE APPROACHING TRANSITS OF VENUS AND THE BEST MEANS FOR OBSERVING THEM.*

On account of the interest attaching to the approaching transits, and more especially to the transit of 1874, for which a sum of more than 10,000*l.* has been voted by the Government, it seems desirable to exhibit here in a popular form the results to which I have been led by a very careful mathematical investigation of the conditions under which the coming transits may be most satisfactorily observed.*

Already in Chapter I. I have exhibited the general principles on which the determination of the Sun's distance by observation of transits of Venus depend. But as it is always advantageous in discussing astronomical relations to view them in as many aspects as possible, I now present a mode of

---

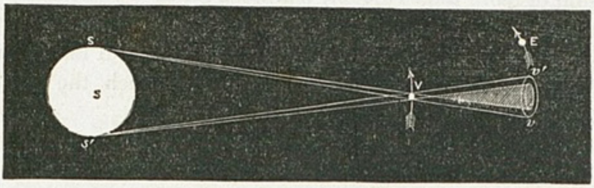
* I may remark here that I have gone over, at the expense of a considerable amount of time (for the work was altogether new to me), the process of calculation by which the elements of the transit are deduced from the tables of Venus and the Sun; but (as I anticipated when I began) the results I obtained accord so closely with Mr. Hind's, that the labour (save for the practice it gave me) was in a sense thrown away. It is worthy of notice, however, that M. Puiseux having obtained somewhat different results, this confirmation of Mr. Hind's results has a value which ordinarily would be wanting to researches of the sort. Mr. Plummer, of Mr. Bishop's Observatory, Twickenham, had already, at Mr. Hind's request, tested the published elements; and there can be now no doubt whatever that in this matter (as always) Mr. Hind's calculations are beyond question.

considering transits of Venus which is perhaps the best and simplest conceivable.

Let S (fig. 92) be the Sun, V (Venus) and E (the Earth) travelling in the same direction and nearly in the same plane round the Sun as shown by the arrows, Venus the more swiftly; and let it be noted that the dimensions of the Sun, Venus, and the Earth are here necessarily exaggerated, since even the Sun, if presented on the same scale as the distances, would be scarcely perceptible in our figure.

Now, Venus, like every other body in the solar system, throws a shadow, and the shadow is represented by the triangular space behind Venus in fig. 92. It comes to a point not very far (relatively) from Venus. Suppose this cone produced beyond its apex so as to form the shaded cone shown in the figure. It is obvious that to an eye placed within this shaded region Venus will be seen *fully within the Sun's disc;* and to an eye placed on the surface of this region Venus will be seen just touching the Sun's disc *on the inside.* Now conceive a double cone touching both Venus and the Sun, but having its apex *between* these bodies, as shown in the figure. Then obviously to any eye on the surface of the region between this apex and *v v′*, Venus would be seen just touching the Sun's disc *on the outside.** Clearly, then, to any eye between

FIG. 92.



---

* For instance, to an eye at *v* Venus would seem just to touch the Sun's disc on the outside at *s*, while to an eye placed at *v′*, Venus would seem just to touch the Sun's disc on the outside at *s′*. This is obvious, because the lines *v* V, *v′* V, just touch the Sun and Venus, whereas any other lines from *v* and *v′* to points on Venus pass clear of the Sun.

this surface and the region shaded in the figure, a part only of the disc of Venus will be seen on the disc of the Sun.

So that if an observer were carried through the double cone shown in fig. 92 behind Venus, he would see the following successive phenomena. When he came to the outer surface Venus would be in exterior contact, or as at A (fig. 14, p. 42); as he passed on to the inner surface Venus would enter more and more on the Sun's disc, until when he reached that surface she would be in interior contact, or as at B (fig. 14). Then as he travelled on through the inner cone Venus would seem to cross the Sun's disc, and she would just touch it on the inside when our observer reached the surface of this inner region on his passage outwards. Next, as he passed onwards to the surface of the outer region, Venus would be seen crossing the edge of the Sun's disc. And, lastly, as he passed that surface he would again see Venus in exterior contact, the transit thereupon coming to an end.

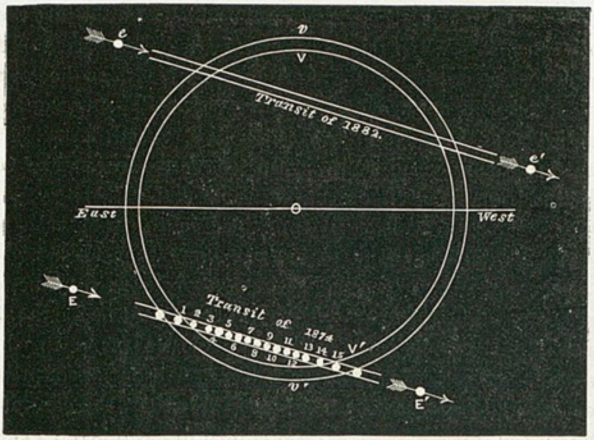
Now, during a transit of Venus the Earth does actually pass in such a way through these regions; or rather these regions overtake and pass over the Earth. In ordinary conjunctions the cones of fig. 92 pass above or beneath the Earth; but when the Earth is sufficiently near to the plane in which Venus is moving, the cones do not pass without encountering the Earth, and so a transit takes place. Further, the considerations in the preceding paragraph suffice to exhibit the general circumstances of a transit.

Since the cones overtake the Earth in the direction shown by the arrows, we may consider that the Earth passes through the cones in the contrary direction.

Suppose $v\ v'$ (fig. 93) to represent the same section of the outer cone as $v\ v'$ in fig. 92; V V′ the section of the inner cone; and E (fig. 93) the Earth, as shown at E in fig. 92. Then $v\ v'$ is really moving towards the left; but we are to suppose that E is moving towards the right through $v\ v'$. Furthermore, if Venus is near an ascending node, as she will be during both the approaching transits, we must suppose the Earth to pass descendingly along such a course as E E′ through the region *v v′*. The actual course, both as respects position and direction, is determined from the calculated elements of the transit. With this calculation we need not here concern ourselves.* The figure shows the course actually traversed by the Earth in 1874 and 1882.

Now, taking the Earth through *v v′* for the 1874 transit, let us consider the various critical points, so to speak, of her course. When she first touches the outer circle *v v′* external contact will have begun at that point of the Earth which first reaches this circle. She passes on, falling more and more within *v v′*, until she is just wholly within. All this time external contact is taking place wherever the outline *v v′* intersects the Earth's disc; at parts within that line Venus is seen partly

Fig. 93.



Illustrating the transits of Venus.

---

* As to the size of *v v′* compared with that of the Earth, it is easily seen from fig. 92 that *v v′* is less than a great circle of the Sun, very nearly in the proportion that the Earth's distance from Venus exceeds the Sun's. If the cone of which *v v′* is a section had its vertex at Venus's centre this proportion would be exact.

within the Sun's disc, and at parts outside of it external contact has not yet taken place. When the Earth has passed wholly within the circle *v v′*, external contact has taken place at all parts of the visible hemisphere. But as at this time no part of the Earth has reached the circle v v′,[*] internal contact has nowhere commenced. In other words, Venus is not yet fully upon the Sun's disc as seen from any part of the Earth.

Now, this part of the Earth's motion is not illustrated in fig. 93, because external contacts and the passage of Venus across the Sun's outline are not phases which the observers of transits pay great attention to. We now come to the important phases.

When the Earth just reaches the inner circle v v′, interior contact has just begun at the point on the Earth which first touches this circle. Here, then, earliest of all, internal contact begins, and we have at this point the phenomenon called by astronomers *first internal contact most accelerated.* The Earth is then in the position numbered 1 in fig. 93.

She passes on, the outline v v′ encroaching more and more over her face until she is wholly within this outline or in position 2. All this time internal contact is taking place wherever the outline v v′ intersects the Earth's disc. At parts of the Earth within that line internal contact has passed, or Venus is already fully upon the Sun's disc. At parts of the Earth outside that line Venus still breaks the outline of the Sun's disc. When the Earth is at 2, internal contact has taken place for all places on the Earth's illuminated hemisphere. This contact takes place latest of all at that point on the Earth's surface which at this moment touches v v′. It is here, then, that there occurs the phase which astronomers call *first internal contact most retarded.*

---

[*] The distance between the circles *v v′* and v v′ is obviously greater than the Earth's diameter, if we consider how the two circles *v v′* and v v′ are obtained. For the diameter of Venus is very nearly equal to the Earth's; so that the diverging lines from *s* or *s′* (fig. 85) are already separated at v by a distance nearly equal to the Earth's diameter, and herefore at *v* or *v′* are wider apart.

Then the Earth passes onwards through the positions shown severally along her track in fig. 93.

As the Earth passes out of the spaces V V′, *v v′*, similar phases occur in reverse order. We need note only the positions numbered severally 14 and 15. The first shows where the Earth first reaches V V′, and the point on her surface which first touches V V′ is the place where occurs the phase called *second internal contact most accelerated*; while 15 shows where the Earth is just passing clear of V V′, and the point on her surface which is the last to touch V V′ is the place where the phase occurs called *second internal contact most retarded*. The circumstances of the progress of the Earth from one position to the other precisely correspond to those already considered in dealing with the Earth's motion from 1 to 2, only they take place in reverse order.

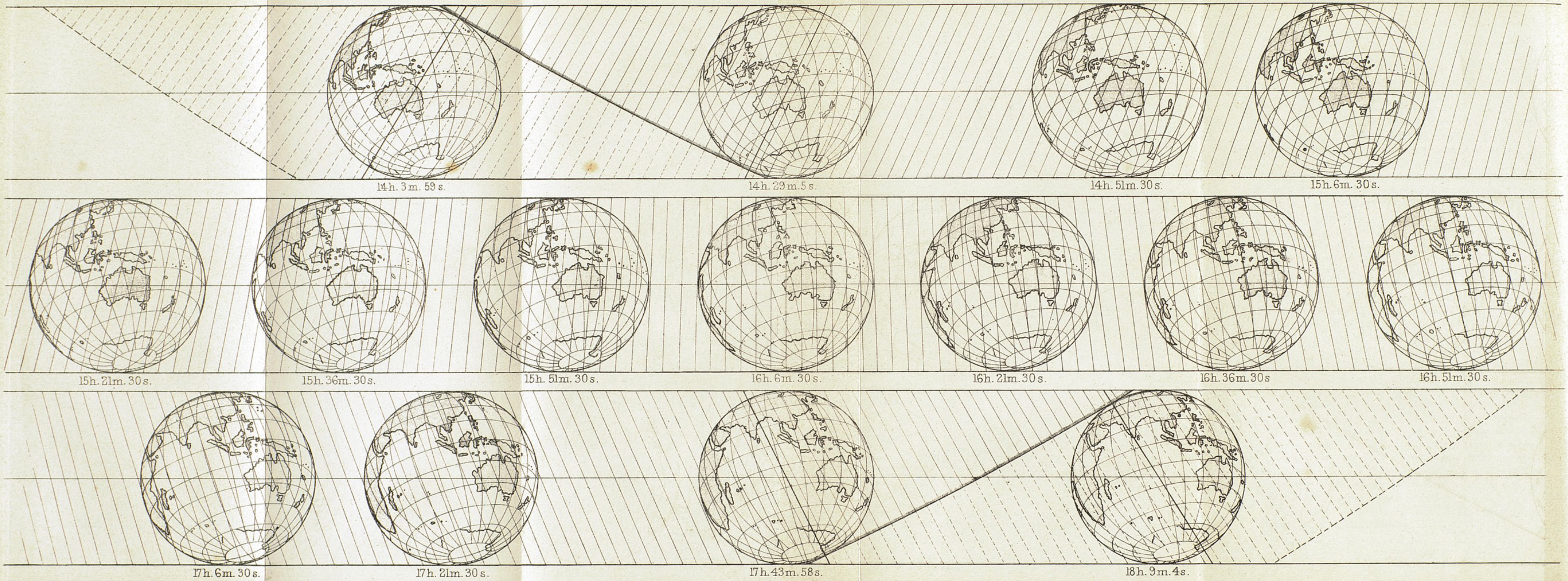
Such a passage as I have described lasts altogether some four or five hours (a passage through the centre C about eight hours), and in this interval the Earth's axial rotation is not inconsiderable. This rotation has to be taken into account in dealing with the real circumstance of any transit.

The reader will see, then, that to present in an exact manner all the relations thus involved would require not merely a large amount of space, but the discussion of mathematical considerations of some complexity. And yet there is absolutely no simpler way of exhibiting these relations in a descriptive manner.

This is, then, one of those cases where, if we are to give views at once exact and generally intelligible, we must employ that too much neglected aid of the astronomical teacher, truthful pictorial illustration. We must have representations of the Earth as she is actually placed at the most important parts of her course through *v v′*, truly poised, with true axial slope, and in the true position as respects axial rotation. Such pictures will do what verbal description in this case can never do, save for the mathematical reader.

The illustrative plates VIII., IX., and X. have been specially onstructed to meet this difficulty.

PASSAGE OF THE EARTH THROUGH VENUS'S SHADOW-CONE, DEC: 8, 1874.



The long folding plate pictures the Earth on her course through the positions marked 1, 2, . . . 14, 15, in fig. 93. The path of the Earth in this figure is, for convenience of engraving, broken up into three parts, as shown in fig. 94, and the Earth is represented at each part of her progress, precisely poised and rotated, as she would appear if she could be viewed from the Sun during the course of the transit of 1874.

Leaving the cross-lines out of consideration for the present, let the student study this plate, interpreting it by reference to figs. 92 and 93, and he will be able to form more exact conceptions of the real relations presented during the transit than he could from a very long and recondite explanation. He sees in the first picture of the Earth those regions whence the beginning of the transit will be visible. He sees in the last those regions where the end will be visible. Those parts of the Earth which appear in both these views are those from which the whole of the transit will be visible. And, finally, those parts which do not appear in either of these views (nor, therefore, in any of the fifteen) are those whence no part of the transit will be seen.

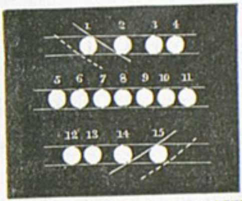


Fig. 94, explaining Plate VIII.

But now, having done this, and understanding also that he will presently be invited to return to the consideration of this plate, let him examine the larger and more elaborate views Plate IX. and X., representing the Earth as supposed to be seen from the Sun at the beginning and end of the transit.

Of these views the first represents the Earth as she would appear from the Sun when *her centre* is just crossing the circle V V′ of fig. 93 at ingress, and the second represents her as she would appear when her centre is just crossing the same circle at egress. So that the first corresponds to an epoch between those represented in the *first* two Earth-pictures of the folding plate, while the second corresponds to an epoch between those represented in the *last* two pictures of that plate. The seemingly parallel cross-lines in Plate IX. represent the encroaching outline of the circle v v′ (fig. 93) at intervals of a single minute of time between the epochs represented by the first two figures in the folding plate. The corresponding cross-lines in Plate X. represent the same outline gradually passing off the Earth's face between the epochs corresponding to the last two figures in the folding plate. The encroachment and the passing off not being strictly uniform,* these lines are not equi-distant, nor are they strictly parallel. (They should not be absolutely straight, since they really form short arcs of circles; but this consideration is relatively unimportant.)

Now, these two plates give us all we require for determining what are the best stations, whether for Delisle's or Halley's method.†

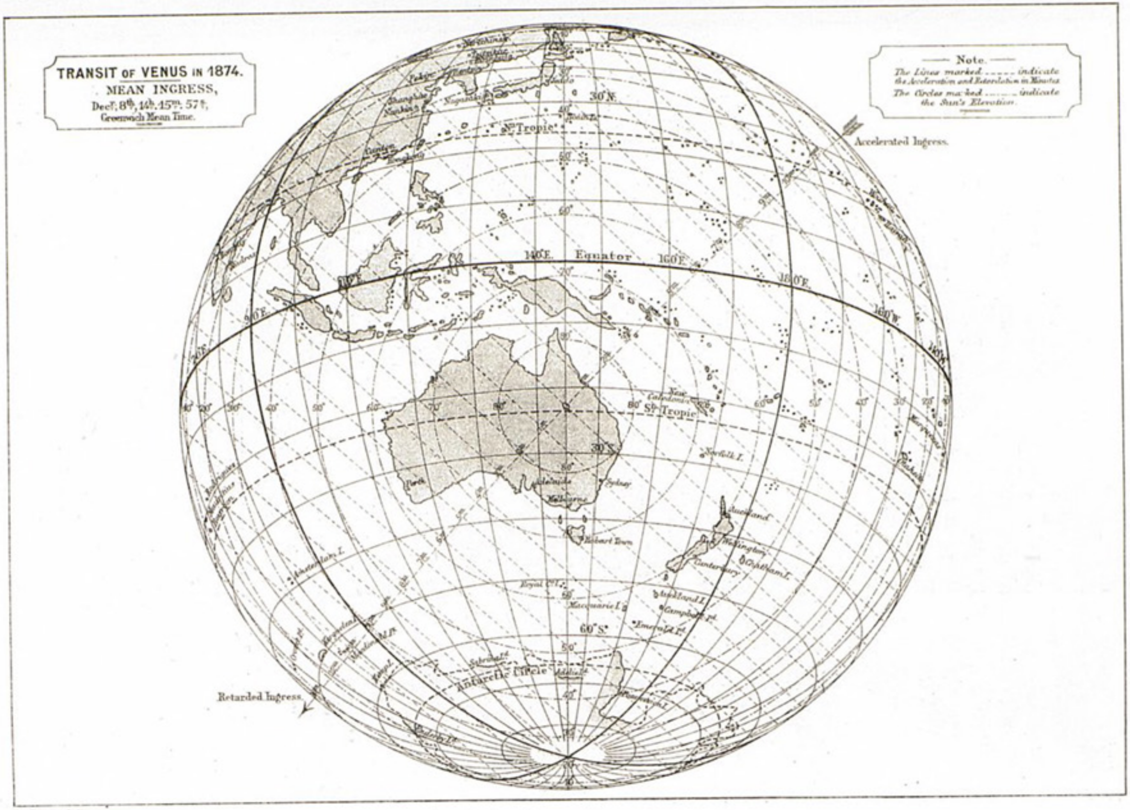
For Delisle's, applied as at ingress, consider Plate IX. We

---

* The reason of this will be seen by a reference to fig. 93. Obviously the rate at which the Earth's centre is approaching the centre of Venus (which rate really measures the rate of encroachment) diminishes during ingress, while for a like reason the rate of passing off increases during egress.

† Properly speaking Plates IX. and X. only represent the Earth accurately for the moment when the outline of v v′ (fig. 93) crosses the Earth's centre. Since, as we see by the cross-lines, no less than 25m. 6s. are occupied by the passage of the outline of v v′ over the Earth's face, both at ingress and egress, the Earth's rotation has to be considered. This, however, can very easily be done, since the latitude circles are shown, and the longitude circles are separated by ten degrees, corresponding to the Earth's rotation in forty minutes. Thus from Plate IX. we see that the cross-line marked 7m. on the right of the centre passes near Jeddo. But as the cross-line occupies this position seven minutes before it crosses the Earth's centre, we must put Jeddô back through an amount corresponding to seven minutes' rotation, or about one-sixth of the distance separating two longitude circles in this neighbourhood.

Pl. IX



TRANSIT OF VENUS IN 1874.
MEAN INGRESS,
Decr. 8th, 14h. 15m. 57s.,
Greenwich Mean Time.

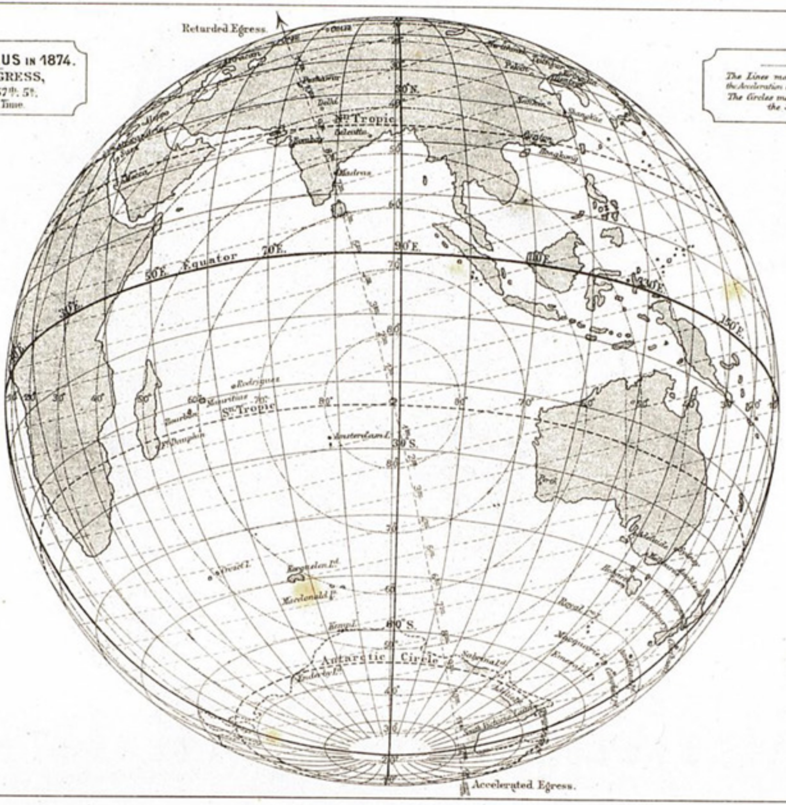
— Note. —
*The Lines marked - - - - - indicate the Acceleration and Retardation in Minutes*
*The Circles marked — - — - indicate the Sun's Elevation.*

R. A. Proctor, Delt.

Pl. X.

# TRANSIT OF VENUS IN 1874.

MEAN EGRESS,
Decr. 8th. 17h. 57m. 5s.
Greenwich Mean Time.



Note.
*The Lines marked - - - - - - indicate the Acceleration and Retardation in Minutes*
*The Circles marked -·-·-·- indicate the Sun's Elevation.*

F. A. Proctor, Delt.

want stations where the acceleration is greatest and where the retardation is greatest. The first lie at the point where accelerated ingress is written. We must not choose a station *at* this point, because there the Sun would be on the horizon, and therefore distorted; but taking the line representing the places where ingress is beginning a minute or so later we see that it passes near Woahoo and Hawaii. These, then, are good stations for observing this phase. Three or four minutes later the line passes Jeddo, Bonin, Marquesas, Otaheite, and so on; and these, therefore, though not such good stations as Hawaii or Woahoo, are still excellent. We may note, too, that at these stations the Sun will have a greater elevation, the actual elevation at different stations being indicated by the concentric circles marked with degrees.

As respects retarded ingress, we see that the best station is Crozet Island, close by the point marked retarded ingress. But Kerguelen's Island, Macdonald Island, Amsterdam Island, as also Rodriguez, Mauritius, and Bourbon, are all good stations for observing this phase.

Now let us turn to Plate X. to determine what stations are best for observing accelerated and retarded egress.

It will be seen that the place marked accelerated egress falls inconveniently near to the south pole. Only when we reach the cross-lines marked 11m. and 10m. do we come on places where stations could be conveniently taken. The lines marked 9m. and 8m. bring us past several excellent stations in New Zealand; and then we come to stations in South Australia; and on the other side of the arrow-line we find Kemp Island, and (inferior, but still serviceable) the Macdonald Islands, Kerguelen, and Crozet.

Lastly, as respects retarded egress, we find an abundance of excellent stations, the best being in Siberia and Eastern European Russia; but there are several excellent stations in India; * while Alexandria will supply a very suitable place of observation.

---

* The very best station in English territory, namely Peshawur—far superior to Alexandria both as respects the amount of retardation and

Tables at the close of this Appendix exhibit the actual amount of acceleration and retardatiom at the several stations indicated in these maps, and at some others not named in either plate.

Now, as respects Halley's method, it will be remembered (see page 33) that we have to consider that the whole transit (or at least the beginning and end) * should be visible. We want stations (these will obviously be northern ones) where the transit will last as long a time as possible, and other stations where the transit will last as short a time as possible. We should therefore naturally look for northern stations where the transit begins as early as possible and ends as late as possible, and *vice versâ* as respects southern stations.

But when we consider Hawaii and Woahoo, where the transit begins at the earliest, we find, on turning to Plate X., that these stations will not suit our purpose; for in Plate X. they are not visible; in other words, before the end of transit Hawaii and Woahoo pass to the un-illuminated side of the Earth,—the Sun sets, in fact, and the end of the phenomenon cannot be seen. In like manner, if we take those stations shown in Plate X. which are most suitable for observing the retarded egress, we find that at the epoch represented in Plate X. they are not visible; in other words, they are on the un-illuminated side of the Earth, or the Sun has not yet risen at these stations when transit begins.

It needs but a brief study of the two Plates to see that the stations which give the longest duration are those in Man-

---

solar elevation—had wholly escaped notice until my construction of Plate X. (reduced from the sixth plate of the original series drawn by me for the Royal Astronomical Society, and published in vol. xxix. of their *Proceedings*) exhibited the advantages of this station.

* This parenthetical remark may seem strange at first sight; but it must be remembered that there are southern stations (though I do not say any in this case are available) where the beginning of the transit can be seen before sunset and the end after sunrise. It is only necessary to study Plates IX. and X. thoughtfully in order to see that this is the case.

chouria, Japan, and North China, whose names are shown in both maps.

Now, as regards the southern stations where the shortest duration is to be observed, we have in some respects a wider selection, for the obvious reason that day lasts longer at these southern stations in December (a relation corresponding, of course, to the longer portions of southern latitude-parallels shown in both maps).

We require to find a southern station where the transit will begin as late and end as early as possible.

All the stations by the place of most retarded ingress in Plate IX. are shown also in Plate X., but in the latter plate they are seen to be very far from the place of most accelerated egress. On the other hand, the stations near the latter place in Plate X., though all visible in Plate IX., are seen in the latter plate to be very far from the place of most retarded ingress.

The best stations as respects proximity to both the place of retarded ingress and that of accelerated egress are Kemp Island, Enderby Land, Sabrina Land, and those in the neighbourhood of these spots. All are in very unsatisfactory and almost inaccessible regions of the Earth's surface.

It happens, however, that Crozet Island, Kerguelen Land, Royal Co. Island, and Macquarie Island give sufficiently shortened transit-periods to afford very satisfactory means of comparison, by Halley's method, with the lengthened transit-periods at Nertchinsk and other neighbouring northern stations.

As regards the elevation of the Sun the difficulty is of course greater at the northern than at the southern stations. But at all the northern stations marked in the maps the Sun will have an elevation exceeding ten degrees at the epochs of internal contact.

On the whole, as will be inferred from the tables at the end of this Appendix, Halley's method will be applicable under very favourable conditions during the transit of 1874.*

---

* It will be seen that I express in the above paragraphs the opinion that Halley's method, which had been pronounced 'wholly inapplicable'

Before proceeding to consider certain other matters belonging to the general subject, I will briefly discuss the transit of 1882.

to the transit of 1874, can be applied under very favourable conditions. In vol. xxix. of the *Monthly Notices of the Royal Astronomical Society*, I have exhibited the calculations requisite to indicate the probable relative values of Delisle's and Halley's method in 1874; and the conclusion to which these calculations point is that Halley's method is on the whole superior to Delisle's. Somewhat before my papers appeared, the French astronomer Puiseux had published a paper expressing his belief that Halley's method could be applied under conditions sufficiently favourable to render it advisable that that method, as well as Delisle's, should be employed. The actual results obtained by Puiseux gave however a slightly inferior position to Halley's method. The difference is due to the fact that M. Puiseux employed approximate instead of exact modes, considering the passage of Venus's centre, for example, instead of internal contacts, and taking no account of the equation of time. My results, not only as relates to the several methods, but as respects those cases in which I deduce different relative values for certain stations suitable for applying either Delisle's and Halley's method, have been now abundantly confirmed by the calculations of Peters, Hansen, and others. They were never indeed seriously questioned, because I was able to point to the exact places where my processes diverged from former and less exact computations, and to show how differences of considerable importance came thus to be discernible in the results. But I must disavow all desire to dwell upon or to magnify errors either of computation or of plan in the work of the eminent astronomer who preceded me in dealing with this problem. The work was not undertaken by me, as I fear the Astronomer Royal judged at the time, in any spirit of captious criticism. Deeply imbued with a sense of the extreme interest and importance of the problem of determining the Sun's distance, and attracted to it also by the exceedingly beautiful nature of the geometrical considerations it involves, I worked at it without any reference in the first place to the labours of others. Only when I found that my results differed in many respects (which seemed to me, and still seem, important) from the Astronomer Royal's, was I led to compare his processes with my own, and to trace out the causes which led to the difference in the results. I was prepared to find I had fallen into some error. As the reverse appeared, and as his results had been made widely public, and were, as I believed, to be made the basis of the choice of stations and methods for English observers in 1874, I should have been wanting in my allegiance to the cause of science had I failed to

In the first place it is well to have a picture indicating the relations which Plates IX. and X. indicate for the transit of 1874. The time has not yet come perhaps when so carefully-constructed a drawing is needed; but a drawing of a somewhat similar nature is essential to the adequate illustration of the subject. I might avail myself here of the Astronomer Royal's two drawings, which will be found in Guillemin's 'Heavens' (and are repeated, with Mr. Airy's statements, in Mr. Lockyer's 'Elementary Lessons of Astronomy'); for the corrections involved by the considerations I have attended to in the case of the transit of 1874, have (for reasons which need not be entered into) a far less important effect in the case of the later transit. But I do not find myself on the whole content to adopt this course, partly because the differences (as any one will see by comparing figs. 95 and 96 with the Astronomer Royal's drawings) are quite appreciable; secondly, because the cross-lines which indicate the passage of the boundary of Venus's shadow-cone over the face of the Earth have not been separated by minute intervals, as in my maps (but by tenths of the total interval of passage), and are not quite correctly placed; and, thirdly, because I think it on the whole more worthy of the student of science to give his own work in such instances.

Figs. 95 and 96 show the exact presentation of the Earth

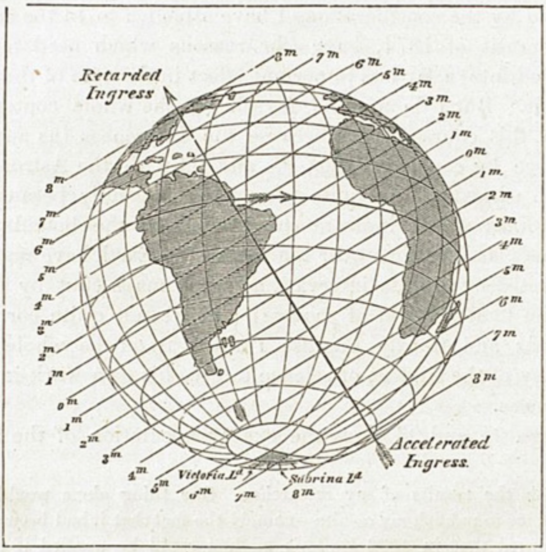
publish the results of my researches. One thing alone would have forced me to publish my results—namely the fact that it had been widely announced that in 1882 Halley's method could be applied if certain Antarctic stations were reached, whereas my calculations serve to prove that there is absolutely no station where Halley's method can be applied in 1882 under conditions sufficiently favourable to warrant the dangerous expeditions and the protracted stay in Antarctic stations by which alone the requisite observations could be made. I repeat here, and urge as the main reason for the earnestness with which I have pressed my views, that to send expeditions to survey the neighbourhood of the proposed stations near Victoria Land and Repulse Bay, and to select either of these neighbourhoods for wintering in anticipation of the summer (Antarctic) transit of 1882, would be to risk the lives of British seamen and men of science without any prospect of adequate return.

at the begining and end of the transit of 1882. The general relations indicated correspond precisely to those dealt with already.

As regards the application of Delisle's method at ingress, we have for the accelerated phase few convenient stations, those best situated being very near the Antartic regions. Kerguelen's Land, parts of Madagascar, and the Cape of Good Hope seem the best. The retarded phase can be viewed under singularly favourable circumstances, however, since the whole seaboard of the United States and many inland towns there and in Canada fall close by the place of most retarded ingress. Many West Indian stations also seem not unsuitable.

TRANSIT OF 1882. (INGRESS.)

FIG. 95.

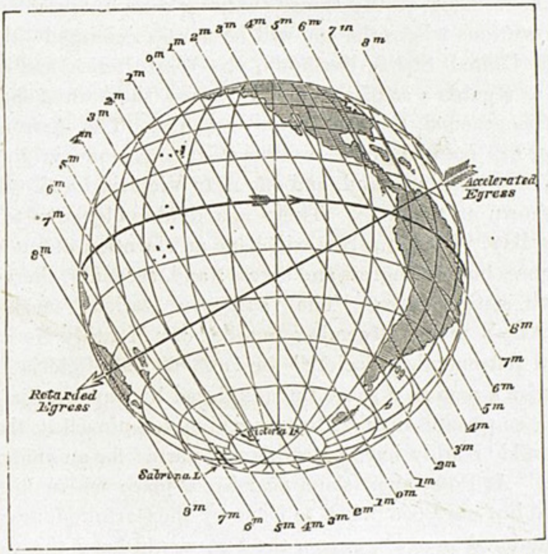


Illuminated side of the Earth at ingress, Dec. 6, 2h. 15m. 56s (Greenwich mean time.)

As regards egress, we also find a number of well-suited stations, many of which are identical with those just referred to as well suited for observing the retarded ingress. It is well that these doubly fortunate stations are American, since our American brethren in science are far more warmly supported by their Government than ours are (or perhaps I should say their cause is far more warmly advocated before their Government).

TRANSIT OF 1882. (EGRESS.)

FIG. 96.



Illuminated side of the Earth at egress, Dec. 6, 8h. 0m. 32s. (Greenwich mean time.)

Retarded egress can be observed favourably in New Zealand, and parts of Australia.

Now, as respects the application of Halley's method, I find myself approaching a delicate subject. It has been so long asserted (thirteen years at least) that Halley's method is only applicable in 1882, that many will be disposed to ridicule the notion that there can have been any mistake in the matter. Yet, confident in the simple mathematical relations dealt with (and confirmed also by the view of the matter now steadily growing among mathematicians), I point to figs. 95 and 96, and ask where the stations are to be placed? So far as the shortened transit observed in northern stations is concerned, there is no difficulty; since, as we have seen, the commencement of the transit will be most retarded at the very stations where the end will be most accelerated—that is, in the United States, Bermuda, the West Indies, and so on. But as regards a southern station where the transit shall be most lengthened, the case is not so simple. The Astronomer Royal has pointed to two southern stations—one at Repulse Bay, near Sabrina Land, and one near Victoria Land. These are shown on the map. There is a double objection to Repulse Bay. First, the Sun will have an elevation of but about 4 degrees both at ingress and egress; and, secondly, there is no known station there. The 'suggested station' marked in Mr. Airy's maps (December number of 'Monthly Notices of Royal Astronomical Society' for 1868-69) at Victoria Land, occupies a spot on a shore line (explored by Captain Sir J. C. Ross) so precipitous that Ross could not approach it, though he would 'readily have given his right arm' for an anchorage there. At Possession Island near by, a place where his men landed but were compelled to leave by the fearful odour of the accumulated penguin guano, the Sun would have the totally insufficient elevation of but about 5 degrees at ingress.

Now, remembering that it has been shown by skilful Arctic and Antarctic seamen that to make observations at these so-called stations on December 6, 1882, astronomers would have to winter there (making their way to them in February or March, 1882, at latest), it surely will not be thought that the poor observations which could alone be made on a Sun but 4 or 5 degrees above the horizon are worth the expense and great risk which must needs be involved. But even if they were, then *à fortiori* would the application of Halley's method in

1874 be called for; since I have been able to demonstrate that without seeking such dismal regions, a more favourable opportunity will be afforded in 1874 than in 1882, even on the supposition that the selected stations were visited during the latter transit. And this result has been confirmed by mathematicians of eminence.

As regards other southern stations in 1882, it needs but a brief study of figs. 95 and 96, to see that none are available. All the stations on the right of the arrow-line in fig. 95, and close by the arrow-feather, are unseen in fig. 96—that is, the Sun has set at those stations. The barren Antarctic land south of Cape Horn (Graham Land) is not very favourably situated in fig. 95, but in fig. 97 has passed over to the wrong side of the central cross-line; in other words, there is acceleration of egress instead of the needed retardation. Similar remarks apply to the southern parts of South America.*

So far, then, is it from being true that Halley's method is wholly inapplicable in 1874 but is applicable under favourable circumstances in 1882, that the exact reverse is the case. Halley's method is even better than Delisle's in 1874, and is wholly inapplicable in 1882.†

---

* The reasoning in the note beginning on p. 37 will be found to be well illustrated by a comparison of Plates IX. and X. with figs. 95 and 96. It will be seen that in the transit of 1874 the rotation of the Earth carries the northern stations the *wrong way* as regards that lengthening which is required; while in the transit of 1882 the northern stations are carried the *right way* for the shortening which is required. As to the southern stations, it will be seen how, according as they lie above or below the pole (as shown in the maps), the lengthening or shortening can be increased or diminished. It is the fact that no stations below the pole are suitable in 1882, which spoils the otherwise sound reasoning of the Astronomer Royal.

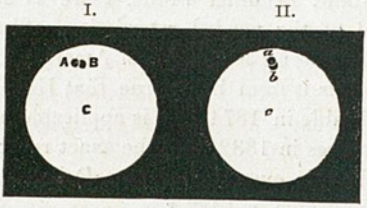
† It will be seen from fig. 93 that the path of the Earth crosses the surface of the shadow-cone much more obliquely in 1874 than in 1882; and, as this path is as it were the reflexion of the apparent path of Venus over the Sun's disc, Venus crosses the limb much more obliquely in 1874 than in 1882. Thus the actual progress of ingress and egress must take place much more slowly. Mr. Stone believes that all time-errors will be correspondingly increased. The comparison above referred to is founded

And now I propose to indicate, in conclusion, considerations which lead me to believe that the direct method of determining the actual parallactic displacement of Venus on the Sun's disc can be applied under very favourable circumstances during the approaching transits.

Let us first consider if we cannot in part diminish the difficulties which seem inherent in this method.

Suppose an observer at one station sees Venus at a given epoch as at A (fig. 97, I.), while at the same epoch another observer sees her at B. Then it is perfectly obvious that in such a case as is illustrated by I., when the two observations are brought into comparison, any error affecting the *bearing* of B or A from C will tend to produce its full effect in increasing or diminishing the parallactic displacement separating A and B. But now suppose that *a* and *b* in II. represent the

Fig. 97.



Illustrating the displacement of Venus as seen from different terrestrial stations during transit.

---

on this opinion, very unfavourable to my views. I must, however, express my own conviction that though the time-errors will be larger in 1874 they will not be proportionately increased. The slowness of ingress and egress will give the observer an opportunity of observing the features of ingress and egress more satisfactorily. Especially if my suggestion is attended to as regards the breadth of the ligament at the moment of breaking or forming (see p. 63) will this be the case. Apart from this, as all the time-differences are increased precisely in proportion to the slowness of ingress or egress, a very slanting passage is *cæteris paribus* as favourable as a more direct one.

true apparent positions of Venus as seen from the two stations —*a*, *b*, and *c* (the centre of the Sun's disc), lying on, or nearly on, a line. Then it is clear that a small error of bearing will produce no appreciable effect in increasing or diminishing the parallactic displacement separating *a* and *b*.

Hence, since distances are much more easily determined than bearings in observations such as are here considered, it is obvious that if we select pairs of stations where the parallactic displacement of Venus will be along a radius of the Sun's disc, the resulting estimate of the distance of Venus is much more likely to be correct than in any other possible case.

Now, if we inquire what are the conditions to be fulfilled for this purpose, we shall find it exceedingly easy to obtain a geometrical means of determining what the answer should be. Lines drawn to Venus from two stations on the Earth meet the Sun's disc *on a radius*, if they are to fulfil the required conditions. In other words, these lines are in a plane which passes through the Sun's centre, and of course they are in a plane through Venus's centre, since each passes through that centre. Hence they are in a plane through the axis of the cones shown in fig. 92; for this axis passes through the centres of Venus and the Sun. But a plane through the axis of these cones must intersect such a section as $v\ v'$ in figs. 92 and 93 in a radial line. Hence the two terrestrial stations must at the moment of observation be on a line through o in fig. 93. Now, *the slanting lines over Plate VIII. are such radial lines; and therefore any two stations which in any figure of this plate lie along one of the slanting lines fulfil the required conditions.* They must, however, be as far apart as possible. For example, taking the fifth picture of the Earth in Plate VIII., we see that Japanese stations and a station near Crozet Island could be very effectively combined at the hour named under this particular presentation of the Earth.

Of course the best of all such opportunities would arise when the two stations lay near opposite extremities of one of the cross-lines traversing the middle of the Earth's disc.

The various stations which fulfil the required conditions can be very readily determined from a study of Plate VIII.

The application of photography, as proposed by Mr. De La Rue and Lieut.-Col. Tennant, would be greatly facilitated by the choice of stations on the principle here enunciated. (See, further, my paper in the 'Monthly Notices of the Royal Astronomical Society,' No. 3, vol. xxx.)

The following tables, which I have calculated with great care, supplement this essay, and serve to exhibit the qualities of the principal stations suited for observing the transit of 1874, either as respects De Lisle's or Halley's method :—

## TRANSIT OF 1874.

TABLE I.—*Places where Ingress is accelerated.*

| Station | Sun's elevation | Acceleration in minutes |
|---|---|---|
| | deg. | m. |
| Woahoo . . . | 19·8 | 11·2 |
| Hawaii . . . | 19·7 | 11·1 |
| Aitou Id., Aleutian . | 10·8 | 10·3 |
| Marquesas Island . | 17·7 | 7·9 |
| Mouth of Amoor R. . | 14·0 | 7·6 |
| Jeddo . . . | 32·1 | 6·8 |
| Otaheite . . . | 29·7 | 6·4 |
| Nertchinsk . . | 10·1 | 5·8 |
| Tsitsikar . . | 17·0 | 5 8 |
| Kirin-Oula . . | 19·5 | 5·7 |
| Nagasaki . . | 32·7 | 5·3 |
| Tientsin . . . | 22·2 | 5·0 |
| Pekin . . . | 20·8 | 4·3 |
| Shanghai . . | 28·5 | 3·9 |
| Nankin . . . | 27·1 | 3·6 |
| Canton . . . | 35·5 | 1·6 |
| Hongkong . . | 36·2 | 1·6 |

TABLE II.—*Places where Ingress is retarded.*

| Station | Sun's elevation | Retardation in minutes. |
|---|---|---|
| | deg. | m. |
| Crozet Island . . | 15·0 | 12·6 |
| Enderby Land . | 20·0 | 11·8 |
| Kerguelen Land . | 27·5 | 11·6 |
| Macdonald Island . | 31·0 | 11·2 |
| Kemp Island . . | 30·0 | 11·1 |
| Bourbon Island . | 12·4 | 11·1 |
| Mauritius . . | 14·1 | 10·7 |
| Amsterdam Island . | 34·1 | 10·3 |
| Rodriguez . . | 19·0 | 9·9 |
| Sabrina Land . . | 45·0 | 8·2 |
| Adélie Land . . | 45·0 | 6 8 |
| South Victoria Ld. . | 38·5 | 6·0 |
| Perth (Australia) . | 65·0 | 5·3 |
| Royal Co. Island . | 62·0 | 4·5 |
| Madras . . . | 21·0 | 4·0 |
| Bombay . . . | 12·5 | 3·8 |
| Macquarie Land . | 52·0 | 3·5 |
| Hobart Town . . | 67·0 | 2·8 |
| Adelaide . . | 75·0 | 2·5 |
| Melbourne . . | 75·0 | 2·2 |

## TRANSIT OF 1874.

TABLE III.—*Places where Egress is accelerated.*

| Station | Sun's elevation | Acceleration in minutes |
|---|---|---|
| | deg. | m. |
| South Victoria Land (Possession Ilnd.) | 25·0 | 11·4 |
| Adélie Land | 34·0 | 10·6 |
| Campbell Island | 26·0 | 10·3 |
| Emerald | 30·0 | 10·3 |
| Macquarie Island | 32·0 | 9·8 |
| Chatham Island | 16·0 | 9·8 |
| Canterbury (N.Z.) | 22·5 | 9·3 |
| Wellington | 20·0 | 9·2 |
| Sabrina Land | 43·0 | 9·2 |
| Enderby Land | 39·0 | 8·5 |
| Royal Co. Island | 42·0 | 8·5 |
| Auckland | 19·2 | 8·5 |
| Kemp Island | 51·0 | 7·6 |
| Hobart Town | 40·0 | 7·6 |
| Melbourne | 43.0 | 6·6 |
| Sydney | 37·2 | 6·6 |
| Adelaide | 47·8 | 5·8 |
| Kerguelen Land | 57·1 | 5·0 |
| Crozet Island | 47·5 | 4·2 |
| Perth (Australia) | 66·2 | 3·6 |

TABLE IV.—*Places where Egress is retarded.*

| Station | Sun's elevation | Retardation in minutes. |
|---|---|---|
| | deg. | m. |
| Orsk | 12·5 | 11·8 |
| Omsk | 11·5 | 11·7 |
| Astracan | 12·0 | 11·6 |
| Aleppo | 14·6 | 10·5 |
| Peshawur | 31·5 | 10·3 |
| Alexandria | 14·0 | 10·0 |
| Suez | 16·1 | 9·8 |
| Nertchinsk | 10·1 | 9·8 |
| Delhi | 38·0 | 9·4 |
| Tsitsikar | 12·0 | 8·7 |
| Bombay | 45·0 | 8·5 |
| Pekin | 21·0 | 8·6 |
| Kirin-Oula | 14·0 | 8·4 |
| Tientsin. | 17·1 | 8·4 |
| Calcutta | 45·3 | 8·2 |
| Aden | 30·0 | 7·8 |
| Nankin | 27·0 | 7·6 |
| Madras | 52·0 | 7·4 |
| Shanghai | 26·0 | 7·2 |
| Canton | 37·0 | 6·6 |
| Hongkong | 37·0 | 6·5 |

## TRANSIT OF 1874.

Table V.—*Showing the actual difference of duration when comparison is made between different stations.*

| Stations where the duration is lengthened | Stations where the Duration is Shortened | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | South Victoria Land | Adélie Land | Sabrina Land | Macquarie Land | Royal Co. Island | Hobart Town | Perth (Aust.) | Melbourne | Adelaide |
| | Differ. m. | Differ. m. | Differ. m. | Differ. m. | Differ. m | Differ. m. | Differ. m. | Differ. m. | Differ. m. |
| Nertchinsk | 33·0 | 33·0 | 33·0 | 28·9 | 28·6 | 26·0 | 24·5 | 24·4 | 23·9 |
| Tsitsikar | 31·7 | 31·7 | 31·7 | 27·6 | 27·3 | 24·7 | 23·2 | 23·1 | 22·6 |
| Kirin-Oula | 31·5 | 31·5 | 31·5 | 27·1 | 26·8 | 24·5 | 23·0 | 22·9 | 22·4 |
| Tientsin | 30·8 | 30·8 | 30·8 | 26·7 | 26·4 | 23·8 | 22·3 | 22·2 | 21·7 |
| Pekin | 30·3 | 30·3 | 30·3 | 26·2 | 25·9 | 23·3 | 21·8 | 21·7 | 21·2 |
| Nankin | 28·6 | 28·6 | 28·6 | 24·5 | 24·2 | 21·6 | 20·1 | 20·0 | 19·5 |
| Shanghai | 28·5 | 28·5 | 28·5 | 24·4 | 24·1 | 21·5 | 20·0 | 19·9 | 19·4 |
| Canton | 25·6 | 25·6 | 25·6 | 21·5 | 21.2 | 18·6 | 17·1 | 17·0 | 16·5 |
| Hongkong | 25·5 | 25·5 | 25·5 | 21·4 | 21·3 | 18·5 | 17·0 | 16·9 | 16·4 |

# APPENDIX B.

## *ECLIPSES.*

SOLAR Eclipses play so important a part in aiding our study of the Sun, that this work would be incomplete without an account of the laws according to which these phenomena occur. Even lunar eclipses bear significantly on certain questions of solar physics; so that since the two orders of eclipses cannot very well be considered apart, the general theory of eclipses will be presented in this essay. This is the more desirable because in all the popular treatises on astronomy with which I am acquainted the subject of eclipses is but inadequately dealt with. I wish, without entering into the discussion of minutiæ, to convey a clear idea of the orderly sequence which characterises the occurrence of eclipses.

Let the reader first endeavour to conceive the Moon's orbit as a ring accompanying the Earth as she moves onward in her path round the Sun. This ring is not truly circular, nor is the Earth at its centre, nor does it occupy a fixed position in space, nor is its size invariable; but all these points we may for the moment overlook. The one point to which I desire the reader to direct his special attention is the fact that this orbit (neglecting perturbations) is to be conceived as accompanying the Earth and shifting parallel to itself along the Earth's orbit; so that, as seen from the Sun, this orbit changes in shape precisely in the same way that the Earth's equator changes during the course of a year. The orbit may be said in fact to have its summer and winter and its equinoxes, corresponding to the epochs when it is tilted with its northern or southern side towards the Sun, or when its plane is turned directly towards him. The orbit opens out and closes up precisely as the ring-system of Saturn does, only in the course of rather less than a year instead of nearly thirty years, and also within much narrower limits of change.

There are reasons which render it unadvisable to attempt to illustrate the motion of the Moon's orbit around the Sun directly; the chief being that the distance of the Earth from the Sun so enormously exceeds the diameter of the Moon's orbit that a picture accurately drawn to scale and intended to show the Moon's orbit ought to be several yards in diameter. On the other hand, pictures not drawn to scale are most unsatisfactory illustrations of astronomical relations.*

Let us, however, picture the Moon's orbit, as supposed to be seen from the Sun's centre at different epochs of its passage around that orb.

First of all, let us consider the orbit when so placed as to appear like a straight line across the Earth's disc. It is then as shown at I. in fig. 98; in which E represents the Earth, $mm'$ the Moon's orbit, M the Moon at a point on her orbit (this, merely to indicate her relative dimensions); and the dotted line a part of the Earth's path (that is, a part of the ecliptic). The arrow on this line shows the direction in which the Earth is moving while the small arrow on $m\,m'$ shows the direction of the

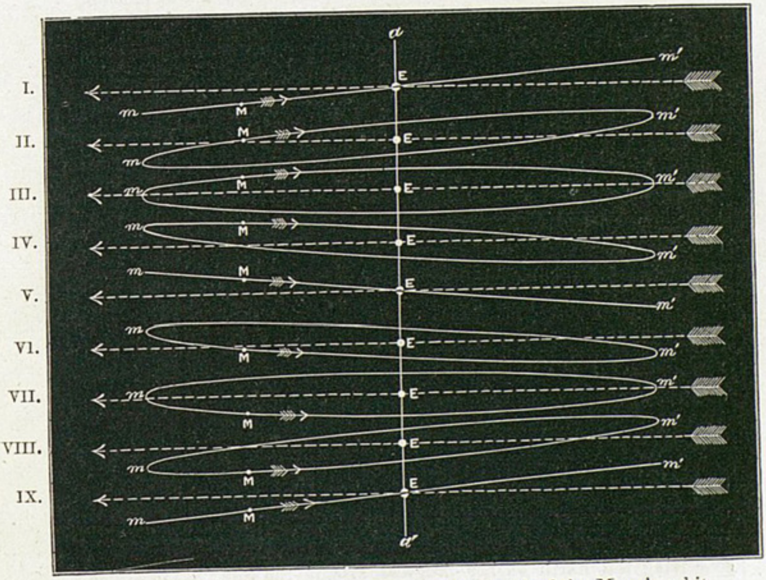
---

* I am convinced that a large part of the perplexity which intelligent and thoughtful readers experience in the study of astronomical works is due to the incorrect proportions of the figured objects – orbits, globes, and so on. I believe, also, that but for these monstrous pictures the charlatans who pretend they think the Earth a plane or the like, would not find hearers, still less (as they *do*) believers. I know many worthy people, far from wanting in abilities, who only believe the theories of Copernicus, Kepler, and Newton, on the score of authority,—not because the evidence in astronomical treatises seems to them convincing or even intelligible.

Moon's motion. It is to be noted that the arrow is supposed to lie on the nearer part of her orbit in all the presentations included in fig. 98.

Now, the Earth moving off towards the left, while the orbit *m m'* moves along with her and parallel to itself, it is perfectly obvious that we shall begin to see the orbit open out, its lower side (regarding it as a plane) becoming visible. In other words, it will begin to assume such an appearance as is shown at II. the nearer half uppermost and the slope diminished (the figures, letters, and arrows representing the same relations as before.)

Fig. 98.



Illustrating the apparent changes of the figure of the Moon's orbit as supposed to be viewed from the Sun.

But to simplify matters at this stage, let us consider the changes in another aspect. Since the Earth is going off towards the left and round the observer, who is supposed to be at the Sun's centre, it is plain that the same changes will take place in the appearance of the Moon's orbit as though the latter remained fixed and the observer set off towards the right, and so went round the Earth. But we get the same changes of view in going round an object as though we remained still and the object simply turned round. For example, let fig. 99 represent a plant in a flower-pot, and suppose that an observer, keeping his eye on a level with the rim of the flower-pot, went round the pot, setting off towards the right: then it is perfectly obvious that he would see the same successive views of the plant as though he remained still and the flower-pot were slowly *twirled* (not *shifted*) by means of the suspending cords in the direction indicated by the arrow. Applying this consideration to the case in hand, we may conceive E in fig. 98 to be slowly rotated on the axis $a\,a'$, carrying the orbit $m\,m'$ with it, as if that orbit were rigidly attached to E; the rotation being such as to carry the nearer part of the orbit towards the left. Then all the changes of appearance will be precisely the same as occur in the actual case in which the Earth moves bodily off towards the left around the observer in the Sun.*

Fig. 99.

---

* This mode of considering the case is not only very convenient in this special instance, but also in many other cases. In such a way, a much clearer conception may be formed of the orderly succession of the seasons (see my *Sun Views of the Earth*) than by the ordinary way of conceiving the Earth on her progress around the Sun, a conception rendered difficult to the general reader, on account of the twofold orders of change—change of place and change of relative bearing—which have to be attended to. In like manner, the changes in the appearance of the Saturnian ring-system, as supposed to be seen from the Sun, are far better understood in this way than in any other. The general principle on which this mode of considering such cases depends may be thus enunciated:—*When a body or system shifts parallel to itself as it circles around a centre, the changes in the appearance of the body or system, as supposed to be seen from that centre, are precisely the same as though the body or system rotated on an axis at*

The reader will at once see how the successive appearances represented in fig. 98, from I. to IX., would result from this simple rotation of the whole scheme upon *a a′*.* We see the orbit-ring opening out and becoming level, closing up and becoming inclined with the left extremity uppermost, opening out again and again becoming level, and lastly closing up and becoming inclined, as at first, with the right extremity uppermost. The whole rotation is supposed to take place in the course of a year, because the Earth takes one year in completing her circuit. Further on we shall have to consider a peculiarity which causes all these changes to take place in rather less than a year; but for the present we are not concerned with details of the sort.

Now, it only needs a glance at fig. 98, to see that when the orbit is presented as at I. there will be an eclipse of the Earth if the Moon is on the nearest part of her orbit, and an eclipse of the Moon if the Moon is on the farthest part of her orbit. For it is to be remembered that we are supposed to be stationed at the Sun; so that if M hides any part of E from us (*i.e.*, from the Sun) that part of E must be in shadow; while if E hides any part of M, or the whole of M, from us, that part

---

*right angles to the plane of its actual orbit, in a direction contrary to that of its real motion, and in the same period.* In such a case as the illustration of the Earth's seasons, we must, after considering each change of bearing, consider the effect of a complete rotation of the Earth on her own axis; just as in the case dealt with in the text we consider the Moon's revolution in her orbit in addition to the successive changes in the aspect of the orbit. In my *Sun Views of the Earth* the twelve successive plates correspond to twelve changes in the Earth's *general* presentation towards the Sun during the course of the twelve months, while the four pictures in each plate correspond to the changes in the course of a day (at intervals, therefore, of six hours), owing to the Earth's rotation. So illustrated, this method of considering the subject of the seasons becomes singularly simple and truthful.

* Of course the arrow on *m m′*, and the globe of the Moon at M, are simply put in each figure at that part of the orbit which is most convenient, and are not supposed to be carried round with the rotation here specially dealt with.

or the whole of M is in shadow. But when the orbit is presented —a quarter of a year later—as at III., there can be no eclipse, wherever the Moon may be on her orbit. A quarter later, when the orbit is presented as at V., the same state of things results as at the beginning; and yet another quarter later, when the orbit is presented as at VII., no eclipse is possible.

The figure is drawn as nearly as possible to scale, and we see that the intermediate presentations of the orbit as at II., III., VI., and VII., are such that there can be no eclipses. We infer, therefore, that eclipses can only happen when the orbit is presented as shown at I., and for some relatively short time before and after that epoch. At such times, whenever the Moon (M) crosses the place of E, on the nearer or farther half of M's orbit, an eclipse must occur. But after that eclipse-season (if I may invent a word) has passed there can occur no eclipses of either sort until nearly half a year has passed and the presentation of the orbit has approached that shown at V. Then, for a while, eclipses are possible. Lastly, after this eclipse-season has passed, another period of nearly half a year passes during which eclipses cannot happen. And so on continually.

All this is perfectly simple and obvious. The recognition of the fact that these eclipse-seasons recur at intervals of about half-a-year tends also importantly to simplify the consideration of the whole matter. Let me note in passing that the term eclipse-season is not ill-chosen, inasmuch as one eclipse at least must needs takes place while the presentation of the orbit is changing through the critical aspects, such as I., V., IX. (fig. 98), and so on.

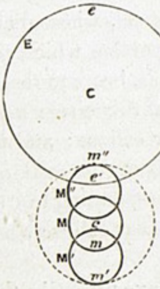
But let us now enter somewhat more into details. And first let us inquire how much the orbit *m m′* must be opened out in order that the Moon (M) may pass clear of E, whether on the nearer or farther side, in such sort that there may be no eclipse.

We have hitherto, for convenience, supposed the observer at the Sun's centre. But now we must give him liberty to traverse the whole of the Sun's globe; or rather we may suppose that millions of eyes placed all over the Sun's surface are viewing the changes pictured in fig. 98. For if *any* part of the Sun is concealed from the Earth or Moon a solar or lunar eclipse—partial or otherwise—is in progress.

Now, suppose the Earth and Moon, as seen from the Sun's centre when the Moon is passing close by E in fig. 98, to be represented by the discs E and M (fig. 100.) Then M is either on this side of E or beyond E—that is, it is either new Moon or full.

First, suppose M on this side of E. Then if our observer leaves the Sun's centre, and goes to the *uppermost* point of the Sun's surface,* both E and M will seem lower down on the background of the heavens—E by about sixteen minutes of arc

FIG. 100.



---

[*] I use here the familiar expression *uppermost* and further on, the familiar expression *lowermost*. It is not always the case, however, that familiar expressions are the most intelligible. I could cite instances from several popular works on astronomy to show that the use of familiar and ordinary expressions may result in the most perplexing and in reality untrue statements. In the present instance, the term uppermost refers to the relations presented in fig. 100, and the uppermost point on the Sun will be readily understood to signify the point on the Sun corresponding to the point *e* on the globe E representing the Earth. In any general sense, the term uppermost has no meaning as applied to the celestial bodies, and must be classed in the same category with many expressions (too often met with in scientific treatises) whose very simplicity is misleading.

(half the Sun's diameter as seen from the Earth), M somewhat more as being nearer to the Sun. And a very little consideration will show that M will be thus thrown downwards with respect to E, to a position as M′, such that *m m′* bears to *c m* the same proportion that the Sun's semi-diameter (as seen at the time from the Earth) bears to the Moon's. On the other hand, if our observer proceeds to the lowermost point of the Sun's surface he will see the Moon projected as far upwards, or to the position M″ *m″*. And clearly, by shifting his place to other portions of the Sun's edge as seen from the Earth, he would see the Moon shifted in other directions; the whole region covered by the Moon during these excursions corresponding to the circle *m′ m″*, whose diameter bears to the diameter of M the same proportion which the Sun's apparent diameter, added to the Moon's, bears to the Moon's diameter alone.

Now, if any portion of this circle *m′m″* overlaps the circle E *e*, there will be a solar eclipse; and this amounts to saying that if half the lesser axis of the oval orbit *m m′* in fig. 98 be less than the amount corresponding to C *e′* together with *c m′* in fig. 100, there will be a solar eclipse when the Moon is passing by E.

And clearly we shall have precisely the same relations when the Moon is close by E on the further part of the orbit. The only difference in the reasoning depends on the circumstance that when our imagined observer goes to the uppermost part of the Sun, the Moon is raised instead of lowered with respect to the Earth, and *vice versâ*. But we still get a circle such as *m′ m″* in fig. 100.

Hence it appears that so long as the orbit *m m′* in fig 98 has no greater opening than that corresponding to the sum of the diameters of the circles *e e′* and *m′ m″* in fig. 100, there will be a solar or lunar eclipse when the Moon is passing by E (fig. 98) either on the nearer or further part of her orbit.

But we can readily tell (with sufficient approximation for our present purpose*) how long the orbit *m m′* (fig. 98)

---

* I have purposely omitted, so far, all reference to certain circum-

takes in changing from the appearance shown at I., V., and IX. to such a degree of opening as enables the Moon to pass by E without an eclipse, and thus we can readily tell what must happen during our 'eclipse-seasons.'

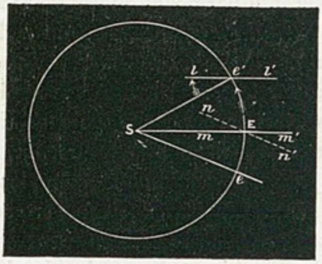
For this purpose, we may now take into consideration a circumstance hitherto left out of sight—the fact, namely, that the Moon's orbit does not move strictly parallel to itself. It would do so were the Sun powerless to disturb the Moon's motions around the Earth. But as a matter of fact the Sun largely influences those motions. Much in the same way that by acting on the protuberant mass of the Earth's equatorial regions he causes the Earth's axis to sway conically round the direction of a perpendicular to the Earth's orbit—making the pole of the heavens travel in a circle around the pole of the ecliptic in a period of more than 25,000 years—so he causes the line through the Earth at right angles to the Moon's orbit to sway conically round the direction of a perpendicular to the Earth's orbit, making the pole of the Moon's orbit travel in a circle around the pole of the ecliptic in a period of rather more than 18½ years, or, more exactly, 6793·391080 days. The motion of the nodes of the orbit—that is, of the points in which the orbit crosses the plane of the Earth's orbit—is *precessional*, like the corresponding motion of the nodes of the Earth's equator—that is, the nodes *advance* as it were to meet the Moon. It is easy to see the effects of this motion on the reasoning applied to fig. 98. Suppose *m* E *m′* (fig. 101) to represent the line in which the plane

---

stances which affect the details above considered, without affecting the general reasoning. For example, the orbit of the Moon, as seen from the Sun, is not truly an ellipse around the Earth as centre (without referring to perturbations or the like). Regarding the Moon's orbit for the moment as a circle about the Earth as centre, a diameter of this circle, as seen from the Sun, would not appear to be bisected at its real point of bisection—unless it were at right angles to the line of view—for its two halves would not be at exactly equal distances from the supposed observer. This and many other similar points, though all-important in an analysis of the details of eclipses, may be safely neglected in considering the general aspect of the subject.

of the Moon's orbit around the Earth (E) intersects the plane of the Earth's orbit round the Sun (S): *m* E *m′* is therefore the *line of the Moon's nodes.* Then at the end of a year when the Earth is again at E, the line of intersection will have taken up such a position as *n* E *n′*, so that it is obvious that before the Earth came to E, or when she was placed somewhat as at *e*,* the nodal line had passed through the Sun. Now, it would be easy to determine geometrically where *e* should fall; but there is in this and all similar cases a far simpler mode of determining the intervals between the successive concurrences of the nodal line with the line joining the Sun and Earth. We see that if the nodal line moved parallel to itself as from *m m′* (fig. 101), to *l l′*, and so on, it would seem to revolve with respect to the line from S to the Earth in the direction shown by the arrow near *l*, and in 18·6 years would make 18·6 revolutions in this direction. But in about 18·6 years the nodal line travels once round in this same direction, owing to the perturbative effects already

Fig. 101.



Illustrating the retrogression of the Moon's nodes.

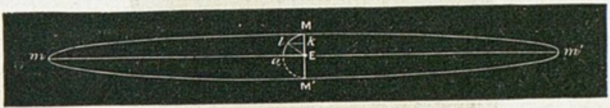
---

* S *e* is not quite parallel to *n* E, but inclined so that the lines would meet towards *n′ e*. It is plain that if S *e* were parallel to *n* E, then when the Earth was at *e* the Moon's nodal line, not having fully reached the *direction n n′*, would not have coincided with the line from *e* to the Sun, the end corresponding to *n* falling in that case below S *e*. The Earth would not therefore have gone quite far enough round towards E. The true place of *e* is therefore such as described.

referred to. Hence in all, in the course of 18·6 years, it makes 19·6 revolutions in this direction. Therefore it makes one revolution in $\frac{186}{196}$ths of a year (approximately), or about 346·6 days, so that half of this time, 173·3 days, or about five months and three weeks, is the mean interval at which our eclipse-seasons succeed each other.

Now the greatest opening of the Moon's orbit as seen from the Sun is easily determined. The Moon's orbit is inclined about 5° 9′ to the Earth's, and therefore if M *m* M′ *m′* (fig. 102) represent the Moon's orbit as seen when most opened, E M or E M′ (half the lesser axis) bears to E *m* or E *m′* the ratio which the sine of 5° 9′ bears to unity, or (nearly enough for our purpose) E M or E M′ is about $\frac{9}{100}$ths of E *m* or E *m′*. But as seen from the Sun the Earth's radius subtends about $\frac{1}{60}$th part of E *m′*, the Moon's about a fourth as much, and (since the mean apparent diameters of the Sun and Moon as seen from the Earth are nearly equal) we may add another fourth for the quantity corresponding to *m m′* in fig. 100. Thus we get in all $1\frac{1}{2}$ sixtieths, or $\frac{1}{40}$th of E *m*, as the length which the half-axis of the Moon's orbit, as seen from the Sun, must not exceed if there is to be an eclipse when the Moon is crossing the Earth's apparent place. Such a half-axis would be about $\frac{5}{18}$ths of E M.*

Fig. 102.



Illustrating the Theory of Eclipses.

But it may easily be shown that whereas E M the greatest opening is obtained by rotating the orbit (in the manner already explained) from the position shown at I., fig. 98, through a right angle, any smaller amount of opening, as E *k* (fig. 102), will be obtained by rotating the orbit through an angle equal to *l* E *a*,

---

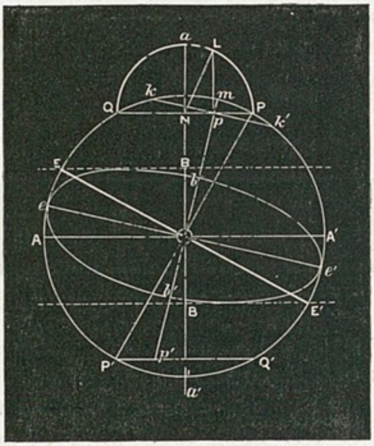
* Obtained by dividing $\frac{1}{40}$ by $\frac{9}{100}$.

obtained, as shown in fig. 102, by drawing the quadrant M *a* and *k l* parallel to E *a*.* And therefore (remembering

---

* This is a very important general proposition, continually involved when we are considering the apparent changes in the presentation of a globe, orbit, or ring;—as the changes in the Earth's presentation towards the Sun (on which the seasons depend), or the changes in the apparent figure of Saturn's rings, or of the orbits of his satellites, or, again, the changes (which belong more particularly to the subject of this treatise), in the presentation of the Sun's latitude parallels—the paths apparently followed by the spots as seen from the Earth. The following simple geometrical proof of the property is worth noticing, and is, I believe, new :—

Fig. 103.



Let E E' be a circle seen edgewise from a very distant station, so as to appear as a straight line, and suppose that the circle rotates bodily about the axis *a a'* through its centre O; it is required to determine the aspect of the circle after any definite amount of rotation. Enclose the circle in a sphere about O as centre, and let P O P', a polar axis of the circle at the beginning of the motion, rotate with it. Then, from the distant station, P and P' will seem to move along right lines P Q and P' Q' parallel to A O A', though in reality the line P O P' will be revolving conically and uniformly about *a a'*. And it is clear that if a always what the rotation corresponds to) it follows that the time occupied by the orbit in opening out from an apparent line to an oval having a half-axis E $k$, will bear to the time occupied by the orbit in obtaining its full opening the same proportion that the angle $l$ E $a$ bears to the right angle M E $a$. But we require this proportion to be such that $k$ E shall bear to M E the ratio 5 to 18; and it follows, therefore, that the angle $l$ E $a$ must be one of as nearly as possible $16\frac{1}{8}$ degrees.*

---

circle be described on P Q as diameter (only half the circle is shown in the figure), then, as P really revolves uniformly round a circle of this size, but seen edgewise, we can determine the apparent position of P after rotating through any given angle, by simply taking P N L equal to this angle, and drawing a perpendicular L $p$ on P Q. At this moment, then, $p$ O $p'$ represents the apparent position taken up by P O P′; and clearly $e$ O $e'$ at right angles to $p$ O $p'$ is the greater axis of the ellipse now presented by the moving circle. The minor axis $b$ O $b'$ lies of course on $p\,p'$. Now in order to determine the length of $b$ O $b'$, conceive the circle to rotate on $e$ O $e'$, till $b$ and $b'$ appear to coincide with O. Then plainly $p$ has moved to $m$ on the edge of the disc presented by the sphere (O $p$ $m$ straight), and it is obvious that the amount of rotation about $e$ O $e'$ necessary to effect this change is measured by the arc $m$ $k$, ($p$ $k$ being drawn square to O $m$), so that O $b$ must be the projection of a radius inclined to the eye as O $k$ is inclined to O $m$. Therefore O $b$ must be equal to $p$ $k$,—that is to L $p$ (for L $p$ and $p$ $k$ are obviously equal, since the square of each is equal to the rectangle Q $p$, $p$ P).

Hence we have a very simple construction for determining the position of the ellipse $e$ $b$ $e'$ $b'$ for any amount of rotation round $a$ O $a'$. This construction in full (starting from nothing given, save E O E′, the position of $a$ $a'$, and the amount of rotation) runs thus:—

Describe the circle E P E′, draw P O P′ square to E E′, and P N square to $a$ $a'$. Describe the arc P L equal to the given rotation-angle, round N as centre, and draw L $p$ square to N P. Then $p$ O $p'$ is the *position* of the lesser axis of the apparent ellipse now formed by the circle originally seen as the line E O E; L $p$ is the length of the half-axes O $b$ and O $b'$, which we can now measure off along $p$ O $p'$; and of course the major axis is simply the diameter $e$ O $e'$ square to $p\,p'$.

Also, since the greatest amount of opening is obviously obtained by drawing E B, E′ B′ parallel to A O A′, and since B O′ B′ is obviously equal to P N Q, the statement made in the text is shown to be just.

* The angle must have a sine equal to 0·277777, and the sine of $16\frac{1}{8}°$ is 0·277734.

Hence the required time is obtained by reducing half the before-mentioned period, 173·3 days, in the proportion of $16\frac{1}{8}$ to 90; that is, as nearly as possible, $15\frac{1}{2}$ days, which must be doubled, because we have to consider the range on either side of the epoch corresponding to the presentations I., V., IX., in fig. 98. Thus, so far as this rough process is concerned, the eclipse-season lasts 31 days, or thereabouts. The real mean is somewhat greater, for the Moon's diameter is more than one-fourth of the Earth's. But, as we have only had in view the general principles on which the recurrence and duration of our eclipse-seasons depend, exact accuracy has not (thus far) been necessary. For our present purpose we shall take thirty-three days as about the average, and consider one or two consequences of this relation.

A period of 33 days is a few days more than a lunation. Hence, supposing that when an eclipse-season is beginning, it is either new Moon or full Moon, there will be three eclipses during that season, for the Moon will pass to full or new, and thence to new or full, before the eclipse-season is over. Now of these three eclipses the first and last will be solar or lunar, and the other lunar or solar. Yet we never hear of a lunar eclipse followed by a solar eclipse, and then by another lunar eclipse, in the course of 33 days or thereabouts. We *do* find instances (as anyone can see by looking through a few successive almanacs) of a solar eclipse followed by a lunar eclipse, and then by another solar one within such an interval; but never of the other succession. The fact is, the 'Nautical Almanac,' from which all other almanacs take their astronomical facts, pays no attention to a certain order of lunar eclipses, to which, in the case considered, the first and last of a set of three eclipses must necessarily belong. It will easily be seen that if the middle eclipse of a set of three is a solar one, it will be very considerable, the orbit of the Moon being presented as at I., V., or IX., fig. 98. But the two lunar eclipses —one preceding and the other following the solar one—will be very slight affairs, for they will happen when the orbit is barely contracted enough (in aspect as seen from the sun) for an eclipse to occur at all. As a matter of fact, they are of such a nature that though a portion of the Sun is hidden from the Moon the whole of the Sun is not hidden from any part of the Moon's illuminated hemisphere. They correspond to partial eclipses of the Sun; but though a partial eclipse of the Sun is a noteworthy phenomenon to terrestrial observers, and therefore finds a place in the 'Nautical Almanac,' one of these corresponding lunar eclipses (differing altogether from partial lunar eclipses properly so called *) is a very different matter, and can scarcely be recognised at all by the terrestrial observer. Delicate photometric appliances would doubtless show that full sunlight was not shining on parts of the Moon at such a season, but to ordinary observation no trace of the deficiency of light is discernible. No notice is taken, therefore, of these eclipses in the 'Nautical Almanac,' which deals (very properly of course) only with phenomena that can be observed.

It will be seen then that under the circumstances considered there would be three eclipses or one during the eclipse-season, according as full Moon or new Moon occurred at the time when the Moon's orbit was presented as at I., V., or IX., fig 98.

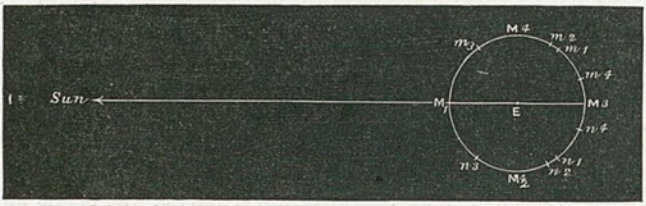
The case may be illustrated as in fig. 104; only the reader must remember that the just proportions of the orbits and

---

* In what is properly called a partial lunar eclipse, there is a part of the Moon from which the whole disc of the Sun is concealed (setting aside the refraction of the solar rays by the Earth's atmosphere); but in the eclipses considered, which I have ventured to designate *penumbral lunar eclipses*, every part of the moon is illuminated by direct sunlight, though not by the whole solar disc. I believe I may claim to have been the first to calculate a penumbral lunar eclipse. The details are given in the *Monthly Notices of the Royal Astronomical Society* for 1867–1868 (vol. xxviii.), the eclipse occurring on the night of September 2 in that year. The theory of eclipses cannot be considered complete without a consideration of these hitherto neglected penumbral eclipses. To lunarians, if such there be, the Sun must appear really eclipsed—though but partially—at such epochs; and in fact, as stated in the text, these eclipses correspond exactly to partial solar eclipses (that is, not to eclipses which, though really total, are partially seen at certain stations, but to solar eclipses which are partial wherever seen).

distances cannot be indicated in a single diagram. If when the Moon's orbit is presented as at I., V., or IX., fig. 98, the Moon is at $M_1$ (fig. 104) there will be a central eclipse of the Sun; but when the Moon has passed on to $M_3$, her orbit, as seen from the Sun, will be so far opened out that *no part of the Moon will be concealed from all parts of the Sun.* Hence there will only be a penumbral lunar eclipse, of which no notice will be taken in almanacs. And the like must have been the case as respects the Moon's position when at $M_3$ *before* the central solar eclipse. On the other hand, if the Moon is at $M_3$ when her orbit is presented as at I., V., or IX., fig. 98, there will be a total eclipse of the Moon. And, further, when the Moon was at $M_1$ before this eclipse, and when she is at $M_1$ after this eclipse, the Sun must be partially eclipsed, *though no part of the Earth will be concealed from all parts of the Sun.*

FIG. 104.



The same holds when the Moon is *near to* $M_1$ or $M_3$ in the middle of the 'eclipse-season.'

But when the Moon is at or near $M_4$ or $M_2$, at the time when her orbit is presented edgewise towards the Sun, only two eclipses can take place. Suppose, for instance, she is at $M_4$, then carrying her onwards, we see that she must eclipse the Sun when at $M_1$ but that she cannot be herself eclipsed when she gets to $M_3$, for then three quarters of a lunar month will have elapsed since the middle of this eclipse-season, and an eclipse-season cannot last a month and a half under any circumstances. Carrying the Moon backwards from $M_4$, we see that she must have been eclipsed when at $M_3$, but cannot have eclipsed the Sun when at $M_1$. Hence there occur two eclipses, one solar and the other lunar, in this eclipse-season. And clearly the reasoning is precisely the same when the Moon is at $M_2$ in the middle of an eclipse-season.

The same holds when the Moon is *near to* $M_2$ or $M_4$ in the middle of an eclipse-season.

In these cases the two eclipses would not be so important generally as an eclipse occurring in the middle of an eclipse-season. If the Moon were exactly at $M_2$ or $M_4$ the solar eclipse would be central and the lunar eclipse partial;[*] but even the

[*] It is easy to extend the calculation made above, as to the average duration of eclipse-seasons, to determine how long the opening of the Moon's orbit, as seen from the Sun, continues small enough for a particular order of eclipses to take place. In the following inquiry it will be understood that only mean values are considered. Farther on, the effect of the eccentricity of the lunar and terrestrial orbits, and other circumstances, in modifying the limits of eclipse-seasons, will be dealt with; but only in a general way.

For a central solar eclipse to take place—that is, either an annular or total solar eclipse—it is necessary that the opening of the Moon's orbit, as seen from the Sun's *centre*, should be so small when new Moon occurs as to intersect the Earth's disc. For clearly in this case a line from the Sun's centre to the Moon's, at the time of new Moon, will fall on the Earth's disc, and where this line meets the Earth there must needs be a central solar eclipse; whereas, if the line joining the centres of the Sun and Moon did not meet the Earth there could be no central eclipse. We have, then, the greatest opening of the Moon's orbit as before, about $\frac{9}{100}$ths of the long diameter of the orbit, while the Earth's diameter is, as we know, but about a sixtieth part of this long diameter (or $\frac{5}{27}$ths of the greatest opening). So that the time required to open the orbit to this extent bears to 86·7 days (half of 173·3, that is) the ratio which an angle whose sine is $\frac{5}{27}$ bears to a right angle. But the sine of an angle of $10\frac{2}{3}^\circ$ is about $\frac{5}{27}$, and such an angle is about $\frac{2}{17}$ths of a right angle. Hence the required time is about $10\frac{1}{5}$ days; and, counting it on either side of the middle of the eclipse-season, we get $20\frac{2}{5}$ days as the duration of what may be called the central-solar-eclipse-season.

Now the relation here considered has no counterpart among recognised orders of lunar eclipses, since the fact that a line through the centres of the Sun and Moon at the time of lunar eclipse crossed the Earth would correspond only to the fact that the Sun's centre was concealed from the centre of the Moon's disc, a relation not requiring special consideration.

solar eclipse, though central, would be less important (regarding the whole Earth, than a solar eclipse occurring during the

Let us, however, inquire what are the average limits of the two orders of lunar eclipses which are dealt with by astronomers—viz., total and partial lunar eclipses.

Total eclipses of the Moon are determined by the consideration that all parts of the Sun are concealed from the whole of the Moon's disc (always setting on one side the effects of atmospheric refraction), and, therefore, the opening of the lunar orbit must be less than the Earth's apparent diameter, as seen from the Sun, by the sum of the two quantities, which, in the inquiry in the text, were added to that diameter. We must, therefore, diminish the Earth's apparent diameter as seen from the Sun by about one-half; so that, instead of getting the angle whose sine is about $\frac{5}{27}$, as before, we get the angle whose sine is but one-half of this, or an angle of about $5\frac{1}{3}$ degrees; and the corresponding proportion of 86·7 days (or about $5\frac{1}{8}$ days) is one-half of the total-lunar-eclipse-season, whose full length is therefore about $10\frac{1}{4}$ days.

Lastly, for the occurrence of lunar eclipses generally, we must have the opening of the lunar orbit such that from some part of the Moon's disc the whole of the Sun is concealed, and therefore, on the assumption hitherto made (which is not far from the truth), that the average value of the Sun's apparent diameter is equal to the Moon's, we need neither increase nor diminish the Earth's apparent diameter, as seen from the Sun. We therefore get the same results as when we were considering central solar eclipses—namely, $10\frac{1}{5}$ days for one-half of the lunar-eclipse-season, whose full average length is therefore about $20\frac{2}{5}$ days.

Now these results enable us to determine the general conditions under which various orders of eclipses will occur during the eclipse-season.

That there may be a central solar eclipse, the middle of the eclipse-season must occur when the Moon is somewhere within the arc $m_1\,\mathrm{M}_1\,n_1$, such that $m_1\,\mathrm{M}_1$ and $n_1\,\mathrm{M}_1$ are each equal to the space traversed by the Moon in about $10\frac{1}{5}$ days. That there may be a total lunar eclipse, the middle of the eclipse-season must occur when the Moon is somewhere within the arc $n_2\,\mathrm{M}_3\,m_2$, such that $n_2\,\mathrm{M}_3$ and $\mathrm{M}_3\,m_2$ are each equal to the space traversed by the Moon in about $5\frac{1}{8}$ days. That a partial lunar eclipse may occur, the middle of the eclipse-season must take place when the Moon is somewhere on the arc $n_3\,\mathrm{M}_3\,m_3$, such that $n_3\,\mathrm{M}_3$ and $\mathrm{M}_3\,m_3$ are each equal to the space traversed by the Moon in about $10\frac{1}{5}$ days. And, lastly, it will be gathered from the inquiry in the text that for two solar eclipses to occur during the eclipse-season, the middle of this season must take place when the Moon is somewhere on middle of the eclipse-season. It is easy to see in what respect it would be less important. When a solar eclipse occurs in the middle of the eclipse-season, the Moon's shadow traverses the centre of the Earth's disc as seen from the Sun. It therefore has a longer course on the Earth, and if total is rendered more remarkable by traversing that part of the Earth which is nearest to the Moon at the time. It is worthy of notice, however, that an annular eclipse, if its importance is measured

the arc $n_4$ $M_3$ $m_4$, such that $n_4$ $M_3$ and $M_3$ $m_4$ are each equal to the space traversed by the Moon in the excess of half an eclipse-season over half a lunation.

Now the places of the points $m_1$ $m_2$, &c., will vary slightly, according to the length of the lunar month, the position of the Moon at new or full with reference to her perigee and apogee, and so on; and in particular it is to be noted that the limiting positions of $m_1$ and $m_2$, as of $n_1$ and $n_2$, are such that $m_1$ may be between $m_2$ and $M_4$, $n_1$ between $n_2$ and $M_2$. But taking them as at present placed, and proceeding round the orbit from $M_1$ towards $M_2$, &c., we have the following relations:—

If at the middle of the eclipse-season the Moon is between $M_1$ and $n_3$, there will be one central solar eclipse during the season; if between $n_3$ and $n_2$, there will be one central solar eclipse and one partial lunar eclipse; if between $n_2$ and $n_1$, there will be one central solar eclipse and one total lunar eclipse; if between $n_1$ and $n_4$, there will be one partial solar eclipse and one total lunar one; if between $n_4$ and $m_4$, there will be two partial solar eclipses and one total lunar one; if between $m_4$ and $m_1$, there will be one partial solar eclipse and one total lunar one; if between $m_1$ and $m_2$, there will be one central solar eclipse and one total lunar one; if between $m_2$ and $m_3$ there will be one central solar eclipse and one partial lunar one; and, lastly, if the Moon is between $m_3$ and $M_1$, there will be during the eclipse-season one central solar eclipse.

Thus there will be one solar eclipse if the Moon is on the arc $m_3$ $n_3$ at the middle of the eclipse-season, a solar and lunar eclipse if the Moon is on either arc $m_4$ $m_3$ or $n_3$ $n_4$, and two solar eclipses and one lunar one if the Moon is on the arc $n_4$ $m_4$. The dimensions of these arcs indicate the probability that an eclipse-season will include one, two, or three eclipses. Only when the Moon falls on either of the arcs $m_1$ $m_2$ and $n_1$ $n_2$ can there be a central solar eclipse and a total lunar one; this combination is, therefore, very infrequent. Still more infrequent is the occurrence of a partial solar and a partial lunar eclipse in the same eclipse-season; for this can only happen when $m_1$ is for the time between $m_2$ and $M_4$, $n_1$ between $n_2$ and $M_2$.

by the breadth of the ring when the Sun is centrally eclipsed, is affected in a contrary manner when the shadow of the Moon falls near the centre of the Earth's disc; for that point being nearest to the Moon, the Moon appears relatively larger there, and the annulus therefore relatively narrower. It is true this part of the Earth is nearest also to the Sun, but his apparent magnitude is little affected, whereas the Moon's (owing to her relative proximity) is appreciably enlarged.

We may thus sum up the general characteristics and relations of our eclipse-seasons, the note on the preceding paragraph supplying the details on which the results are founded:—

The most common of all orders of eclipse-seasons are those in which two eclipses take place. Of these one of course is solar, the other lunar, and most commonly the solar eclipse is central, the lunar one partial, but in a considerable proportion of cases the solar eclipse is partial and the lunar one total. Very seldom does a total lunar eclipse accompany a central solar one, and yet more seldom are both partial. Next in order of frequency to the seasons of two eclipses are the seasons of but one eclipse, always a central solar one. Lastly come the seasons in which there are three eclipses, which are always—in order—a partial solar eclipse, a total lunar eclipse, and again a partial solar eclipse.*

---

* From the preceding note it follows that the average frequency of the several orders of eclipses—omitting the case of two partial eclipses as of such infrequent occurrence—are fairly presented in the following table, in which the letters refer to fig. 104:—

| | FREQUENCY PROPORTIONAL TO | |
|---|---|---|
| | | Days of lunation. |
| Class I. Three eclipses, partial solar, total lunar, and partial solar . . . . | arc $m_4\,n_4$ . . | about $3\frac{1}{2}$ |
| Class II. One central solar eclipse . . . . | ,, $m_3\,n_3$ . . | ,, $9\frac{1}{2}$ |
| Class III. One solar and one lunar eclipse . . . | ,, $m_4\,m_3$ + arc $n_3\,n_4$ | ,, 17 |
| Subdivisions:— | | |
| Solar central, lunar partial | ,, $m_2\,m_3$ + ,, $n_2\,n_3$ | ,, $10\frac{1}{6}$ |
| Solar partial, lunar total . | ,, $m_1\,m_4$ + ,, $n_1\,n_4$ | ,, $5\frac{2}{3}$ |
| Solar central, lunar total . | ,, $m_1\,m_2$ + ,, $n_1\,n_2$ | ,, $1\frac{1}{6}$ |

Now, with regard to the succession of these eclipse-seasons, it needs only to be noted that three seasons in which there are three eclipses never occur in succession.

We can now easily determine the greatest and least number of eclipses which may occur in any single year. The average interval between successive eclipse-seasons is 173·3 days. Two such intervals amount together to 346·6 days, or fall short of a year by about 19 days. Hence there cannot be three eclipse-seasons in a year. For each eclipse-season lasts on the average 33 days. Now suppose an eclipse-season to begin with the beginning of a year of 366 days. The middle of the season occurs at about midday on January 17; the middle of the next eclipse-season 173·3 days later, or on the evening of July 8; and the middle of the third occurs yet 173·3 days later, or on December 29, early in the forenoon; so that nearly the whole of the remaining half belongs to the following year. Now this is clearly a favourable case for the occurrence of as many eclipses as possible during the year. If all three seasons could be of the class containing three eclipses, there would be eight eclipses in the year, because the second eclipse of the third season would occur in the middle of that season. This, however, can never happen. But there may be two seasons, each containing three eclipses, followed by a season containing two eclipses, only one of which can occur in the fragment of the eclipse-season falling within the same year. In this case there would be seven eclipses in the year. So also there would be seven if in the first season there were three, in the second two, and in the third three, for then the fragment of the third falling within the year, being rather more than one-half, would comprise two eclipses. So also if the three successive seasons comprise severally two, three, and three eclipses.

The same would clearly happen if the year closed with the close of an eclipse-season.

There may then be as many as seven eclipses in a year, in which case at least four eclipses will be solar, and at least three of these partial, while of the lunar eclipses two at least will be total.

As regards the least possible number of eclipses, it is obvious that, as there must be two eclipse-seasons in the year, and at least one eclipse in each, we cannot have less than two eclipses in the course of a year. In this case each eclipse is solar and central.

As regards intermediate cases, we need make no special inquiry. Many combinations are possible. The most common case is that in which there are four eclipses—two solar and two lunar. Further, it may be noticed that, whatever the number of eclipses, from two to seven inclusive, there must always be two solar eclipses at least in each year.

And now we may turn from the particular mode of considering eclipses, which we have thus far followed. There is another by which we might have arrived at similar results almost as readily. We might, instead of viewing the Earth and Moon in imagination from the Sun, have traced the course of the Sun and Moon around the heavens. Both methods of dealing with eclipses are employed by astronomers, the method used in the preceding pages corresponding to what is termed the method of projection, the other to the method of direct calculation from the celestial ordinates of the Sun and Moon. For our present purpose, however, one method is all that need be considered.*

And now, before closing this essay, I will consider in the usual manner the nature of the Moon's shadow-cone in solar eclipses, and of the Earth's shadow-cone in lunar eclipses.

Eclipses of both sorts may be regarded as illustrated together in fig. 105. Here E is the Earth, and the Moon is shown in two

---

* In the *Popular Science Review* for July 1868 I have exhibited t line of reasoning by which the results deduced above can be obtained by considering the apparent motions of the Moon and Sun around the celestial vault. By the artifice of regarding these motions as taking place on a sphere which can be viewed from without, and shifted or rotated so as to illustrate the various relations dealt with, the whole subject may be very conveniently discussed. The student of astronomy does well to examine all such questions by as many independent methods as possible; but in the present treatise there is not space for a complete investigation of the theory of eclipses on the second plan.

places,—at M, directly between the Earth and Sun, and at the point opposite M, in the heart of the Earth's shadow-cone. The true geometrical shadows of the Earth and Moon are shown black, the true geometrical penumbræ are shaded. It must be understood, however, that the vertical dimensions have had to be exaggerated; the angle at C ought properly to contain but about half a degree. Such an angle could not be conveniently employed in illustrating our subject.

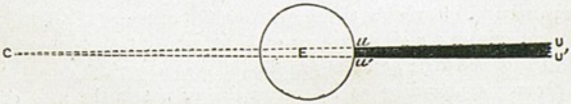
FIG. 105.

C *e* and C *e′* produced touch the Sun's globe; so also do the boundaries of the Moon's black shadow. The boundaries m *e* and *m e′* touch the Sun's globe *after crossing*; so also do the boundaries of the Moon's penumbra.

The distance E C is variable, being as great as 870,300 miles when the Earth is in aphelion, and as small as 843,300 miles when the Earth is in perihelion. The Moon's orbit round the Earth has a mean radius of 238,770 miles. Thus the Earth's shadow extends about three and a half times as far from the Earth as the Moon's orbit.

FIG. 106.

FIG. 107.



Illustrating the Theory of Eclipses.

The end of the Moon's shadow is represented on a larger scale (and without the penumbra) in figs. 106 and 107. In fig. 106 the shadow's extreme point C does not reach the Earth; in fig 107 it passes far beyond the Earth. The two figures represent the extreme possible range of the shadow-point either way. In fig. 106 there is shown beyond C a shaded anti-cone. From any point within this the Sun will be annularly eclipsed. Thus the section at $a\,a'$ includes that part of the Earth whence at the moment an annular eclipse of the Sun is visible.* On the other hand, the section $u\,u'$ in fig. 107 includes the part of the Earth whence the Sun is totally eclipsed. It is important to notice that the greatest possible width of $u\,u'$ is about 173 miles.†

Now in fig. 105 the points M $m$ $m'$ M′ may be supposed to lie on the Moon's orbit seen *in plano*. If we suppose this orbit not to lie in the plane of the paper, but tilted at an angle of 5° (or rather to an angle as much larger than 5° as the shadow-angle at C is increased beyond its true value of half a minute), then by conceiving the whole figure turned about C M, until the Moon's orbit is seen sideways, this orbit, according to the direction in which the tilt existed, would exhibit a shape resembling one of those shown in fig. 98 (only more open on account of the exaggeration of the tilt. And, further, if we could watch from such a standpoint—that is (obviously) a standpoint towards which or from which the Earth was travelling—during a period of about 346 days, we should see the Moon's orbit passing in

---

* It is obvious that the Sun will seem to be annularly eclipsed from any point within this *anti-cone*; for lines drawn from any such point to the circle on the Moon in which the shadow-cone begins will form a cone (right or oblique) which, *beyond* the Moon, will be wholly within the extension of the shadow-cone's geometrical surface. Hence a portion of the Sun's globe must lie outside and around this inner cone. This portion will be visible, therefore, from the vertex of this inner cone (the point within C $a$ $a'$) as a ring of light, whose boundaries will be concentric or eccentric according as the inner cone is right or oblique—in other words, according as its vertex lies or not on the axis of the shadow-cone produced.

† The extreme limits of central solar eclipses result when, first, the Sun's diameter has its greatest value, 32′ 36″·4, and the Moon's its least, 29′ 21″·9, in which case a ring of light 1′ 37″·2 wide remains; and, secondly, when the Sun's diameter has its least and the Moon's its greatest value, in which case the Moon's disc overlaps the Sun's by 59″·6, and the Sun continues for several minutes totally eclipsed.

orderly succession though all such phases as are exhibited in fig. 98.

Now, clearly, since for a lunar eclipse to occur the Moon must enter the cone $m' c m'$ opposite $m$, while for the occurrence of a solar eclipse the Moon must enter this cone opposite $m'$, lunar eclipses must on the whole be less numerous than total ones, for the cone is appreciably narrower opposite $m$ than opposite $m'$. It is, however, also obvious, that when the Moon is in the Earth's shadow she is eclipsed as viewed from a whole hemisphere of the Earth, whereas when the Moon casts a shadow on the Earth the Sun is only eclipsed as viewed from parts of the Earth which are traversed by that shadow. The extent of such regions falls very far short* of half the Earth's surface. Hence solar eclipses are less frequent at any given station than lunar ones.

But it is worthy of notice that, if penumbral lunar eclipses are included, more lunar eclipses than solar ones occur in any long period of time. For, clearly, the section of the penumbral cone opposite $m$ is greater than that of the cone $m c m'$ opposite $m'$, since both cones enclose the Sun, but the vertex of the former is nearer to the Sun than that of the latter, and therefore the vertical angle of the former cone is the greater.

It is convenient to notice in conclusion, that in every period of 21,600 lunations there are on the average 4,072 solar eclipses and 2,614 lunar eclipses, not counting penumbral ones. If penumbral lunar eclipses are included, the number rises to 4,231. In all there are (on the average) 6,686 lunar and solar eclipses in the course of every 21,600 lunations, the total rising to 8,303 when penumbral lunar eclipses are added.

---

* The extent of the region actually in shadow at a given moment will vary in different eclipses, and at different hours during the same eclipse. It will be least of all when the Moon's real shadow has its greatest possible extent (*i. e.* when the Sun is in aphelion, the Moon in perihelion, and both as near the zenith as they can be compatibly with those conditions). At such a time the edge of the penumbra forms a circle (approximately) having a radius equal to the Moon's diameter diminished by about 86½ miles, or a radius of about 2,078 miles. The extent of the Earth's surface then in shadow is easily shown to be about one 37th part of the whole surface of the Earth.

## TABLE I.

*Principal Elements of the Sun.*

| | |
|---|---|
| Equatorial horizontal parallax at mean distance from the Earth | 8″·9 |
| Diameter in miles | 850,000 |
| Diameter (Earth's as 1) | 108 |
| Volume (Earth's as 1) | 1,260,000 |
| Mass (Earth's as 1) | 318,000 |
| Density (Earth's as 1) | 0·250 |
| Density (Water's as 1) | 1·42 |
| Surface (Earth's as 1) | 11,650 |
| Gravity at surface (Earth's as 1) | 27·1 |
| Fall of bodies in feet in one second | 436·3 |
| Greatest apparent diameter viewed from the Earth | 32′ 36″·41 |
| Mean „ „ | 32′ 3″·64 |
| Least „ „ | 31′ 31″·79 |
| Linear value of 1″ at mean distance from Earth | 450 miles. |

*The elements of the Sun's rotation are given in Chapter IV., pp.* 210, 211.

## TABLE II.

*For determining the effect of changes in the value of the Sun's equatorial horizontal parallax (at his mean distance) on the estimated mean distance.*

| Parallax (in seconds) | Mean Distance (in miles) | Difference for 0″·01 (in miles) |
|---|---|---|
| 8·0 | 102,173,020 | 126,142 |
| 8·1 | 100,911,600 | 123,061 |
| 8·2 | 99,680,990 | 120,097 |
| 8·3 | 98,480,020 | 117,240 |
| 8·4 | 97,307,620 | 114,478 |
| 8·5 | 96,162,840 | 111,820 |
| 8·6 | 95,044,640 | 109,244 |
| 8·7 | 93,952,200 | 106,766 |
| 8·8 | 92.884,540 | 104,362 |
| 8·9 | 91,840,920 | 102,045 |
| 9·0 | 90,820,470 | 99,805 |
| 9·1 | 89,822,420 | 97,632 |
| 9·2 | 88,846,100 | 95,532 |
| 9·3 | 87,890,780 | 93,504 |
| 9·4 | 86 955,740 | 91,530 |
| 9·5 | 86,040,440 | 89,624 |
| 9·6 | 85,144,200 | 87,780 |
| 9·7 | 84,266,400 | 85,986 |
| 9·8 | 83,406,540 | 84,248 |
| 9·9 | 82,564,060 | 82,564 |
| 10·0 | 81,738,420 | |

*Spottiswoode & Co., Printers, New-street Square and Parliament Street.*