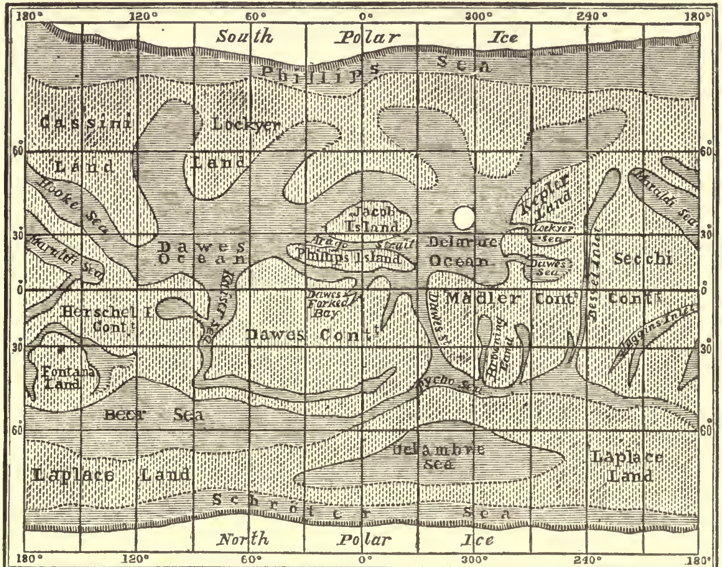

CHART OF MARS ON MERCATOR'S PROJECTION.

THE ORBS AROUND US:

A SERIES OF FAMILIAR ESSAYS ON

THE MOON AND PLANETS, METEORS AND COMETS, THE

SUN AND COLOURED PAIRS OF SUNS.

BY

RICHARD A. PROCTOR, B.A. (CAMB.)

Author of 'The Sun,' 'Other Worlds than Ours,' 'Saturn and its System,'
'Essays on Astronomy,' 'Sun-Views of the Earth,'
'A New Star Atlas,' &c.

'Not to this evanescent speck of earth
Poorly confined—the radiant tracts on high
Are our exalted range; intent to gaze
Creation through, and from that full complex
Of never-ending wonders, to conceive
Of the Sole Being right.' THOMSON.

SECOND EDITION.

LONDON:
LONGMANS, GREEN, AND CO.
1875.

FROM THE

PREFACE TO THE FIRST EDITION.

THIS VOLUME contains matter which, if space had permitted, I should have included in 'Other Worlds than Ours,' partly in the way of introduction or explanation, and partly to complete the information contained in the several chapters. It will be understood that to make this work complete in itself, I have admitted certain passages which contain information already given in 'Other Worlds than Ours;' but such passages amount in all but to some ten or twelve pages; the rest of the work is strictly supplementary to that volume.

The paper on the 'Gamut of Light' presents the subject of spectroscopic analysis in a way which I have found effective with those readers who are not desirous of discussing the details of this mode of research. In the next two papers on 'Other Habitable Worlds' and 'Other Inhabited Worlds,' a sketch is given on the subject of the Plurality of Worlds. The fourth paper relates to the application of the Rosse Telescope to determine the heat of the lunar surface. Then follow papers on Venus, Mars, and Jupiter; two essays on Meteors, one relating to the past history and the other to the present condition of meteoric researches; a paper on Tyndall's Theory of Comets (a subject deserving close examination), and another on the general phenomena of comets and comets' tails. The three papers on the Solar Corona might have been added, almost as they stand, to my chapter on 'What we learn from the Sun,' in 'Other Worlds,' but for the requirements of space. Lastly there is a paper on the Colours of the Double Stars, a subject which the same cause compelled me to leave almost untouched in that work.

I have to return my thanks to the editors and publishers of the magazines from which these essays have been taken, for permission to reproduce them.

R. A. P.

PREFACE

TO

THE SECOND EDITION.

ALL the Essays in the volume have been carefully revised, and some drawings illustrating Solar Outbursts have been added. In other respects the volume remains almost unaltered.

R. A. P.

LONDON: *October* 1874.

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THE

ORBS AROUND US.

INTRODUCTION.

THE GAMUT OF LIGHT.

A FEW YEARS AGO I had occasion, during the course of a lecture on astronomy, to explain the nature of spectroscopic analysis to a mixed audience. I had gone through the usual statement of the laws on which this mode of research depends; but I felt convinced that the explanation had been insufficient. That sense of sympathy which enables every lecturer to tell whether his hearers are following him assured me that the audience, with all willingness to be convinced, had not grasped the essential principle on which spectroscopic analysis depends. It will be understood that my object was not to give a complete account of the new analysis; but it was essential that the convincing nature of the evidence which the analysis affords, should be brought clearly before the audience. I knew that before long I should have to tell them certain very startling facts, and that, unless they had some solid ground for believing these facts, the whole lecture would resolve itself into a mere series of statements to be accepted on trust, whereas the end and aim of lecturing is, or should be, to demonstrate.

In this strait, it occurred to me to re-state, but in a new form, the evidence on which the application of the spectroscope depends. It is no new conception to compare light and sound together, or to illustrate the analysis of light by a reference to the combinations of musical notes. In fact, I had long before employed this method of illustrating the subject. But it was a new thing—to me at least—to test the efficiency of this method of explanation by bringing it before an audience immediately after the ordinary explanation had failed. It was, therefore, with no small interest and satisfaction that I found the audience grasping at once the points I was so anxious to enforce, and becoming eager to hear how the mode of analysis they now trusted in, had been applied by physicists to astronomical problems.

This experience, and the fact that day after day new facts are being revealed by the spectroscope, induce me to think that an explanation of the powers of the instrument on the plan referred to may be serviceable to many who daily hear the work of the spectroscope mentioned, and have perhaps often seen its action scientifically explained, but have yet no clear and definite ideas of the nature of the evidence it supplies, or of the reasons on which men of science base their acceptance of such evidence.

Everyone is familiar with the gamut of *sound*. It is also easy to conceive the orderly succession of notes, separated by definite tone-intervals, replaced by an arrangement in which the difference between successive notes should be imperceptible. We can imagine, for instance, that in place of the white and black keys between two C's of a piano there might be an indefinite number of keys, so that, supposing these swept from C to C, every possible gradation of sound between those notes would become audible. We shall call this arrangement a *continuous gamut*.

Now it is found that when the light of an incandescent solid or fluid body is dispersed by a prism, it forms a rainbow-tinted streak, in which all orders of colour from red, through orange, yellow, green, blue, and indigo, to violet, are present, without break or interruption. So that we can compare this rainbow-tinted streak (or *spectrum*, as it is called) with the stream of sound in which all orders of tone, from one C to the next above it, are heard without break or interruption. We need not concern ourselves about the scientific exactness of the illustration if it suffices for our purpose.[1]

[1] It is the attempt to secure at the same time clearness of illustration and strict scientific exactness, which causes so many explanations to perplex instead of edifying. Scientific exactness can come afterwards if the beginner is encouraged to pursue the devious tracks which lead to it, by obtaining a clear view of what he will gain by the labour.

And now, before proceeding, let us take an example of the application of this first and fundamental fact. With special exceptions, into the nature of which we need not now enter, it may be said that all incandescent solid and fluid bodies show this continuous rainbow-tinted streak, and that only the light from such bodies will exhibit a continuous streak of colour from deepest red to deepest violet. This is an experimental fact. Now suppose there is some self-luminous body that we cannot attain to, and we wish to tell what its nature may be. If we find that its light, when dispersed by the prism, shows a continuous rainbow-tinted streak, we can conclude as surely that it is an incandescent solid or fluid, as we could tell that our imagined set of keys from C to C had been swept from end to end, if we heard the whole succession of sounds, even though the instrument were out of sight. Always supposing a certain keenness of perception on the part of the auditor, it would make no difference to him whether the musical instrument were close by, or in another room, or even in another house; so long as he heard the whole succession of sounds he would know that the whole series of keys had been struck. And just as certainly the physicist can tell that light comes from an incandescent solid or liquid because the whole series of colours is present in the spectrum without break or interruption, even though the source of light be millions of miles away. As our imaginary auditor would be certain so long as he could hear the continuous succession of sounds, so the physicist, using the spectroscope, is certain as long as he can see the continuous spectrum.

Let us now consider another case. Suppose certain notes only of those forming our continuous gamut of sound were struck in quick succession. An auditor would be able to tell what those notes were. He would recognise them as a definite set of notes. If the same series were struck simultaneously, either by the fingers of a musician, or by some instrument constructed for the purpose, the auditor would be able, if he were a practised musician, to tell the exact set of notes thus sounding simultaneously. But it will be convenient for the purposes of illustration, to consider the case of a succession of definite notes; [1] because

[1] Recently, attention was directed, in the *Quarterly Journal of Science*, to the analogy between sound and light. It appears to us that although such an analogy undoubtedly exists, an attempt was made to push the analogy farther than the evidence warrants. In the spectrum we have a succession of colours precisely as in the gamut we have a succession of notes; but the succession in one case depends on position, in the other on time. The colours of the spectrum are seen to succeed each other as the eye passes from the red end to the violet end; the notes of the gamut succeed each other as they fall one after another on the ear. Hence a chord in music corresponds to a spectrum compounded of several prismatic lines. So far the analogy may be followed. But we cannot reasonably extend the analogy so far as to assert that there is anything in the theory of colours corresponding to the effects produced by concordant or discordant sounds. If three successive notes of the gamut are sounded together we have an unpleasing sound; but if red, orange, and yellow lights are commingled, the resulting light is not unpleasing—no eye can, in fact, distinguish it from pure orange light. And similarly of other combinations. Three or more colours corresponding (so far as the waves of light are concerned) to a pleasing musical chord, produce together a colour which is not a whit more pleasant than the colour produced by mixing three or more colours corresponding to a discordant combination of sounds. Who would pretend to say, for

everyone can understand how such a succession could be recognised, whereas the musician's power to recognise the component notes of a chord is less common.

Now it is found that when the light of a glowing gas is examined by the spectroscope, it is resolved into a definite number of coloured bands or lines. Some gases show only one or two lines; others many; others, again, show broadish bands, with dark spaces between them. But we may assert in a general way of the spectra of glowing gases that they are discontinuous under ordinary circumstances. Further, setting aside as before certain exceptions, the consideration of which belongs to a more advanced stage of the science, we may say that each gas has its own family of lines, which always make their appearance when the light of that

instance, that the coloured rings seen when a lens of glass is pressed against a glass plane, or the colours seen in a bubble, are less pleasing to the eye than the colours of the prismatic spectrum; yet the latter are pure, while the analogue of the former colours in sound would be a series of noises as painful to the ear as saw-filing.

It may be questioned, indeed, whether there is such a thing in nature as an ugly colour, that is a colour which, apart from some association of ideas, is painful to the healthy eye; whereas only certain combinations of sound are pleasing to the tutored ear, and many are essentially painful even to those who have no musical taste.

On the other hand, it is worthy of notice that whereas certain combinations of pure colour by juxtaposition are essentially unpleasant, it may be questioned whether any sequence of simple notes can be so regarded. It would, however, require more space than is here at our disposal to discuss this question, since each of the parts into which it divides itself are associated with a subject of some difficulty. It would be no easy task to determine either on the one hand the essential principles on which the pleasing or unpleasing effects of juxtaposed colours depend (the laws of complementary colours being by no means sufficient for the purpose), or on the other, the principles which render certain sequences of sound more or less pleasing to us.

gas is examined. So that we can compare the spectra of gases with the succession of certain definite notes just considered.

The reader will, therefore, understand the confidence with which the chemist or the astronomer who recognises one of these discontinuous spectra, infers that the source of light is such and such a gas glowing with intensity of heat. Precisely as a musician would have no hesitation in pronouncing that such and such a succession of notes had been struck, even though the performer were in a distant room, so it makes no difference to the physicist whether the source of light is far off or near. So long as he sees in the spectrum a certain succession of coloured lines, he knows certainly that the particular gas which has the power of showing those lines as its spectrum, is the source of light. We can even compare the method which a spectroscopist adopts to assure himself that such and such lines (and not others nearly in the same place) are seen in the spectrum, with those which a musician might employ to assure himself that such and such notes (and not others nearly resembling them) are successively struck by a distant performer. For we can imagine our musician to have before him a piano capable of giving the same continuous succession of notes between two C's as we have conceived already. If, then, this musician were in doubt whether a certain succession of notes had been struck, he could test the matter by striking that succession himself. He could repeat this process until he struck the exact succession he had heard; and then he would no longer have any doubt as to the nature of that succession. In like manner the spectroscopist who is doubtful whether a certain set of coloured lines really corresponds with a set belonging to a known gas, can cause his spectroscope to show both sets side by side; then if the lines of the two sets agree exactly, line for line (so that each line of one set is in the same line with one of the other set), he knows that the source of light really is the glowing gas he had supposed it to be. If there is no such agreement, he can try other spectra until he finds one which corresponds exactly.

Lastly, there remains to be considered the case where all the notes of the continuous gamut, save a certain definite set of notes, are struck in succession. Suppose the performer to hold down a certain chord while he sweeps the notes from end to end. There would then be heard a succession of notes, with here and there certain breaks. A practised ear could tell as readily what notes corresponded to those breaks as though the notes themselves were separately struck. Or conceive that certain instruments were formed, by each of which a special chord could be held down. Then if any one of these instruments were employed while the notes were swept from end to end as before, the musician could recognise the absence of certain notes, which might (according to the nature of the instrument) be any in number, from one or two to many.

Now it has been found that when an incandescent solid or fluid body is shining through a gaseous or vaporous envelope, the spectrum of the body's light is a rainbow-tinted streak, across which there lie certain dark lines. According to the nature of the gaseous envelope these lines may be more or less numerous. Some gases cause, not lines, but bands, to appear in the spectrum of an incandescent body shining through them. But speaking generally, we may say that the spectrum of such a body is a rainbow-tinted streak crossed by dark lines. Further, setting aside as before certain exceptions, we may say that each gas has the power of producing its own family of dark lines (these dark lines having precisely the same position as the bright lines seen when the spectrum of the same gas is separately examined). So that we may compare the spectrum of an incandescent solid or fluid shining through a gaseous or vaporous envelope, with the case in which the continuous gamut of sound is crossed (as it were) by a silent chord.

Hence, in this case, as in the two former, the reader will understand the confidence with which the spectroscopist who recognises certain dark lines across the rainbow-tinted streak forming the continuous spectrum, is able to pronounce that the source of light is an incandescent substance shining through certain gases. A musician who noticed certain gaps in the continuous gamut of sound, could not feel more certain as to the particular notes which were held down (or silent) than the spectroscopist is that the source of light is surrounded by an envelope of such and such a constitution. Distance makes no more difference in the one case than in the other, provided always that in one case each sound is distinctly audible, and that in the other each portion of the spectrum is distinctly visible.

In this instance, also, as in the former, it is possible to illustrate the method by which the spectroscopist assures himself that the dark lines in the spectrum correspond to the bright lines of any gas. We can conceive that our musician, recognising the absence of certain notes, and not certain of the power of the unaided ear to determine what those notes were, might try the experiment of sounding certain notes to see if they corresponded to those of the silent chord. Now precisely in this way the spectroscopist brings a spectrum of bright lines into comparison with the gaps of a spectrum crossed by dark lines; and only when he finds that the bright lines and the dark lines correspond exactly, does he conclude that the particular substance which produces the dark lines exists in the atmosphere surrounding the source of that light which he is analysing.

We are now upon the most important part of our subject, so far at least as the application of spectroscopic analysis to the celestial objects is concerned. For although no inconsiderable proportion of the celestial objects show spectra of bright lines or bright bands, yet the number of different bright-line spectra hitherto recognised is singularly small. All the gaseous nebulæ, for example, show the same set of bright lines. Among them are some, indeed, which show only one line, whereas others show three, or even four. But the difference, such as it is, doubtless depends only on the relative brightness of these objects, for where only one line is shown, that line always corresponds with the brightest line of the set of three where that number can be seen. But the number of dark-line spectra seems almost illimitable. Every star has its own peculiar spectrum, as distinct from that of any other star as the spectrum of hydrogen from that of iron. We may arrange the star-spectra into four orders, as Father Secchi has done, or we may arrange them according to other modes of classification; but it is resemblance, not identity, which determines the arrangement. Probably, among all the millions on millions of stars in the sidereal system there are not two which have spectra exactly alike—or, in other words, there are probably not two suns in the universe which are in precisely the same physical condition.

It is to be noted, further, that those nebulæ which have stellar spectra exhibit the same variety in the number, arrangement, and intensity of the dark lines or bands. The planets also, though we examine them under less favourable conditions, show the absorption-bands peculiar to their several atmospheres, and amongst these, too, we find a similar variety. Nay, even one and the same object may present a varying spectrum. Our readers are probably familiar with the change which came over the spectrum of the star

T Coronæ when this orb suddenly blazed out in May, 1866, after shining for hundreds of years (at least) as a tenth-magnitude star. But there is an instance nearer at hand, in the case of our own sun. Different parts of his surface give different spectra. The spots have not the same spectrum as the bright parts of the disc; the ordinarily bright parts have not the same spectrum as the exceptionally bright parts, called the faculæ. Then the spectrum of a solar spot is variable, actually changing under the eye of the observer.

It is this wonderful variety in the spectra of the celestial objects which renders it so important that the student of astronomy should recognise the absolute certainty which characterises the results of spectroscopic research. It seems so amazing that objects lying at distances as enormous as those which separate us from the nearest fixed star, should yet admit of being analysed, that the student can scarcely believe that there is not some flaw in the reasoning, or some over-confident acceptance of theories which do not admit of proof. When he feels that, wonderful as the new mode of analysis is, its teachings differ only in degree, and not in kind, from the information conveyed by the ear, he grasps at once the full significance and value of the new method. And this is, indeed, a true statement of the case. The great range of the spectroscope seems to make a comparison with any instances of the analysis of audible sounds altogether out of the question. At the outside, sounds can be distinguished at a distance of but a few miles, whereas the new analysis is applied to tell us what is the constitution of orbs whose distance is so enormous that a million of miles is an altogether insufficient unit for their measurement. But when it is remembered that, after all, the same distinction applies to the range of vision as compared with that of hearing, this difficulty vanishes. If we can tell that in a musical band a few hundred yards off there are performers on the bassoon, the ophicleide, the big drum, and so forth, it is because the band falls within the limits to which the hearing extends. And therefore, since our eyesight ranges over the countless millions of miles which separate us from the stars, it need not be regarded as an incredible thing that an optical instrument should be able to analyse light from such distances, as well as from nearer sources. The power of analysis is, in fact, merely the analogue to the power which the tuning-fork gives the musician to determine the pitch of a note which he hears. For what does the tuning-fork in such a case? It tells him that within the limits of his hearing, a cord or metal tongue, or the air within a pipe, or the like, is vibrating at such and such a rate. And precisely of the same nature is the information conveyed by a dark line or a bright line in the spectrum of a celestial object. It tells the observer that within the limits of vision (aided, if need be, by the telescope) some molecules or atoms are vibrating at a particular rate, so as to send out, or to absorb, light of a certain colour.

Even the work of the telescope in aiding the spectroscope has its analogue in the theory of sound. The true analogue is not to be sought (as I have seen stated) in the power of sounding-boards, or vessels of a certain shape to condense sound near its source. The speaking-trumpet of the seaman is not a sound-telescope, though it causes sounds to be perceived at a distance. Nor can we regard the phenomena of whispering-galleries as in any sense illustrating the work of the telescope. But an ear-trumpet is a true analogue of the telescope. Such a case also as that well-known one in which the sound of bells ringing a hundred miles away was rendered audible to men at sea by the action of a ship's mainsail, tautened by the wind into a vast curved reflector, illustrates exactly the work of the great curved mirror of the Rosse telescope. If a musician on board that ship, provided with a tuning-fork, or several, had tested the sounds thus rendered audible by the sail-reflector, he would have been doing, in the case of those sounds, precisely what the spectroscopist does in the case of light gathered up for him, so to speak, by a powerful telescope.

It may occur to the reader to enquire whether the phenomena of sound which thus satisfactorily illustrate the simpler phenomena dealt with by the spectroscope, may also be employed to illustrate some of those special modes of research to which that instrument has been applied. I propose now to show how aptly the analogy followed out so far may be employed to illustrate two subjects which have lately attracted a large share of attention, and have been regarded as specially surprising.

The first is the application of the spectroscope to determine the rate at which a celestial object is moving directly from or towards the eye.

Now it is to be remarked that not only does the theory of sound *illustrate*, but it actually *suggested*, this singular mode of research. It had been pointed out, as a result of the nature of sound, that if a source of sound is approaching, or receding very rapidly (whether by its own motion or the hearer's), the tone of the sound must be affected. For the tone of a simple sound depends on the rapidity with which the sound-waves successively reach the ear, and clearly more or fewer will reach the ear in a given time if the source of sound is approaching or receding, respectively. Experiment showed this to be the case. Indeed, Professor Tyndall has remarked that, without our having to renew the elaborate experiments by which the theoretical deduction was shown to accord with observed facts, everyone who has heard the peculiar change in the tone of a railway whistle as the train sweeps past us (first rapidly approaching and then as rapidly receding) has had this fact sufficiently demonstrated to him.

It was argued by Doppler that a corresponding peculiarity ought to affect the light of a star, which, either through its own motion or ours (or both), was very rapidly approaching or receding. In the former case its light, he reasoned, ought to be bluish, in the latter reddish—this relation corresponding to the waves of light of different colours. His reasoning was incomplete, though I need not here enter into the explanation of its weak points. Suffice it to say that, though no peculiarities of *colour* could be expected to arise in this way, yet the lines of the spectrum—which correspond, as we have seen, to musical *notes*—ought to be appreciably changed in place, if only the rate at which a star recedes or approaches bears an appreciable relation to the amazing velocity of light. I believe that Dr. Huggins was the first who put the idea forward in this practical form. Professor Maxwell made the requisite mathematical enquiries, and confirmed Dr. Huggins's view; and as everyone knows, or ought to know, Dr. Huggins presently applied the method so successfully as to be able to prove that Sirius is receding from us at the rate of seven hundred miles per second. The method has also been successfully applied to determine the velocity with which cyclonic storms sweep over the surface of the sun. The results on this point may be accepted as proving, if not actually that the solar hurricanes move so many miles per second, at least, that they have a very enormous velocity.

This brings us to the other instance, in which the phenomena of sound serve to illustrate the results of spectroscopic research.

Few circumstances have attracted more notice than the discovery that the bright lines which form the spectrum of the solar prominences can be seen when the sun is shining in full splendour. It is known that the ordinary appliances for viewing the sun fail altogether to show the prominences, for this sufficient reason, that the light of our own atmosphere when illuminated by the solar rays, is so much stronger than the light of the prominences as to obliterate those objects altogether. We may, by means of dark glasses or like appliances, reduce the atmospheric light as much as we please; but we reduce, *pari passu*, the light of the prominences, and they 'go out' altogether while the atmospheric light is still strong.

Under these circumstances, it seems incredible that the spectroscope should be able to make the prominence-lines visible, and still more that the prominences themselves should become visible (as they actually do) by the aid of this instrument. But the analogy of the musical continuous gamut at once explains the mystery.

We have seen that the continuous spectrum corresponds to the continuous gamut of sounds, the juxtaposition of colours in the spectrum corresponding to the succession of notes in the gamut. And the longer the spectrum the slower the succession of notes must be supposed to be, for distances along the spectrum are analogous to time-intervals in the sounding of the gamut. Now, if all the time that the gamut is being swept a certain note is sounding, we should clearly have so much the better chance of hearing this note as the gamut was more slowly sounded. If the gamut was very slowly swept we should be sure to hear the sound. And precisely in the same way, if while we get a rainbow-tinted streak (our gamut of light) as the spectrum of the illuminated air, we also get from the prominences certain bright lines, we also clearly have so much the better chance of seeing the latter as the former spectrum has the greater length; and if the length of the atmospheric spectrum is made very considerable, we shall be sure to see the bright lines of the prominences.

In the same way, the visibility of the prominences themselves, as distinguished from the visibility of their bright-line spectrum, is illustrated by the analogy of sound. The bright lines, be it remembered, are merely images of the slit which admits the light. When this slit is widened, we get images of the prominence instead of images of the slit, for the prominence is then wholly included in the opened slit. The case may be compared to that of a picture concealed by two curtains which can be opened by drawing them horizontally apart, so as to disclose a vertical section of the picture. Here, if the curtains were nearly closed, we should have a mere strip of the picture visible—a vertical bar, so to speak; whereas, when the curtain is fully open, we have no such vertical limits to the objects forming the picture, but see the true shape.

Returning to our musical illustration, we must conceive that in place of a note sounding uniformly all the time that the gamut is being swept, the note is repeated some set number of times at definite time-intervals—as, for instance, three semiquavers, a quaver, a crotchet, and three semiquavers. The recognition of this fact by the auditor would correspond exactly to the recognition of the shape of a prominence by the spectroscopist; and precisely as the former would be rendered so much the easier according as the gamut (whose sound would partially hide the sound of the single note) was more slowly swept, so the recognition of the figure of a prominence by means of the spectroscope is rendered so much the easier according as the spectrum of the illuminated atmosphere (which partially obliterates the image of the prominence) is made of greater length by the dispersive power of the spectroscope.

It has been asked what prospects there are that the history of the spectroscope will resemble that of the telescope, insomuch that at some distant date men will look back at what we are now doing as we look back at the work of Galileo or Huyghens. This is a question which is not very easy to answer. There are, undoubtedly, limits to the powers of the spectroscope as there are to those of the telescope; but whether those limits have been already nearly reached, or will only be reached after a long interval of years, is not so easy to determine. Taking for our guidance the telescope, we should have to regard the present work of the spectroscopist as unimportant compared with that which future ages will see. We should gather that the labours of a Huggins in our day bear the same relation to the work of future years, as those of Galileo bear to the researches of the Herschels, Rosse, and

Lassell. But we are inclined to view the matter somewhat less hopefully, to regard the first ten years of the spectroscope's history as comparable rather with the interval between Galileo and the elder Herschel than with that during which the first feebler efforts of telescopists were directed to celestial exploration. There will doubtless be improvements in spectroscopic appliances, as there will doubtless be improvements in telescopes; but there is little reason to hope that the work of any future spectroscopist will as far surpass what Huggins has done, as the labours of Sir W. Herschel surpassed those of Galileo. Presently, indeed, we may hear of great results obtained by Dr. Huggins himself. With a far more powerful telescope, and a corresponding increase in the dispersive powers of his spectroscope, he is about to renew his researches into the celestial depths. But while full of hope, or rather, we should say, while absolutely certain, that the results will be of the utmost importance to science, it would be too much to hope that they can be comparable in interest with those with which but four or five years ago the new analysis startled the astronomical world.

Our opticians are not idle in devising new means of attacking celestial problems. Mr. Browning's automatic spectroscope, for instance, is most ingeniously contrived to purify the spectrum—to make the gamut of light run smoothly from end to end. By this contrivance, any part of the spectrum can be examined as satisfactorily, after dispersion through six prisms, as though a single prism alone were used, and of course with far more important results. And the principle has been extended to give a pure spectrum of much greater extension.

There remains, however, in all such arrangements the necessity for increased light-gathering power. We can only apply the spectroscope effectually to light which has been gathered for us by the telescope. The problem, therefore, which telescopists have been so long attacking, that of increasing the light-gathering and defining qualities of their instruments, until the great Rosse telescope comes to be regarded as a rather puny instrument, affects importantly the fate of the spectroscope also. Sir David Brewster said that 'the long interval of half a century seems to be the period of hybernation during which the telescopic mind rests from its labours in order to acquire strength for some great achievement.' If this is so, the time is approaching when a telescope powerful enough to dwarf the fame of the Parsonstown reflector should be in process of construction. When the new telescope is completed, we may look for very important spectroscopic discoveries.

The *St. Pauls Magazine*, February, 1871.

OTHER HABITABLE WORLDS.

Not many years have elapsed since Whewell, in 'The Plurality of Worlds,' and Brewster, in 'More Worlds than One,' respectively oppugned and defended the belief that there exist other inhabited worlds besides our own earth; yet so many and such important discoveries have been made in astronomy and physics during the interval, that the question which was at issue between Brewster and Whewell may be said to have assumed in the present day a totally different aspect. The invention of a mode of physical analysis, the powers of which seem absolutely incredible to anyone who is unfamiliar with the laws on which they rest, has enabled the modern physicist to answer some of the very questions respecting which Brewster and Whewell were at issue. I propose to discuss, very briefly, the more important of the discoveries referred to above, and then to consider the evidence we have respecting the habitability of certain members of the solar system.

One of the arguments on which Whewell laid most stress was founded on our want of knowledge respecting the constitution of the celestial bodies. We know nothing, he reasoned, even respecting the substances of which our own moon is constituted, and this body is but a quarter of a million of miles from us. What, then, can we ever learn respecting the constitution of bodies which are many millions—in some cases hundreds of millions—of miles removed from us? For aught we know, not one of the elements which exist on our own earth is present in these distant globes. Nay, he even ventured to express positive opinions respecting the immense difference which he assumed to exist between several of the celestial bodies and our own earth. He held that Jupiter, Saturn, Uranus, and Neptune are but 'immense clouds,' or 'water and vapour packed into rotating masses.' The asteroids he held to be 'mere shreds and specks of planetary matter'—'watery globes, with perhaps a lump, or a few similar lumps, of planetary matter at their centre.'

In expressing the opinion that astronomers could never obtain any certain knowledge of the constitution of the celestial bodies, Whewell can hardly be said to have been unduly confident. Even his opponent concurred with him here. Brewster held, indeed, that the physical constitution of some, at least, of the other planets may resemble that of our own earth; but he was compelled to acknowledge that his views could never be established by positive arguments. He held that they were more probable than Whewell's, and that was all he ventured to say for them.

And indeed, if we consider the subject a little attentively, we cannot but feel that no scientific man could have hoped, with any show of reason, for positive information respecting the constitution of the celestial bodies. One might almost as well have hoped that it would one day become possible to communicate with the inhabitants of these outer worlds. A certain philosopher once said, jestingly, that if there be inhabitants on the moon, we might interchange ideas with them respecting mathematical problems, by means of trees planted in geometrical figures. 'For instance,' he said, 'we might construct in this way a figure illustrating the famous forty-seventh proposition of Euclid, and wait until the lunarian geometers showed by some corresponding labours their appreciation of our mathematical acquirements.' Ridiculous as this notion may appear, it certainly does not seem more absurd, at a first view, than the expectation that, by any processes man might invent, he could learn the physical constitution of bodies even more distant than the moon—that he should be able, for instance, to assert with the fullest certainty of conviction that enormous quantities of iron and copper exist in the sun's mass.

Yet it is precisely such knowledge as this which has been deduced from the application of the wonderful method of research termed 'spectroscopic analysis,' the principles of which are explained in the preceding paper.

The evidence supplied by this powerful analysis affords very complete and satisfactory information on the points mentioned by Whewell. The rainbow-coloured streak of light which forms the solar spectrum is crossed by hundreds of fine lines—here separated by well marked intervals, there clustered together with almost inconceivable closeness. It is evident, therefore, that the solar light reaches us through a very complex atmosphere. And when these lines are compared with the lines of the various terrestrial elements, it is found that many of the most important of these certainly exist in the solar atmosphere. Other terrestrial elements are probably present, but some of their fainter lines are not seen; and the lines of some elements are wanting altogether. We are not, of course, to assume that those elements are wanting whose lines are not seen; because, if any element were present in small quantity its lines would be proportionately faint. We find, indeed, a certain correspondence in this respect between the solar constituents and those of our own earth. Iron is present in large quantities as an element in the earth's composition; and we find the iron lines in the solar spectrum so strongly marked that no doubt whatever can exist respecting the presence of enormous quantities of iron in the solar atmosphere. The same remark applies to sodium, magnesium, calcium, and other elements. But gold and silver, mercury, antimony, arsenic, &c., which are so much less common on our own earth, have not yet been detected in the solar atmosphere. Copper and zinc, which are moderately common terrestrial elements, are found to exist in the solar spectrum, but probably in less quantities than iron, sodium, magnesium, and calcium, since the fainter lines of the two former elements are not noticeable in the solar spectrum.

We need hardly point out how largely the discovery that terrestrial elements exist in the sun modifies the views we are to form respecting the constitution of the planets. As the planets are opaque, we cannot tell what elements exist in their substance; but when we know that the great centre of our system is formed of the elements which constitute our own earth, we are justified in accepting as highly probable the opinion that all other planets are similarly constituted.

But this is far from being all. The range of the spectroscope extends beyond the centre of our own system. Unlike the telescope, which can do simply nothing with the fixed stars—revealing them, indeed, with heightened splendour, but affording no indication whatever of their true nature—the spectroscope tells us more about them than we could have hoped to learn even of our nearest neighbour, the moon. We obtain, in fact, precisely the same sort of evidence respecting the stars as we have already had respecting the sun, with this important difference in the evidence itself, that whereas the sun exhibits a close affinity to our own earth as respects the proportions which exist between its elementary constituents, the stars (centres, doubtless, of other systems) exhibit no such affinity. It may seem rashly speculative to found a theory on this evidence alone; but we cannot but regard it as a legitimate deduction that, in all probability, all the members of a planetary system circulating around any star are similarly constituted, and that the nature of their common constitution is exhibited by the spectroscopic analysis of their central sun.

But there is evidence of yet another kind to show that the elements we have been in the habit of speaking of as 'terrestrial' exist in other parts of the solar system. Although meteoric stones, or aërolites, have fallen on the earth at intervals during many hundreds of years, it is but recently that the scientific world has accepted as indubitable the fact that these stones are really visitants from the interplanetary spaces. Now that this fact is recognised, the chemical analysis of aërolites becomes the chemical analysis of portions of the solar system. 'There is an interest attached to aërolites,' says Humboldt, 'wholly different from that connected with any other objects of astronomical or physical research, inasmuch as by means of them we are brought into contact, so to speak, with external space, and are permitted to weigh, to handle, and to analyse masses not belonging to our terrestrial formations.' The analysis of aërolites exhibits to us the same fact which has been revealed by the spectroscopic analysis of the sun. We find that the very elements which are most common on our own earth occur most commonly also as components of meteoric stones. But, remembering that the stones which reach the earth are few in number compared with those which are wholly dissipated in the upper regions of air, the enquiry is suggested whether we cannot learn anything respecting the structure of these objects also. They are luminous through intensity of heat, and therefore they are suitable objects for spectroscopic analysis. But the difficulty is to view them with a spectroscope during their hasty swoop across the sky. Patient observers have, however, overcome this difficulty; and although it is impossible to obtain a well-defined spectrum from the light of a shooting-star, yet it has been found that certain elements which happen to have well-marked lines, and notably sodium—which, it will be remembered, is one of the elements most plentifully distributed throughout the solar atmosphere—exist in the masses of these wandering and minute members of the great planetary family.

Another argument on which Dr. Whewell laid great stress was founded on the doubt whether any planet has an atmosphere resembling that of our own earth. Astronomers had been led to suspect that most of the planets—if not all of them—are surrounded with atmospheric envelopes of some sort; but there was little real evidence on this point, and still less respecting the constitution of the planetary atmospheres. Here was another negative argument, which it seemed wholly impossible that men should ever be able to oppugn satisfactorily. Yet here again the spectroscope has afforded the clearest evidence. We have said that, the planets being opaque, it is impossible to learn in what manner they are constituted. But we can learn—or, at least, there is a possibility of our learning—whether the light reflected from a planet's surface has passed through an atmospheric envelope; for, if the planet's spectrum is crossed by dark lines not existing in the solar spectrum, these lines must be caused by vapours existing either in the earth's atmosphere or in the atmosphere of the planet; nor is it so difficult as, at first sight, might be supposed, to determine in which of the two atmospheres those vapours exist. In observing the planet Mars, Dr. Huggins noticed that the spectrum was crossed by a number of lines which appear in the solar spectrum when the sun is low down, that is, when his light passes through the denser strata of our atmosphere. Now, although Mars was not so low down as to suggest the probability that the lines were caused by the earth's atmosphere, yet it was not wholly impossible that they might have been, because the constitution of the atmosphere, as respects the amount of aqueous vapour present in it, &c., is not absolutely constant. Therefore it did not become certain that the vapours indicated by these lines exist in the atmosphere of Mars until the following crucial test had been applied:—The spectroscope was directed towards the moon, then lower down than Mars; so that, if the vapours were due to the earth's atmosphere, their lines must have been more strongly shown in the moon's spectrum than in that of Mars. But they were not seen in the moon's spectrum. Thus it was proved that there is a Martial atmosphere, and that it is loaded with the very vapours that are found in the earth's atmosphere.

It has been shown that the same vapours exist also in the atmosphere of Venus, Jupiter, and Saturn; but their lines are not quite so distinctly seen as in the spectrum of Mars—for this reason, probably, that the light received from the former planets is not reflected from their true surface, but from vaporous masses floating above the denser atmospheric strata. Thus the light has traversed a smaller quantity of these characteristic vapours, and their lines are proportionately indistinct.

Sir David Brewster laid great stress on the analogy between the planet Mars and our own earth. He pointed to the continents and oceans of the ruddy planet, to its snow-crowned poles, to the clouds which float in its atmosphere, and to numerous other analogies which mark it as well fitted to be the abode of creatures resembling those which exist on our own earth. Dr. Whewell was not ready to admit that all these analogies really exist. He argued that what we call continents and oceans may not be so, and that it is assuming too much to say that the white specks of light which cap the Martial poles are certainly masses of snow and ice. On these points recent discoveries do not speak quite so positively as on the others. But this has been done: it has been shown that the so-called lands and seas are permanent features. They have even been charted and named, and a globe of Mars has been constructed. It has been shown that the red colour of the 'continents' is not due to the Martial atmosphere. The waxing and waning of the polar snow-caps have been more carefully watched than before, and found to correspond closely with the progress of the Martial seasons. Then, as we have seen, the existence of aqueous vapour in the Martial atmosphere has been established, so that we cannot doubt that water exists on Mars in large quantities. And lastly, clouds covering extensive regions have been observed to melt away with the progress of the Martial day, exactly as the morning mists are dissipated by the heat of one of our summer days. The words applied by Brewster to long past ages of the earth's history will at once suggest themselves as applicable to the planet Mars. If this orb be indeed uninhabited, then it exhibits to us physical relations 'fulfilling no purpose that human reason can conceive; lamps lighting nothing, waters quenching nothing, clouds screening nothing, breezes fanning nothing, and everything around, mountain and valley, hill and dale, earth and ocean, all meaning nothing.'

But perhaps the most important of all Whewell's mistakes was his assumption that the climate of each planet must necessarily correspond with the planet's distance from the sun. He argued that Mercury and Venus must be as unfit for habitation, through excessive heat, as Jupiter and Saturn through excessive cold. He drew, in particular, a dismal picture of the climatic relations presented by the giant planet Jupiter, an orb which exceeds our earth more than twelve hundred times in volume, and outweighs all the other planets taken together more than twofold. A dismal mass of snow and ice, clothed in perpetual fog, with perhaps a cindery nucleus—such was his picture of that magnificent orb, the centre of a system whose motions have formed for three centuries a subject of study and contemplation for astronomers.

The labours of Professor Tyndall and his compeers have shown that it is quite impossible to judge what a planet's climate may be from the mere consideration of the planet's distance from the sun. The extent and quality of the atmospheric envelope around a planet exercise fully as important an influence on the planet's climate. The sun's heat may either be retained or radiated away as fast as it is received. If a planet has an atmosphere which is always loaded with aqueous vapour, the heat poured on the planet passes freely through this vapour to the planet's surface: but it does not pass freely away again; it is retained and stored up precisely as in a glass-house. But dry air has not this power; the reflected heat passes as freely through it as the heat directly received from the sun. There are vapours and gases which have yet more power than aqueous vapour in preventing the escape of heat. Amongst these are the gases emitted from flowers; and Tyndall estimates that 'a layer of air two inches in thickness, and saturated with the vapour of sulphuric ether, would offer very little resistance to the passage of the solar rays, but would cut off' more than one-third of those rays which would otherwise pass away as soon as received. 'It would require no inordinate thickening of the layer of vapour,' he adds, 'to double this absorption; and it is perfectly evident that, with a protecting envelope of this kind, permitting the heat to enter, but preventing its escape, a comfortable temperature might be obtained on the surface of our most distant planet.' When we remember, on the other hand, that during the full heat of the tropical summer the lofty slopes of the Himalayas and the Andes remain covered with snow, we see how largely a diminution in the extent of a planet's atmosphere may diminish the effect of the sun's heat. Thus the inhabitants of Venus and Mercury might enjoy a climate as genial as that of our own earth.[1]

We know so little of the planet Mercury that it would be idle to discuss at length the physical relations presented by this small globe. The same remark may be made respecting the distant planets Uranus and Neptune. No telescopes have sufficed to supply any positive information respecting the surface-contour and other physical relations of these important members of the solar system. We shall, therefore, confine the remarks we have to make respecting the habitability of planets to the four orbs, Venus, Mars, Jupiter, and Saturn.

If we were to accept Whewell's method of reasoning, and assume that where any one of the principal physical relations presented by our own earth is wanting a planet is not habitable by beings resembling those which subsist on the earth, we should be com-

[1] For further considerations on this subject, see *Other Worlds than Ours*, chap. iii.

pelled to pronounce at once against the habitability of the above-named planets. For it happens that although all these relations subsist severally in one or other of these planets, they do not subsist collectively in any one of them. In Venus we find the following features wherein the planet resembles our own earth. In volume Venus and the earth are nearly equal. They differ little in density; and the attraction of gravity is appreciably the same at the surface of either. The day of Venus is but a few minutes shorter than our day. Her year consists of only two hundred and twenty-five days; but this is a comparatively unimportant point. We have seen, also, that the effects of her proximity to the sun may be counteracted by a suitable diminution in the extent of her atmospheric envelope. So far, then, there is little which need render Venus a habitation unsuited to the wants of man. But, if the observations of the few astronomers who have attended to the point may be trusted, there is one feature of the habitudes of Venus which must cause a marked difference in all her physical relations from those which prevail on earth. It is estimated that her axis is bowed more than three times as much to the plane of the ecliptic as that of the earth. Thus her tropics extend nearly to her poles, and her arctic regions nearly to her equator. An inhabitant of Venus must have but a poor choice of climates if his requirements resemble those of the inhabitants of earth. If he lives near the equator, he has, during spring, a climate resembling our hottest equatorial weather; but at the seasons corresponding to winter and summer, the sun scarcely rises fifteen degrees above the horizon. If he lives near the poles, he has to endure an intensity of heat in summer such as we can form but a faint conception of; for the sun will appear to circle around the zenith without setting for weeks together. On the other hand, he has to endure in winter a cold far more excessive than than that of our bitterest arctic winters; for not only is there perpetual night around him, but the sun never even approaches his horizon, revolving always close around that point which is immediately beneath his feet. Lastly, if he lived in either of the wide zones—comprising more than half of the planet's surface—which are at once tropical and arctic, he would suffer, within the short year of two hundred and twenty-five days, all the vicissitudes of the extremest terrestrial climates.

Thus it appears that, except near the equator, none of the races of men could exist on Venus. For although there are men who live and thrive under the influence of our fiercest tropical heats, and others who endure without injury the bitter cold of arctic winters, yet certainly there are no races of men, and but few individual men, who could long survive the rapid alternation of these extremes.

It must be mentioned, however, that many astronomers are very doubtful whether the axis of Venus is really situated in so remarkable a manner. Venus is a planet very difficult to observe satisfactorily; and we must be prepared to look with extreme diffidence on all observations which deal with so difficult a matter as the determination of the planet's polar position. Modern observers of the highest repute—such astronomers as Hind, Sir J. Herschel, Dawes, and others—have expressed the opinion that the old astronomers were mistaken in many of their supposed discoveries respecting Venus. And certainly, if the exquisite instruments of the present day, in the hands of practised observers, fail—as they have hitherto done—to afford any evidence confirming the old estimate of the planet's position, we may assume that this element can no longer be looked upon as determined.

We know more of the planet Mars than of any other member of the solar system. He does not, indeed, approach us quite so nearly as the planet Venus, but he is seen under much more favourable circumstances. Venus at her nearest approach presents her darkened hemisphere towards us; and at all times, as already mentioned, the peculiar brilliancy of her light renders her a very difficult object of observation. With Mars it is otherwise. He not only turns a fully illuminated disc towards us when he is nearest, but he alone, of all the planets, has an atmosphere so constituted that we can examine his real surface. We have already seen in how many respects the physical relations of this planet resemble those of our own earth. There are other points of resemblance, however. The inclination of his axis differs little from that of the earth's axis, insomuch that his seasons closely resemble those of the earth in character. His day is about forty minutes longer than our own. His year, however, is different, being nearly twice as long as ours. One can hardly imagine that vegetation on Mars can resemble terrestrial vegetation, when his seasons exceed ours so much in length.

But perhaps the point in which the physical relations of Mars differ most markedly from those of our own earth, is the nature of gravitation at his surface. Mars is a much smaller planet than the earth; and as his density differs very little from that of the earth, it follows that gravitation at his surface is much less than at the earth's. A man who weighs ten 'stone' on our earth would weigh less than four on Mars; and our Bantings and Lamberts would be light active fellows, seven or eight 'stone' or so in weight. All substances would be similarly reduced in weight. Martial gold would be no heavier than terrestrial tin, Martial oak than terrestrial cork, and so on. Whewell, in his Bridgewater Treatise on Astronomy, is disposed to attach great importance to the exact relation which subsists between the force of gravity and the motions of vegetable juices. If this view is correct, it is certain that none of our plants could thrive on the soil of Mars. However, those who appreciate the power by which Nature adapts the various races of plants and animals to the soils on which they subsist, will see little in the habitudes of Mars to render that planet uninhabitable by races resembling—though not actually identical with—those which subsist on the earth.

The chief arguments for the habitability of Jupiter are founded on his enormous magnitude, and the magnificence of the system which circles around him. It seems difficult to imagine that so grand an orb has been created for no special purpose, and it is equally difficult to conceive what purpose Jupiter can be said to fulfil unless he is the abode of living creatures. He is, indeed, an object of wonder and admiration to our astronomers; but the mind must be singularly constituted which can accept the view that Jupiter was constructed for no other end. When every object around us suffices to exhibit the omnipotence of the Creator, we require no such evidence as is afforded by a globe exceeding the earth 1,200 times and more in volume. The light afforded to us by Jupiter is so insignificant also, that we cannot suppose him to have been created for no other purpose than to supply it. His influence in swaying the planetary motions is important, and he also appears to have a noteworthy influence on the sun's atmosphere; but neither influence seems necessary to the well-being of the inhabitants of earth. Thus we appear forced to concede that Jupiter has been constructed to be the abode of living creatures—unless we suppose that his function is to sway the motions of his satellites, and that these satellites are inhabited. Without deciding between these two views, I proceed to point out those points in which the physical relations exhibited by Jupiter differ most markedly from those of our own earth.

The enormous volume of Jupiter is in part counteracted—so far as its influence on the inhabitants of Jupiter is concerned—by the small density of the planet, insomuch that the attraction of gravity at his surface is not so much greater than terrestrial gravity as might be supposed. Yet it exceeds the latter more than twofold; so that the weight of an inhabitant of our earth would be increased in about the same proportion if he were removed to Jupiter as it would be diminished if he were removed to Mars. The lightest men on our earth would find themselves as unwieldy as our Lamberts and Bantings if they were placed on Jupiter's surface. We are compelled to recognise in this circumstance a peculiarity which would render Jupiter unfit for beings constituted exactly like the inhabitants of earth; but modifications not much more marked than those which distinguish the various species of the same genera on earth would be sufficient to enable terrestrial races to endure, without discomfort or inconvenience, the powerful gravitation experienced by the inhabitants of Jupiter.

The day of Jupiter is less than ours in the proportion of two to five, while his year contains nearly twelve of ours. His axis is so nearly perpendicular to his orbit that there are no appreciable seasons on his surface. This circumstance has been pointed out by some astronomers as a convenient offset against the effects of his enormous distance from the sun. But it will not do to dwell too strongly on this point, since we find no such arrangement in planets which are yet further removed. The small density of Jupiter's substance led Whewell to pronounce the planet to be a fluid mass; and Brewster was at some pains to deal with the peculiarity. He endeavoured to show that Jupiter might be formed of solid substances, because there are such substances on our earth of even less specific gravity than Jupiter's. However, the possibility that Jupiter's sphere may be smaller than we might infer from the apparent size of his disc—an extensive cloud-laden atmosphere bounding the disc we measure—is sufficient to remove any objection to the habitability of the planet founded on this peculiarity alone.

In many respects the physical relations of the planet Saturn correspond closely with those of Jupiter. There are, however, two points of difference. In the first place, gravitation at his surface is far less than at Jupiter's, and differs so little from terrestrial gravitation that we may look on this relation as one with respect to which Saturn is well fitted to support terrestrial races. On the other hand, the influence of the Saturnian ring-system would be so unfavourable to most terrestrial races, that one can hardly suppose but that Saturnian races are constituted very differently from those which subsist on our earth. It results from a careful examination of the effects of the two gigantic rings which surround Saturn that the sun is totally eclipsed by them for years together in the temperate and sub-tropical zones of Saturn; and that in Saturnian latitudes corresponding to that of Madrid total eclipse lasts for more than eight years.

It appears to me that a careful consideration of all the evidence leads to two conclusions:—First, there is an obvious adaptation of the physical constitution of the planets we have been considering to fit them to be the abodes of living creatures; and secondly, there are obvious reasons for doubting whether these living creatures can very closely resemble terrestrial races.[1]

To some minds it may appear that to discuss the fitness of the planets to be the abode of living creatures different from those which subsist on the earth is altogether beside the question we are dealing with. The habitability of the planets, many argue, means their fitness to support terrestrial forms of life. But this view appears to me a mistaken one. If indeed it can be shown, that in any planet, not one of the physical relations subsists which we hold to be essential to the existence of terrestrial races, then indeed it seems idle to speculate upon the general question of the habitability of that planet. For instance, when we consider the case of the moon—without air or water, subjected to a scorching heat during its long day of half a month, and to a corresponding intensity of cold during its equally long night, and that it is in other important respects utterly unfit for habitation by terrestrial races —we seem little encouraged to discuss how far the moon may be fitted to support other forms of life, since nothing in our experience enables us to conceive what

[1] This was written in 1868. At present (1874) I hold somewhat different views.

forms of life could possibly exist in so sterile an abode. But when we find in certain planets an obvious provision made for the support of forms of life corresponding to the forms existing on the earth, we seem to be justified in recognising and discussing the habitability of these bodies.

And this leads me to point out a mistake which is commonly made in the application of that argument from the analogy of our own earth, which those who believe in the habitability of other worlds justly use. We cannot reason from the fact of the earth's habitability to the habitability of the other planets. We might as reasonably argue from the presumed unfitness of the moon for habitation that the other celestial bodies are also uninhabited. But we can derive a powerful argument from the analogy of our planet when we consider the economy of life upon its surface. When we see the scorched regions of the tropics and the solid ice within the arctic circle freely supporting terrestrial races, while not only the continents, but the depths of the ocean and the realms of air are crowded with living creatures; when we find that in long past ages, during which different physical relations from the present have subsisted, the same abundance of life has existed on the earth's surface, we may fairly assume that the planets which present so many physical relations resembling those of our earth are not untenanted by living creatures.

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OTHER INHABITED WORLDS.

In the preceding essay we considered the conditions of habitability of the four planets—Venus, Mars, Jupiter, and Saturn. We selected these as being the planets which, so far as we know, present the closest features of general resemblance to our own earth. Yet we could not fail to perceive that although each of them exhibited a striking resemblance to the earth as respects one or more features which we are in the habit of associating with its habitability, there was also, in each case, some distinctive peculiarity which prevented us from pronouncing any of the four planets to be a suitable dwelling-place for man, or probably for any of the principal races now subsisting upon the earth.

And if the four planets which had been selected as affording the strongest evidence in favour of our thesis that there are other inhabited worlds, must be pronounced not to be habitable by terrestrial races, it will be readily conceived that the orbs which urge their stately course elsewhere through space, under a thousand different conditions of heat, of illumination, of seasonal changes, or the like, would for the most part be altogether unfit abodes for the present inhabitants of the earth.

Yet it will be noticed that in adopting a different title for the present paper—in which I propose to deal with worlds which seem thus wholly unfit for habitation—I have assumed one which in reality asserts more than the former one. The title 'Other Habitable Worlds,' simply inferred the belief that there are regions throughout space which are more or less fit for habitation, without implying any opinion as to the fact of those worlds or any others being actually inhabited. At present I am about to deal with worlds, many of which may at once be admitted to be uninhabitable by most, if not all, of the races living upon the earth, yet I adopt a title which implies the belief that those worlds are certainly the abodes of living creatures.

The fact is, that although when contemplating our solar system we recognise evidence of adaptation to the wants of living creatures, it is when we attempt to conceive the immensity of that space, thronged with suns, which lies beyond the solar system, that we are most powerfully impressed with the conviction that there must be other inhabited worlds. Insignificant as our earth undoubtedly is when her dimensions are compared with the magnificent proportions of many of the other planets, and still more when considered with reference to the grandeur of the solar system itself, we have a far more startling contrast to contemplate when we compare the solar system with the dimensions of the sidereal scheme. From the nearest of the fixed stars the orbit of Neptune would have an apparent diameter scarcely equal to one-sixth part of the moon's, and the orbit of the earth would be but as a point. Even the gigantic orbit of Neptune would seem but a point, as seen from many of the stars which shine conspicuously in our heavens. But the lucid stars, far off as they are from us, are quite close in comparison with the stars which come into view under the searching eye of the telescope. It has been calculated that some of the stars seen with Lord Rosse's telescope shine from such an enormous distance, that light takes upwards of 50,000 years in travelling to us from them. Now consider for a moment the flight of a light-ray from a star at this distance on one side of our system to another as far off on the opposite side. For 100,000 years the light speeds onward—each second sweeping over nearly 200,000 miles; past stars and systems it rushes on, but far away on every hand are other stars and other systems to which it comes not near. During 3,000 generations of mortal men—if one can conceive that our race could last for that time—the pulsations of the ether are transmitted along the tremendous line which separates the two stars. Yet during all that time—if we are to accept the opinion of those who hold that our earth is the only inhabited world—the onward-rushing light never approaches a single spot where sentient beings are to be found, save one tiny globe, around which it could circle eight times in one of the seconds which make up the vast period of its flight.

But if the magnitude of the sidereal system forbids us to regard our small earth as the only inhabited world within the universe, the evidence afforded by other features of interstellar space is not less convincing.

If it could have been proved that the stars are mere lights—that, in some inconceivable way, they have a power of shining from enormous distances upon our earth and her companion planets—we might indeed be perplexed to find so elaborate a scheme of illumination prepared with no other end but to supply an amount of light which two or three small moons could have furnished equally well; but we should have little to encourage the notion that the stars resembled our sun. For we know of the sun that it is constituted of the very elements which form our earth; that it is a massive globe swaying all the movements of the system of which it is the centre; that while it is indeed a lamp, it is also much more than a lamp—that it is, in fine, the very life, so to speak, of the planetary scheme.

But the stars have been proved to be also suns, not merely in the sense that they are lamps as brilliant as our sun, and many of them much more brilliant, but also in physical constitution. In the paper on 'Other Habitable Worlds,' I simply touched upon the method of this proof; it is necessary, however, that we should now discuss the subject somewhat more at length. Let us begin with the consideration of the noted star, Aldebaran.

How far from us this brilliant orb may be we do not know. Although it is so conspicuous as to suggest the idea that it is much nearer to us than most of the fixed stars, and although the largeness of its proper motion—that is, its change of position amongst neighbouring stars—suggests the same conclusion, this orb has resisted all the attempts of astronomers to determine its distance. In other words, although the earth sweeps round the sun in an orbit upwards of 180,000,000 miles in diameter, the apparent position of Aldebaran is not appreciably affected by this enormous orbital range—not more affected, for example, than the apparent position of a steeple twenty miles off would be if we observed it first through one pane of a window and then through a neighbouring pane. It will be understood, therefore, that Aldebaran shines from a vast distance.

Now, when we combine the consideration of the enormous distance at which Aldebaran shines with the fact that the star is so brilliant, we see that the true splendour of this orb must be very great. Probably Aldebaran shines with thirty or forty times as much light as our sun; and certainly its brilliancy is not less than that of the sun. If, then, Aldebaran were constituted like our sun, if its intrinsic splendour as distinguished from its actual splendour corresponded closely with his, it would follow that the star which seems to us a mere point of light is, in truth, a globe probably 200 times as large as our sun—a vast mass of molten matter, the centre of forces vaster even than those which the sun exerts. Yet we know that the sun is the seat of nearly all the living energy, so to speak, of the solar system. The coals we burn, the food we eat, the works we do, the very thoughts we conceive, owe their several forms of force to sun-power. This is no dream, no fanciful speculation, but the simple statement of an established truth. Since the sun, then, blazing in the centre of the planetary scheme, is to be looked upon as the representative of all the forms of force which exist or have existed during countless æons upon the earth, and also of all those unknown forms of force which prevail in the other planetary worlds, so also Aldebaran's glory is the representative of other unknown forms of force—organic, vital, mental—prevailing in a scheme of which he is the centre. If not, his light is a mockery and a delusion, and our reason, in so far as it deals with questions of the sort we are considering, is given but to lead us astray.

We have said that all this would follow if it were shown that Aldebaran is constituted in a manner resembling that in which the sun is formed. This is all, as it seems to us, which requires proof. Once this is established, all doubt should vanish.

Now, the constitution of the sun, so far as it is known to us, is peculiar. Therefore we shall be the less liable to fall into any error in instituting a comparison between Aldebaran and the sun, or in recognising any traces of resemblance in their respective constitutions.

The sun is not simply, as was supposed of old, a globe of incandescent matter. The light we receive from him comes, indeed, from an incandescent source, but it has been subjected, before reaching us, to a very singular and characteristic process—a process the traces and peculiarities of which there is no mistaking. From the incandescent matter which supplies the solar light, there stream forth light-waves of every possible length between certain limits, namely, between the length belonging to the extreme red end of the solar spectrum and that belonging to the extreme violet end.[1] But when the light reaches us certain wave-lengths are found to be wanting. The case corresponds to that of a harp in which certain strings are missing here and there. Or, rather, if a harp were constructed with an indefinite number of strings increasing gradually in length between the usual limits, so that on striking the finger across them, a sound would be produced changing by indefinite gradations from the gravest note audible by human ears, to the most acute; then if strings were removed here and there—and sometimes several close together—to the number of several thousands, the sound produced when the finger was drawn across the strings would differ from that before produced, precisely as the light received from the sun differs from that which is actually poured forth by the incandescent central mass.

Now, from the principles enunciated in the introductory paper, we have seen how this fact is to be in-

[1] Of the former class of waves 40,000 go to the inch, of the latter 60,000.

terpreted.[1] We know that the missing waves have been absorbed by vapours through which the light from the incandescent centre has had to pass. And we learn what the vapours are which thus absorb a portion of the solar light, with as much certainty as we should learn what the notes were which were wanting from the gamut of sound; because we know what the vapours are which would give out the light that would supply the darkened spaces of the spectrum, as certainly as a musician could tell what the notes are which would fill up the gaps in the chromatic scale.[2]

Let us now see whether this peculiarity of solar light has any counterpart in the constitution of the brilliant star Aldebaran. It need hardly be said that the spectrum produced by a fixed star is very much less brilliant than that of the sun; but still, when a powerful telescope, armed with a spectroscope of adequate power, is directed towards Aldebaran, there flashes out in all its exquisite beauty, the rainbow-tinted streak of light which is so full of meaning to the physicist. And now let us suppose that we are in the observatory of our leading astronomical spectroscopist, Dr. Huggins, and that he had so arranged matters that the spectrum of Aldebaran is sufficiently well-defined to present its dark lines—if indeed the constitution of the star resembles that of the sun. A momentary glance settles the question. *There* are the dark

[1] In the original paper, the reasoning of the introductory essay was briefly sketched in.

[2] See the introductory essay on the Gamut of Light.

lines which indicate that Aldebaran, like our sun, is a self-luminous orb surrounded by absorptive vapours. The star is thousands of times farther from us than the sun, but this matters little, if its light bears so well the message we sought to hear. Over millions on millions of miles that message has come speeding through the interstellar spaces, and it is brought as faithfully to us—so far, at least, as its main meaning is concerned—as if we were no farther from Aldebaran than from our own sun.

It will be noticed that I have not hitherto laid any stress on the question, *What* are the substances which constitute Aldebaran? It has seemed sufficient that it should be proved that this star is constituted after the same peculiar fashion as our sun. The elements which appear in its substance might be wholly different from those which appear in the sun—that is, from the common elements known to us—and yet there would be nothing to discountenance the theory that Aldebaran is a sun, the centre of a system to whose inhabitants he imparts light and heat. For example, if there were no iron in the constitution of Aldebaran we might conclude, with some appearance of probability, that there could be no iron in the orbs which circle around him; but other substances unknown to us might perfectly well supply the place of iron to the inhabitants of these bodies. Nor even would the absence of oxygen and nitrogen—supposing such a defect could be proved—which is not the case—affect the question we are dealing with. It would, indeed, suffice to render it highly probable that the orbs which form the Aldebaranic system have not atmospheres resembling our own; and, therefore, that the inhabitants of those orbs cannot possibly be constituted like any living creature known to us. But to conclude that, therefore, no life subsists on the members of the system which circulates around Aldebaran, would be as unreasonable as it would be for anyone to argue that the seas are unfit abodes for living creatures because land animals are not so constituted as to be able to live under water.

But although not necessary to our argument, the fact that terrestrial elements actually exist in enormous quantities in the constitution of the star Aldebaran, is one whose force is at once apparent. We cannot tell what may be the amount of any vapour which must exist in the atmosphere of so distant an object, in order that the presence of the vapour may be rendered perceptible to the observer on earth. But we are safe in assuming that the quantity required is very great indeed. Hence, when we learn that beyond all doubt iron exists in the atmosphere of Aldebaran, we know that all the produce of the iron mines on earth would not form a millionth or a billionth part of the iron which lies out yonder so many millions of millions of miles beyond the boundaries of the solar system. Then follows at once the question, What is the use of all that enormous mass of iron? That it has a use, prospective, if not present, seems to us a reasonable supposition. Respecting its present use, we cannot guess any more than we can respecting the present use of the iron in the sun's atmosphere. Of its prospective use I will not speak, though I have an opinion of the position which the suns will one day occupy as habitable abodes richly supplied with all the elements which subserve the welfare and comfort of living creatures. But my argument at present leads me to treat of the iron in the atmosphere of Aldebaran, rather as symbolical of the existence of other iron—in a form as available as that of the iron of our mines—in inhabited worlds circulating around Aldebaran. The constitution of a star may be held to typify the constitution of the system of which it is the centre. We have, indeed, no other argument for this than the resemblance which has been discovered between the constitution of the sun and that of our earth; but this argument is a very strong one when rightly considered, since the probability of the conclusion is measured in such a case by the antecedent improbability that a resemblance of this sort should exist by accident. If there is no necessary general resemblance between the constitution of a sun and the constitution of the planets which travel around it, insomuch that all these planets may have totally different elementary constituents, and their sun yet another constitution, then obviously we must look upon it as a highly extraordinary coincidence that our sun should be constituted of materials so closely corresponding with those which form our earth—one of the smallest members of his system. We may, therefore, be permitted to doubt whether there is in truth any coincidence of this sort at all, and to conclude with a high degree of probability that the observed resemblance evidences the existence of a law regulating the whole of the solar system. With equal probability we may conclude that a similar law regulates each of the systems which exist within the sidereal universe.

Thus our question respecting the use of iron in the far distant region of space with which we are dealing, really refers to the iron which exists, we infer, in the planetary orbs circulating around Aldebaran. Without knowing anything about these bodies—about the distances at which they revolve around their sun, or about the seasonal relations which result from the different inclination of their axes to the plane in which they travel, and so on—we are yet enabled to pronounce that they are all supplied with the most useful of all the metals known to man, and that this supply is not meaningless. We infer the existence—either past, present, or prospective—not merely of living creatures, but of rational beings,[1] able to make a proper use of the valuable metal. And this view is strengthened

[1] I have passed here somewhat beyond my record, but not without a purpose. I wish to touch for a moment on a point which is perhaps more interesting than any other connected with the question of other inhabited worlds. It seems to me, that to believe our little earth to be the sole abode of rational creatures, is no less preposterous than to believe that no life of any sort exists beyond the confines of so minute a speck in creation. But many are pained even by the mere suggestion of the possibility that man may not be the only creature which is capable of appreciating the wonderful power and beneficence of the Creator. There

when we find that mercury, bismuth, antimony, magnesium, sodium, calcium, and hydrogen exist in enormous quantities in the atmosphere of Aldebaran, and therefore probably in the constitution of the globes which circle around him.

I have hitherto spoken only of the star Aldebaran. I have selected this star because it happens to be one which has been very carefully examined by the eminent physicist to whom modern astronomy owes so much—Dr. W. Huggins. The conclusions to which he has arrived, and of which I have made use in the preceding paragraphs, are not merely probable suppositions—they are absolute certainties. The

seems to them something dangerous—something irreligious—in the thought. The teachings of Christianity, they argue, mark out mankind as the object of so special a regard, so exceptional a dispensation, that to assume the possible existence of other rational and therefore responsible creatures, is practically to deny the truth of Christian doctrines. To me it appears that those who argue thus misinterpret the doctrines they seek to defend. If there is one truth which may be said to be the essence at once of Bible words and scientific teachings, it is that in the infinity of God's love there are many infinities. It is when we seek to measure that infinity by our finite conceptions that we are in danger of going astray, of speaking of God's love for his creatures as of something exhaustible, and of showing a real want of faith in his words, by the unwillingness we display to open our eyes to the teaching of his works. The Christian, fully as much as the man of science, ought to be ready to admit that, although God's love to man is infinite, yet that infinity of love does not exclude the possible existence of creatures who receive an equal share of his gifts. The argument of the well-meaning but perverse persons we have alluded to is as unreasonable as would be that of a creature—say an ant—who, observing the perfection with which the structure of himself and his fellow-ants was adapted to ant-life, should argue that therefore all other creatures must be imperfectly, or not at all, adapted to the circumstances around them.

significant lines in the spectrum of the star have not been judged merely by a careful comparison of their apparent positions with those of certain lines in the solar spectrum, or in the spectra of different elements. A method, the results of which there is no mistaking, has been made use of. When the telescope was directed upon Aldebaran, matters were so arranged, that side by side with the spectrum of the star there should be brought, in succession, the bright-line spectra of various elements. If in any case it was found that exactly opposite the bright lines of one of the latter spectra there were well-marked dark lines breaking the continuity of the rainbow-tinted spectrum of Aldebaran, this exact coincidence was at once decisive of the existence of the corresponding element in the vaporous envelope of the star.

But Aldebaran was not the only star which was thus examined. Betelgeuse, the leading brilliant of the splendid constellation Orion, was dealt with even more carefully, and the presence of iron, sodium, magnesium, and other elements, determined with equal certainty. This sun, however, is not constituted exactly like the sun which we call Aldebaran. And one very noteworthy peculiarity characterises the spectrum of Betelgeuse. The lines of hydrogen which are well seen in the solar spectrum, and in that of Aldebaran, are wanting in that of this brilliant star.

The star Capella, one of the brightest of the northern hemisphere, was found to have a spectrum closely resembling that of our sun, and crowded with lines. The equally brilliant Vega presents a similar spectrum.

Sirius, which outshines more than threefold the brightest northern stars, is observed under somewhat unfavourable circumstances in our latitudes, as he never attains a high altitude. 'The spectrum is crossed,' says Dr. Huggins, 'by a very large number of faint and fine lines.' The presence of hydrogen in the absorptive atmosphere of this star is indicated in a very decided manner by the intensely strong lines which correspond to this element. 'It is worthy of notice,' remarks Dr. Huggins, 'in the case of Sirius and a large number of the white stars, that at the same time that the hydrogen lines are abnormally strong as compared with the solar spectrum, all the metallic lines are remarkably faint.' The presence of sodium, magnesium, and iron was, however, demonstrated in this case as in many others.

We close this part of our case by remarking that the stars referred to have been merely chosen on account of their brightness and the consequent distinctness and splendour of their spectra. There is every reason for believing that with a telescope of adequate illuminating power, the faintest stars would afford precisely the same evidence. Indeed, Dr. Huggins names a number of stars—some of which are not very brilliant—whose spectra he examined, and he says respecting them: 'Numerous lines are seen in the spectrum of each, and in some several of the lines were measured; but we have not instituted any comparisons with the metallic spectra as yet.'

We see then, in the whole sidereal system, a series of suns resembling our own in all essential respects—many of them falling short of it, perhaps, in splendour, but many of them certainly surpassing it. And around these suns there circle worlds of every variety of magnitude, many of which doubtless form centres of systems as varied in character as are those of Jupiter, Saturn, Uranus, and Neptune. On these worlds subsist, we doubt not, myriads of forms of life, animal and vegetable. Thousands of these systems are probably utterly unfit for habitation by the races which subsist upon the earth or upon the other members of the solar system, and it is equally probable that the races subsisting in thousands of these systems would perish if subjected to the conditions prevailing in any part of the solar system. But on one point we may rest well assured. Whatever be the nature of the races subsisting in any of these worlds, and whatever may be the peculiarities of the conditions to which they are subjected, the most perfect adaptation doubtless exists between those unknown living creatures and the structure of the worlds on which they live. This lesson is taught by all that we see around us. If on our earth there were the most perfect uniformity in the conditions of habitability which prevail in different lands and seas, or if we could even detect no traces that in past ages the world had been less well adapted to support the races which at present subsist upon it, and that accordingly former terrestrial races differed from the present—yet even then, the perfect adaptation of the limited number of races which we should know of, to the conditions under which they lived, would afford to the thoughtful mind the most striking evidence that this adaptation is a fundamental law of nature. But when we find that the most striking peculiarities distinguish the climate and habitudes of one country from those of another—that animals which thrive in one country would perish if removed to another, and yet that no part of the world remains uninhabited,—and not only so, but that the creatures which live in each part of the world are adapted in the most perfect and wonderful manner to the circumstances in which they are placed,—we are at once impressed with the universality of the law of adaptation. And if any doubt could possibly remain, if we could suppose, for instance, that the law of adaptation only extends over the range of variability which is observed in the conditions of life existing under the cognisance of man, this doubt could hardly fail to be removed by the careful examination of the geologic record. There we behold traces of conditions very different from those which prevail in the present day; yet there, also, we obtain tidings of wonderful living creatures, framed on a plan which fitted them admirably for the circumstances in which they were placed; and there, too, we find the signs of a luxuriant vegetation, at once fitted to thrive in the climates which then existed, and to supply the wants of the strange beings which then inhabited our world. On every side, then, and in every age, we find the signs of that power of adaptation, which is, so to speak, the secret of the exuberant vitality of nature. And associated with this law is a law which seems at first sight opposed to it, a law indicative of the prodigality, and even of the destructiveness, of nature's dealings with living races—the law by which races not adapted, or rather less adapted than others, to the conditions around them, gradually perish. We say that this law seems opposed to the other, because it involves a want of adaptation to those very conditions with respect to which the other law implies the most perfect adaptation. Yet, in reality, the two laws are correlative. It is through the action of one law that the other law prevails. The conditions of habitability are at each moment slowly changing, and Nature is careful neither of the individual nor of the type in changing, *pari passu*, the qualities of the living creatures which subsist under these varying conditions—

> 'So careful of the type?' but no,
> From scarped cliff and quarried stone
> She cries 'a thousand types are gone,
> I care for nothing, all shall go.'

Hitherto we have confined our attention to systems which, however their members may differ from the members of the solar system, yet resemble that system in this general respect, that they circle around a single central sun. But I must now touch on the possible existence of life under circumstances which do not even present this feature of resemblance to those with which alone we are familiar.

Around the double, triple, and multiple stars, there doubtless travel systems of worlds crowded with living creatures. How strangely must the conditions under which these creatures subsist differ from those which characterise life upon our earth! To begin with, consider the complexity of the motions which must result under the action of gravity, when, instead of a single centre, the planets which form the system dependent on a double or multiple star are subject to the attractions of two or more bodies which are themselves continually in motion around each other. The mathematicians in those dependent worlds should be far better than ours, if they are to deal successfully with the problems thus presented to them; and the remarkable climatic changes which must result from these complex motions seem to involve the necessity that the inhabitants of these worlds must have strong constitutions to enable them to bear in safety such important variations. Then again, where the suns are differently coloured, there must result those curious interchanges of light suggested by Sir John Herschel—'a green or a red day, for example, alternating with darkness or with white light.' He terms them 'pleasing contrasts and grateful vicissitudes,' and doubtless they are so to the inhabitants of those worlds: yet we on earth should hardly find such vicissitudes agreeable to us. The great law of adaptation exerts its influence, however, in these parti-coloured systems as elsewhere; and whatever doubts we may have respecting the actual habitudes prevailing there, we may be sure that they are fully as well suited to the wants of the inhabitants of those systems as are terrestrial habitudes to the wants of the inhabitants of earth.

The nebulæ again afford an interesting subject of speculation. Some of these objects have been shown by spectroscopic analysis to shine with true stellar light, while others are simply immense masses of glowing gas. It is a moot point amongst astronomers whether we are to regard nebulæ of the former sort as belonging to our own sidereal system, or as lying far beyond it—forming, in fact, to use the expressive verbiage of German astronomers, vast 'island universes' scattered throughout the 'sea of space.' Nor does it greatly signify, so far as our present subject is concerned, which view we take. For if it should be proved that no outlying universes have yet been seen by man, yet every astronomer who recognises the true teaching of his science holds that our sidereal system is no more to be regarded as the only sidereal system of the universe, than our sun is to be regarded as the only sun in the sidereal system. Beyond that system, then, we look into the outlying spaces, and still the mental eye sees myriads of worlds richly stored with endless forms of life.

What opinion we are to form respecting the gaseous nebulæ, or respecting their correlatives in our solar system—the comets—it would at present be difficult to say. Until we know the purposes which these objects subserve in the economy of the universe, it would not be easy to indicate their association with the question of other inhabited worlds. Our knowledge respecting the actual nature of these bodies is too recent to permit us to speculate respecting their functions.

The *St. Pauls Magazine*, March, 1869.

THE ROSSE TELESCOPE SET TO NEW WORK.

THE great Rosse telescope, with its monster tube down which a tall man can walk upright, and with a light-gathering power so enormous that even by day the stars seen through it shine like miniature suns, has not remained idle since the lamented death of the astronomer who constructed it. Not only has the work to which Earl Rosse devoted it—the delineation of those strange stellar cloudlets that fleck the dark vault of the heavens—been continued with unremitting assiduity, but its unrivalled powers have been devoted to aid the progress of those new and subtle modes of research which have recently been invented. The task was no simple one. The gigantic tube, with its ponderous six-feet mirror, had been poised so skilfully that a child could guide its movements. But for the new work which it was to be called on to perform much more was wanted. A new power had to be given to the telescope—a power of self-motion so exactly regulated that the gigantic eye of the telescope might remain steadily fixed on any given star or planet, notwithstanding the swift rotation of the earth, by which in the ordinary condition of the tube, the celestial objects are carried in a few moments across its field of view. This power has now been given to the great reflector, and thereby the value of the instrument as an aid to scientific research has undoubtedly been more than doubled. Already it has solved a question which had been found to lie far beyond the powers of inferior instruments; and what it has done is, we believe, the merest foretaste of what it is likely to do in coming years.

Let us briefly consider a few of the qualities of this wonderful telescope, so that we may be able to appreciate its unequalled power in the subtle modes of research which our physicists are now applying to the celestial bodies.

As a light-gatherer the Rosse reflector is *facile princeps* among telescopes. Sir William Herschel's great four-feet reflector and Lassell's equally large telescope come next to it; but the power of either of these instruments is less than one half that of the Parsonstown reflector, the illuminating surfaces of their mirrors being, in fact, exactly four-ninths of that of the Rosse telescope. It is, however, when we compare the power of the great mirror with that of the unaided eye, that we see its enormous capability as a light-gatherer. On a very moderate computation the light-gathering power of this wonderful instrument is found to be upwards of twenty thousand times that of the unaided eye; and it follows that if the faintest star visible to the unaided eye were removed to 140 times its present distance, it would still remain visible to the giant eye of the Rosse reflector.

If the other qualities of the great telescope were all proportioned to the one we have been considering, I might leave the reader to conceive what its powers would be, from the simple consideration that any celestial object would appear as distinctly when seen by its aid as it would if the unaided eye were brought to only one-140th of its actual distance from the object. Unfortunately this would be largely to overestimate the 'telescopic' powers of the instrument. I have spoken of its strength, I have now to speak of its weakness; and the inquiry is rendered so much the less unpleasing by the consideration that in some of the new modes of research to which the telescope is to be applied, the faults which are inseparable from a reflector of such enormous dimensions are of comparatively small moment.

The fault, then, of the Rosse reflector, as of all very large reflectors hitherto constructed, is that it does not present objects in a perfectly distinct manner. It used to be remarked of the great four-feet reflector of Sir William Herschel, that it 'bunched a star into a cocked hat;' and it is whispered that Lassell's great mirror once exhibited an occultation of one of Saturn's satellites when no such phenomenon had in reality taken place. The fact seems to be, that in the present state of mechanical science, it is impossible to construct a reflector of such enormous dimensions as these with that perfect truth of figure which De La Rue has given to his 13-inch reflector, and which Mr. With seems able to give, in every instance, to the mirrors he constructs for the Browning reflectors. The very weight of a large mirror tends to change the figure of its surface; and though the change may seem insignificant, yet the defining power of the telescope is seriously affected. The reader may judge of the effect of a slight change of figure, from the fact that a single hair between the mirror of a nine-inch reflector and the sustaining-bed suffices to cause the most annoying distortion in observed objects.

It is on this account that we hear so little of any discoveries effected within the range of our own system by means of the great Parsonstown reflector. Far better views of the planets have been obtained by much smaller telescopes. The late Mr. Dawes obtained singularly distinct views of the planet Mars with a refracting telescope only eight inches in aperture, whereas the views of this planet obtained by means of the Rosse telescope are perfectly wretched. I have before me, as I write, eight such views, and it is impossible to say what they mean. The planet Saturn, again, the most beautiful and interesting object in the whole heavens, has exhibited all its most charming features in the 13-inch reflector of Dr. De La Rue. In the Rosse telescope,—well; all that I shall say is that a distinguished foreign astronomer was once invited to look at the planet by its aid, and his account of what he saw was thus worded: 'They showed me something, and they told me it was Saturn, and I believed them.'

But great reflectors are not constructed for that sort of work. Their object is to bring into view those outlying regions of space which are hidden in the twilight of vast distance. The tiny cloudlets which shine from beyond the great depths of space are changed under the eye of the giant reflector of Parsonstown into glorious galaxies of stars, blazing with a splendour which cannot be conceived by those who have not themselves looked upon the magic scene. To span the vast abysms of space, to bring into view galaxies as yet unknown, and to exhibit the strange figures, the outreaching arms, and the fantastic convolutions of those which are but barely visible in other telescopes, such is the work which is looked for from the great reflector, and such is the work which in the energetic hands of the late Lord Rosse it successfully achieved.

But now a new and wonderful mode of inquiry has been devised, and has rapidly taken its place as the most important of all the methods of research which science has as yet placed in the hands of her servants. I refer to spectroscopic analysis, or the analysis of light by means of the prism. This mode of research is one to which the powers of the great telescope are admirably adapted. For a reason that will presently appear, it will be well that I should give a brief sketch of the nature of this mode of analysis.

The shortest and simplest way of exhibiting the nature of spectroscopic research is by a reference to some of the best known phenomena of sound.

White light may be compared to the sound heard when all the notes of a piano or harp are heard at once. We resolve white light by means of a prism into a rainbow-tinted streak, and we have at once the chromatic scale of light—corresponding to the sound produced when the notes of a piano are swept from end to end. The red end of the spectrum is the *bass*, the blue end is the *treble*. But some light when thus resolved shows a spectrum crossed by black lines : in this case some notes of the chromatic scale are wanting. Other light shows a spectrum of bright lines only : in this case some notes only of the scale are sounding. Chemists have found that the luminous vapour of every element has its own spectrum of bright lines, in other words its own *chord* of light. But when white light is shining through the vapour of such an element those lines appear as dark streaks across the rainbow-tinted background of the spectrum. In other words, the *chord* belonging to the vapour, once struck down, sounds no more ; so that as the chromatic scale is swept, from end to end, the sounds belonging to the notes of that chord are wanting.[1]

We see at once then that the whole power of the new mode of research depends on the emission of light from an object. It matters not whether the object be in the laboratory of the chemist, or half a mile off, or a hundred millions of miles off, or in fine as far off as the most distant star, if we can only obtain light enough from it to form a distinct spectrum, we can tell what is its nature.

[1] See the Introductory Essay on 'The Gamut of Light.'

Therefore it is of incalculable importance to the science of spectroscopy that it should have powerful light-gathering instruments placed at its disposal. We have seen that the Rosse telescope is by far the most powerful light-gathering instrument in the world.

But there was a difficulty. The spectroscopic observation of a celestial object is an operation of the utmost delicacy. Without entering into details which would only perplex those who are unfamiliar with the subject, and would be of no service to the practical observer who may read these pages, it may suffice to remark that the light from a celestial object must be made to fall upon a minute slit between two knife-edges, before being subjected to the analysis of the prism. Now if we suppose a telescope to be so directed that a star's light falls in the manner required, this state of things only continues for a second or two, because the earth's rotation immediately shifts the telescope's axis. Clock-motion is wanted to counteract the effect of the earth's rotation ; and in every well-appointed observatory the necessary mechanism is applied to the telescope, so that an observer may watch a star for any length of time he pleases without having occasion to touch the tube of his telescope.[1]

[1] I may narrate here an amusing circumstance which occurred some years since at a celebrated observatory in the suburbs of London. A visitor was desirous of observing a celestial object which was nearly overhead, and having the run of the observatory at the moment, he directed the telescope towards the star, set the clock-work in motion, and placed himself on his back in the observing-frame attached to the floor of the observatory. This frame is so constructed that the observer can fix the head-rest in any position, and as the whole frame revolves

But while this is a comparatively simple affair when ordinary telescopes are in question, the case is different when the telescope to be moved has a tube full forty feet in length, and weighing (with the great mirror) several tons. To sway such a tube with the steady equable motion which alone would be of any use, and without setting up vibratory tremors sufficient to render any delicate observation impossible, was a task sufficient to tax the fullest powers of modern science. The work also involved an enormous outlay.

The task has been achieved, however; and already a number of interesting results have been obtained. But the application of spectroscopic analysis to the celestial objects is a process requiring time, and it is to the future that we are to look for the fruits of this part

round an upright in the middle of the observatory-floor, it is easy to place the frame so that the observer can look in perfect comfort at any object on the celestial vault. In the present instance, as we have said, the observer lay on his back, the object being nearly overhead. But while the frame remained, of course, at rest, the clock-work was slowly driving the telescope after the star; and as the star happened to be approaching the point overhead, the eyepiece of the telescope was being brought continually lower and lower. Intent on observing the aspect of the star (a celebrated double) our astronomer failed to notice that this movement of the eyepiece was gradually imprisoning him; for, his head was fixed by the head-rest, and the eye-tube was beginning to press with more and more force against his eye. The telescope was a very heavy one, and the very slowness of the movement made it irresistible, while the observer's position prevented him from helping himself. Fortunately his cries for assistance were quickly heard, the clock-work was stopped, the head-rest lowered, and the prisoner released; otherwise he would undoubtedly have suffered severely. He would, in fact, have had as good reason to complain of the telescope as the celebrated astronomer Struve had in the case of the Pulkova refractor, 'which,' Struve said, 'was justly called a "refractor," since it had fractured and presently refractured one of his legs for him.'

of the telescope's new work. In the remainder of this paper we may confine our attention to the remarkable discovery already incidentally alluded to, which has been the first-fruits of the recent change.

Astronomers and physicists have long been in doubt whether we receive any heat from the moon. Attempts have been made to concentrate the lunar beams by means of lenses, and so to render their heating effects perceptible. But though De Saussure and Melloni have, in turn, announced that they had detected warmth in the lunar rays, it has been shown conclusively by Tyndall and others, that no faith whatever can be placed in the experiments hitherto conducted. Indeed, Tyndall remarks, that all attempts to concentrate the moon's heat by means of lenses must inevitably fail. 'Even such heat-rays as reach the earth,' he remarks, 'would be utterly cut off by such a lens as Melloni made use of.' Then he adds, significantly, 'it might be worth while to make the experiment with a metallic reflector, instead of with a lens. I have myself tried a conical reflector of very large dimensions, but have hitherto been defeated by the unsteadiness of the London air.'

If any confirmation of the former of these remarks were needed, it would be found in the failure of Dr. Huggins to obtain any evidence of lunar heat by means of the same appliances which had afforded the clearest possible evidence that heat reaches us from the fixed stars. The rays of the star Arcturus, concentrated by means of Dr. Huggins's fine refractor upon the face of the heat-measuring instrument called the thermopile, immediately moved the indicator-needle in a perceptible manner. The rays from the moon, on the contrary, notwithstanding her immensely superior light, produced no signs whatever of the existence of heat.

It is evident that with its new driving apparatus the Rosse telescope was the very instrument for attacking this difficult problem. Accordingly, arrangements were made for receiving the rays of the moon after concentration by the great six-feet mirror upon the face of a very delicate thermopile. When this had been done, and after every precaution had been adopted for preventing misconception as to the true cause of any deflection of the needle, the evidence which had been so long desired was at length obtained. The needle moved sensibly under the influence of the moon's warmth; and for the first time in the history of science, we are at length able to affirm positively, that the earth receives a sensible amount of heat from her satellite.

Lord Rosse has even been able to form an estimate of the relative amount of heat we receive from the moon and from the sun. He states, as the result of his observations, that the radiation from the moon is about the 900,000th part of that from the sun.

But perhaps the most interesting result of the inquiry is the determination of the actual heat of the moon's surface at the time of full moon, or rather at lunar midday. By comparing the heat received from the moon with that derived from certain terrestrial sources of heat, Lord Rosse finds that the moon's surface must be heated to a temperature of about five hundred degrees Fahrenheit, or nearly three hundred degrees above the boiling point!

Nor is this result, startling as it seems at first sight, to be greatly wondered at, when we remember the circumstances under which the moon's surface is exposed to the solar rays. Fancy a day a fortnight long; not as in our polar regions with a sun only a few degrees above the horizon even at midday, but with an almost vertical sun for several days in succession. We know the intensity of the heat which prevails at noon in tropical countries; but that heat is a mere nothing compared with that which must prevail when, instead of a few hours, the sun hangs for five or six days close to the zenith, and pours down his rays on a surface unshielded by any atmosphere. And with respect to the effects of an atmosphere, let us not be misunderstood. It is well known that the intense heat of the tropical *climate* is not tempered, but increased by the density of the atmosphere. On the Himalayan slopes, several thousand feet above the level of the sea, an endurable if not a pleasant climate can be found, because of the rarity of the air. But the direct rays of the sun are hotter—paradoxical as it may sound—on the snow-covered summits of the Himalayas than at the sea-level. Those who have travelled over snow-covered mountains in summer know well that, while the air may be cool and refreshing, the sun will be peeling the skin from hands and face incautiously exposed to his rays.

Thus it is doubtless on the moon's surface, except that all the effects of the sun's heat are intensified through the tremendous length of the lunar day and the absolute absence of any lunar atmosphere. Indeed, Sir John Herschel, from theoretical considerations, was long since led to anticipate the results of Lord Rosse's researches. He remarked that 'the surface of the full moon exposed to us must necessarily be very much heated, possibly to a degree much exceeding that of boiling water.'

The question of the moon's habitability by such creatures as exist upon the earth is, of course, finally disposed of by Lord Rosse's discovery. We could not live conveniently at the temperature of boiling water, nor could any beings known to us. The famous salamander, even if it had all the properties assigned to it in olden times, instead of being one of the most cold-loving of all known creatures, would find the moon an unsatisfactory residence. For tremendous as is the heat of the lunar midday, the cold of the lunar night must be still more terrible. It has been well remarked by Tyndall that were it not for the moisture with which our atmosphere is laden, the cold of a single night would bind our fields in a Siberian frost. Imagine then the effects of a night of three hundred hours in a region where there is neither moisture to form protecting envelopes of cloud or mist, nor an atmosphere to support such envelopes even if they could be formed. Doubtless the cold of the lunar night is of an intensity such as not even the most ingenious appliances of our chemists could produce. Under its influence, not merely would all known liquids be frozen, but probably every gas known to us would be converted into the solid form.

And I may notice, in passing, by how many strange and bizarre theories astronomers have endeavoured to account for the fact that the moon has no appreciable atmosphere. At least four views have been put forward. There is, first of all, the theory that the moon has always been without an atmosphere. Then there is the theory that the moon's atmosphere has all retired to that side of our satellite which is always concealed from us. Thirdly, there is the theory that the oceans and atmosphere which once rendered the moon a fitting abode for living creatures, have retired within the interior of the moon's crust. Lastly, there is the theory that the oceans on the moon's surface first became frozen as the moon gradually parted with her internal heat, that next her atmosphere began to yield to the intensity of cold, and changing first to the liquid and then to the solid form, became no longer recognisable as an atmosphere by our astronomers.

Perhaps Lord Rosse's recent discovery seems more decidedly opposed to the last of these views than to any of the others. The notion of a frozen mass of oxygen or hydrogen under the influence of a heat more than three hundred degrees higher than that of boiling water seems bizarre in the extreme. Yet, after all, it is almost impossible for us to conceive what would happen when there is no appreciable atmosphere to prevent the immediate radiation of heat into space. We know that the snows on the summits of the Himalayas show no traces of diminution under the full heat of the vertical sun of India. Yet the air around those snows is absolutely dense when compared with that which exists (if any at all exist) upon the moon's surface.

Then again, we may look at the matter in another light. Whatever effects are to be ascribed to the heat of a lunar day cannot do more than counterpoise the effects which must be ascribed to the cold of the long lunar night. During the whole twenty-eight days the moon receives no more heat (in proportion to its surface) than the earth does in the same time, though the mode in which the heat is received in either case is very different. Now Professor Tyndall has shown us how nature stores up heat, and how she also stores up cold (to use a somewhat inexact but convenient mode of expression). It is with the latter process we are here concerned, and a very simple illustration will suffice to exhibit the nature of the case. If we subject a quantity of aqueous vapour to the action of intense cold (still our mode of expression is inexact but convenient for our purpose), the vapour parts with as much heat as it can *without* changing, but is presently compelled to change to the liquid form, a process during which it parts with a large quantity of heat; then the liquid repeats the process, parting with as much heat as it can without changing form, but being presently compelled to change to the solid form, a process during which it parts with another large stock of heat. Now when we come to subject the ice thus formed to the action of heat, the processes just described are reversed, and before we can restore ice to the state of water we must employ a large quantity of heat without any apparent heating effect; and we must do the same before we can restore the water to the state of vapour. *Then* only will the addition of further heat raise the vapour to a higher temperature than it had when we began. Nature had not only unwound the spring, so to speak, but had carefully wound it the reverse way, and in reversing the process we have to unwind before rewinding, and to rewind before winding the spring to a higher tension than it had at first.

We see at once, then, that the intense heat of the moon's surface does not by any means imply that, if there were much ice on the moon's surface it would all melt beneath the sun's action, still less that the water thus formed would all be converted into vapour. The intense cold of the long lunar night would have so thoroughly wound the spring the reverse way that all the heat of the long lunar day would be insufficient to unwind it.

We know so little, however, of the results which would follow from such a state of things as exists at the moon's surface that it would be unwise to speculate further on these and similar points. Lord Rosse's discovery gives us good hope that more may yet be learned respecting our satellite, and that thus an answer may be obtained to many questions of interest which hitherto it has seemed useless to inquire into. New modes of research seem to be revealing themselves to our physicists. On every side new labourers are entering the field of scientific inquiry; and each day our men of science are giving fresh proofs of zeal and industry. The very work we have been considering, the addition of motive power to the once inert mass of the great reflector, is even more encouraging from the proof it affords of the disinterested regard which the men of our day feel for scientific interests, than from the immense material aid which it brings to the new modes of physical research.

Fraser's Magazine for December, 1869.

THE PLANET OF LOVE.

THE contrast between Venus and Jupiter (two orbs which often seem so strikingly alike that only their position distinguishes one from the other) is in reality most complete. It is difficult even for the astronomer to realise the fact that of these orbs one is thirteen hundred times larger than the other, that the surface of the lesser is illuminated some fifty times more brilliantly than that of the farther and greater. It requires, too, a strong effort of the imagination to picture to oneself that one orb is solitary, like Mars or Mercury, while the other is circled round by ~~four~~ FIVE orbs, ~~the least of which is as large as our moon~~.

It may be interesting to consider some of the facts which astronomers have learned respecting the beautiful planet which appropriately bears the name of the loveliest of the heathen goddesses. There is much, indeed, in what is known about Venus which rather tends to disappoint than to satisfy the questioner; much also which is more fitted to invite speculation than to afford any basis for sound theorizing. When we compare what has been learned about Venus with the detailed information which the telescope has given us respecting Mars, or with the grand phenomena whose progress has been traced in the distant orbs of Jupiter and Saturn, we are apt to feel astonished that the planet which approaches us most nearly should have revealed so little, even under the most searching scrutiny. Yet it is only by comparison with what has been learned about these most interesting orbs, that our information about Venus seems small in amount. In reality there is much which will very well repay our attention, more especially when we consider Venus not merely with reference to what the telescope teaches us respecting her, but also in relation to her position in the scheme of worlds circling around the sun.

It used to be supposed that Venus is rather larger than our own earth. But more careful measurements made in recent times have shown that she is in all probability considerably smaller than the earth. A circumstance had tended to deceive the earlier telescopists. Venus shines with such exceeding brightness as to appear larger than she really is. The fact that bright objects are thus seemingly enlarged is doubtless familiar to most who read this paper. It is strikingly illustrated by the appearance which the new moon presents when the unenlightened half of her globe is visible, or when 'the old moon is in the new moon's arms.' The dark part appears to belong to a smaller globe than the bright crescent; yet in reality of course the effect is but an optical illusion. Indeed, quite recently astronomers had to reduce their estimate of the moon's size on account of the very effect I am here referring to.

In the case of Venus the effect is, of course, more remarkable, especially when considered as affecting Venus's bulk; for she shines much more brilliantly (though of course giving out very much less light altogether) than the moon; and being so much farther away, the same amount of seeming extension outwards corresponds in reality to a much greater error in the estimated diameter. Thus it happens that in Ferguson's 'Astronomy' we find the diameter of Venus set down at 7,906 miles, while Sir W. Herschel and Arago set it at 8,100 miles; whereas the estimate now generally regarded as most trustworthy assigns to her a diameter of only 7,500 miles. Thus her estimated bulk has been very considerably diminished; for though her diameter has been reduced but by about one-sixteenth part from Ferguson's estimate, it is easily calculated that her volume has been reduced by fully a seventh part—in which degree, also, it falls short of the earth's. Her surface, which is perhaps a more important feature when we consider her as the probable abode of living creatures, is less than the earth's in the proportion of about nine to ten.

Still, it is hardly necessary to point out that these differences are very slight when compared with those which distinguish the other planets of the solar system from our own earth. Mars, with his diameter of but 4,500 miles, on the one hand, and Uranus, with a diameter of more than 35,000 miles, on the other, seem startlingly unlike our earth after the relations of

Venus have been considered ; and yet they come next to her in this respect. We have to pass from Mars to small Mercury and the asteroids in following the descending scale of magnitude, and from Uranus to Neptune, the ringed Saturn, and the mighty mass of Jupiter, in following the ascending scale. In the whole range of planetary bodies, from Jupiter, more than twelve hundred times bulkier than our earth, down to the least asteroid—a globe, perchance, not larger than Mr. Coxwell's balloon—we meet with not one orb which can be regarded as our earth's twin-sister world, save that globe alone whose glories now illuminate our evening twilight skies.

In one respect only the comparison fails. Unlike our earth, Venus has no moon. I shall not enter here into a consideration of the very singular circumstance that many observers, and some of them not unknown for skill and clear-sightedness, have declared that Venus has a moon, and that they have seen it. Astronomers are now agreed that these observers were deceived, and I suppose little doubt can remain in the minds of all who are competent to weigh the evidence, that Venus has no satellite. Still there are few chapters in the history of Astronomy more suggestive than that referring to the supposed discovery of a secondary orb, which has, in reality, no existence. Sir William Herschel's temporary belief in the existence of two rings at right angles to each other around the planet Uranus, can by no means be compared with the strange deception which deluded observers in the case of

Venus. For Uranus is so far off that his phenomena are seen only with extreme difficulty; and the telescope with which Sir William Herschel chiefly studied the planet was notoriously imperfect as a defining instrument, notwithstanding its wonderful light-gathering power. It 'bunched a star into a cocked hat' we are told, and in effect it *made* the rings round Uranus which for a time perplexed the great astronomer. But in the case of a planet so near to us, and so bright as Venus, one would have thought an optical illusion, such as the telescopic creation of a satellite, was wholly impossible. Here was an orb of which its observers felt able to say that its diameter was about one-fourth of Venus's, its light slightly inferior to hers in brightness, and its seeming shape horned, or gibbous, exactly as her own at the time of observation. And yet that orb was a mere moon-ghost, an unreal telescopic vision.

We shall inquire farther on, however, whether the want of a moon necessarily renders the skies of Venus at night dark and gloomy by comparison with ours, or, at least, with our moonlit nights.

The chief difficulty which the telescopist meets with in trying to examine the surface of Venus arises from the excessive brightness with which she is illuminated. Of course, I am not here referring to that splendour which the unarmed eye recognises in her light. Jupiter, when seen on the dark background of the midnight sky, shines with a splendour fairly comparable with that of Venus; and yet rather the defect than the excess of light is what troubles the astronomer in the case of Jupiter. I am referring now to the intrinsic brilliancy of the illumination of Venus's surface—this brilliancy depending on her nearness to the sun. The degree of her brightness may very well be illustrated by an example. Suppose the side of a hill to be so sloped that the sun's mid-day rays fall square upon it. Now, if the slope is covered with white sand, it will shine rather less than half as brightly to the eye as the disc of Venus.[1] But we know how dazzling white sand looks when the sun shines full and squarely upon it; so that it will readily be conceived that the disc of Venus tests the performance of even the best telescopes. For it is to be noticed that although the astronomer can cut off a part of the light by suitable contrivances, yet these must needs impair to some degree the clearness of the definition. Besides, some features may be wholly obliterated by any contrivances for reducing the planet's lustre, precisely as the dark glasses used in observing

[1] This is easily proved. We may be certain that the reflective capacity of Venus's surface is not less than that of the surface of the ruddy Mars. Now Zöllner has shown that Mars reflects rather more of the sunlight which falls on him, than he would if he were a globe of white sandstone. Supposing Venus to do likewise, then as she is so near to the sun as to receive twice as much light as the earth does (surface for surface), her disc must look rather more than twice as bright as white sandstone fully and squarely illuminated. In all such cases (be it noted in passing) distance has no effect. Distance may diminish the brightness of objects seen through air, or other imperfectly transparent media; and of course distance diminishes the total quantity of light received from an object. But distance in no way affects the intrinsic lustre of bodies seen through vacant (or practically vacant) space.

the sun blot from view altogether the coloured prominences which really surround his disc.

But although Venus is thus rendered a difficult object of study, there is one feature in her telescopic aspect which seems to place it in the power of observers to learn more about her surface-contour than even about the details of the planet Mars. Venus travels on a path inside the earth's. Hence she lies, at times, nearly between the earth and the sun, so that her dark half is turned towards us; while at other times she lies directly beyond the sun, so that her illuminated half is turned towards us. Obviously in one case she is presented as the moon at 'new,' while in the other she is as the moon at 'full;' nor does it need much consideration to show that, in passing from one phase to the other, she must exhibit all the changes of aspect which we recognise in the moon. With, however, *this* farther peculiarity, that whereas the moon remains always of about the same seeming size while passing through her phases, Venus, on the other hand, changes most notably in size, as seen in the telescope. When she is directly beyond the sun her distance from us is 66 millions of miles greater than the sun's, or about 157 millions of miles in all. When she is directly between us and the sun, her distance falls short of his by 66 millions of miles, or is reduced to about 25 millions of miles. Her distance in the latter case is less than one-sixth of that which separates her from us in the former; and her disc is more than 36 times larger. So that as she passes from new to full she is at once crescent and waning. Her orb is becoming larger and larger, while a continually diminishing proportion of it is illuminated. In passing away from full to new she decreases in seeming size, while waxing in the sense in which we use the term when speaking of the moon. The reader will doubtless remember how the discovery that Venus actually changes thus in seeming magnitude and phase was among the earliest which Galileo effected by means of the telescope. That his priority might not be questioned he announced the discovery in the following sentence—'Hæc immatura a me jam frustra leguntur, d.y.,' which is very bad Latin for the statement that 'These matters still immature, and as yet (studied) in vain, are read by me.' Four months later he published the key to the anagram in the following much more elegant piece of Latinity: 'Cynthiæ figuras æmulatur Mater Amorum,' or 'Venus, the Mother of the Loves, imitates the changing figures of the moon.'

Now when Venus presents her full face towards us she is much too far off to be well seen, and besides she lies directly beyond the sun, and his light prevents us from seeing her. On the other hand, when she is nearest to the earth, her dark hemisphere being turned towards us, she would be invisible even were she not in this case also lost in the sun's light. When she is best seen she presents much less than a full disc; and, in fact, she is actually best placed for study when showing a crescent phase, somewhat like the moon's two days before she is half full.

At first sight it might seem that this should render the study of Venus even more difficult than any of the circumstances yet named. The central part of her disc, just that portion which is alone unforeshortened, can only be seen when Venus is much farther off than Mars is at his nearest—when, also, he is most favourably seen in other respects; while the portion seen when Venus is nearer is seen edgewise, and therefore very unfavourably placed for study.

But in one respect there results a means of studying Venus which is wanting in the case of Mars. I refer to that very means whereby astronomers have been able to measure the height of the lunar mountains. The boundary between the light and dark parts of the moon is the region where, as seen from the moon, the sun is rising or setting. The mountain tops near that boundary catch the sun's light earlier in the lunar morning, and later in the lunar evening, than the plains and valleys close around. Precisely as the traveller who views the phenomena of sunrise from the summit of the Rigi or Faulhorn,[1] sees the valleys still enshrouded in gloom, while the mountain tops are all illuminated; so out yonder, on our satellite, if there are living creatures there, contrasts of like sort, but much more marked, may be witnessed by such Lunarians as care to climb the summits of the peaks around

[1] One is willing to believe that there are travellers who have been so fortunate.

such craters as Tycho, Kepler, and Copernicus. The telescopist can see the lunar mountains lit up by the sun's rays, when the valleys around are in darkness; for, outside the boundary line between the light and the dark portions, he sees spots and streaks of white light, which he recognises as the peaks of lunar mountains, or the summits of mountain ranges. And by measuring the distance at which a lunar peak which has just caught the light, lies from the boundary between light and darkness—or, as one may say, by measuring how far off the tiny island of light is from the shore-line—he estimates the height of the lunar mountains.

In Venus, similar phenomena are presented. Only her greater distance renders it less easy to study them to advantage. Of course if the planet were a perfectly smooth globe the boundary between the light and dark portions would be quite smooth and uniform. But as early as the year 1700, La Hire could recognise irregularities in the boundary, when the crescent was very narrow. But we owe to the German astronomer, Schröter, the first satisfactory study of these irregularities. Towards the close of the last century he studied the planet with several powerful telescopes; and he was able to recognise distinct inequalities in the boundary. These irregularities varied in figure from time to time, precisely as they might be expected to do when we consider their cause. Now a plain or sea, now a high table-land, would be at some particular part of this border-land between light and darkness;

now valleys, now mountain peaks would diversify the seeming figure of the boundary. Some of the effects recognised by Schröter were so remarkable as to suggest that the mountains on Venus must be very much higher than those on our earth. Schröter, indeed, estimated the height of some of these mountains at no less than twenty-eight miles, or fully four times the height of the loftiest peaks on our own earth.

A circumstance of some interest may be here touched upon in connection with the researches of Schröter. Sir William Herschel, having failed with his more powerful telescopic means in detecting any of the appearances recorded by Schröter, wrote a somewhat lively criticism upon Schröter's statement. Of this paper, which appeared in the 'Philosophical Transactions' for 1793, Arago remarked that it was 'une critique fort vive, et, en apparence du moins, quelque peu passionnée.' It must be said, however, in justice to the greatest telescopist who has ever lived, that the severity of his tone, though not justified by the actual circumstances, was by no means unwarranted by the facts as he saw them. Misapprehension, not injustice, led to the warmth of his expressions. Schröter answered dispassionately and effectively in 1795; and no doubt now remains of the general accuracy of the German astronomer's observations.

The irregularities whose effects thus show themselves by notching or otherwise distorting the boundary between the light and dark portions of the disc of Venus, have been detected also as faint spots within the illuminated portion of the disc. It is only, however, with great difficulty, and under exceedingly favourable circumstances, that they can be so seen. And singularly enough, it would by no means appear as though the most powerful telescopes, or even the greatest observing skill, were the necessary conditions for the detection of these spots. On the contrary, they have been seen with small telescopes when large ones failed to show them; and comparatively inferior observers, like Bianchini and De Vico, have recognised them, when Sir William Herschel and the eagle-eyed Dawes have been unable to detect any traces of their existence. Indeed, all that Sir William Herschel could detect was a slight superiority of brightness in the part of the disc near the edge as compared with the part close by the boundary-line between the bright and dark portions. This peculiarity he misinterpreted strangely; for he ascribed it to the existence of an atmosphere in Venus, failing to notice that it is clearly recognisable in the airless moon.

The spots in Venus are not seen distinctly enough to enable us to judge whether they indicate the existence of land and water, like the greenish and the ruddy markings on Mars. But they have enabled astronomers to measure the rate at which Venus turns upon her axis, and they have also shown us how her axis is placed, so that we can form an opinion as to the nature of her seasons.

Cassini was the first to time the rotation of Venus. He found that a certain spot returned to the same place on her face at intervals of about 23 hours, so that the length of the day in Venus appears to be slightly less than that of our own day. But Bianchini, in 1726, came to a very different, and a very startling, conclusion. He said he could not account for all the changes of appearance he had noted in Venus, without assigning to her a rotation-period of 24 days and about 8 hours. Cassini had not been certain about his results, because he could not follow the spot far across the face of Venus. Bianchini's results were open to a somewhat similar objection. His observatory had not sufficient sky-room to enable him to follow the planet for more than about three hours. Now he was convinced that the spots did not appreciably change their place in that time; and having made his observations at somewhat wide intervals, and finding that at the end of several days a spot seemed considerably advanced when observed at the same hour of the night, he concluded that all those days had been occupied in this advance *alone*. Cassini had judged that each day there was a circuit and a slight advance *as well*.

That excellent astronomer, Ferguson, whose book (out of date as it is) continues far better worth studying than nine-tenths of our modern elementary treatises on Astronomy, adopted Bianchini's explanation as seeming to accord best with the evidence. Working out the consequences after his usual sound and laborious fashion, he came to some very strange conclusions respecting the seasonal changes in Venus. Bianchini had seen reason to believe that Venus turns on an axis very much tilted down towards the level of her path round the sun; and the effects of this tilt would be very striking, even though the day of Venus were judged to be equal, or nearly so, to our own. But with the long day of 584 hours (24⅓ terrestrial days), the resulting effects were found by Ferguson to be so strange that nothing we are familiar with on earth could be very well compared with them.

In the first place (according always to Bianchini's estimate) there are but 9¼ days in the year of Venus.[1]

[1] In my 'Other Worlds' there is a note referring to a remark in Admiral Smyth's 'Celestial Cycle' which had perplexed me. For the Admiral says that in the year of Venus there are but 9¼ of her days, 'reckoned by the sun's rising and setting, owing to which the sun must appear to pass through a whole sign in little more than three quarters of her natural day.' In the note referred to, I remark on this, 'he gives no reason for this remarkable statement, which most certainly is not correct.' I might well, indeed, be perplexed, not only by this particular statement, but by Admiral Smyth's whole treatment of the seasonal and diurnal changes in Venus. For though he nowhere adopts Bianchini's estimate of Venus's rotation-period (on the contrary, he remarks that Schröter's researches have established Cassini's value), yet none of his statements are just if Venus turns round in about 24 hours. I have recently found that all Admiral Smyth's remarks on the seasonal and diurnal changes in Venus were founded on Ferguson's examination of the matter. So that their incongruity is at once accounted for. But it is worthy of notice how important it is that no statement—however eminent its authority—should be repeated without due examination, or failing that (as may well happen when a subject is very recondite), a careful reference to the source whence the statement has been drawn. Admiral Smyth doubtless thought that so accurate a writer as Ferguson could not go wrong, and so, neglecting inquiry, failed to notice that he was himself misinterpreting Ferguson. On the other hand, I was somewhat sharply censured for questioning the dicta of so sound a mathematician as the esteemed Admiral; yet it is now shown how necessary such questioning was in that instance. But in truth it is always so. Doubt in such matters ought to be held as an absolute duty by the scientific writer.

'We may suppose,' says Ferguson, 'that the inhabitants of Venus will be always careful to add a day to some particular part of every fourth year, by means of which intercalary day every fourth year will be a leap-year, and will bring her time to an even reckoning, and keep her calendar always right.'

Then, the day lasting so long, the sun's midday height would be very different on successive days; so that if at any place he were overhead at noon on one day, he would be found far removed from the point overhead at noon of the next day. 'This appears to be providentially ordered,' says Ferguson, 'for preventing the too great effects of the sun's heat (which is twice as great on Venus as on the earth); so that he cannot shine perpendicularly on the same places for two days together; and on that account the heated places have time to cool.' One would have thought the long night of 292 hours would fairly have sufficed for this desirable purpose; but in Ferguson's day men knew more about the final causes of things than we do in our time, so that it is only with extreme diffidence that I venture this suggestion.

When Ferguson wrote, the astronomers of England were paying great attention to the problem of finding a ship's longitude at sea. Ferguson points out how much better off the people in Venus are as respects their means of dealing with this problem. 'The sun's altitude at noon being very different at places in the same latitude, according to their different longitudes, it will be almost as easy to find the longitude on

Venus as it is for us to find the latitude on our earth, which is an advantage we can never have.' Here is another instance of an easily interpretable design. For our seamen have the moon to help them in finding the longitude; and the voyagers over Venus would be badly off without a moon but for the peculiarity pointed out by Ferguson.

But it is as well, before inquiring what purpose was intended to be fulfilled by certain relations, to assure ourselves that those relations exist. For example, before asking why the people in Jupiter and Saturn get so much more moonlight from their many moons than we do from our single one, it is as well to calculate how much light they do actually get; because the argument from design is slightly interfered with when the multiple moonlight in Saturn and Jupiter is found to amount in all to scarce a twentieth of that which our single moon supplies to us. So here, in the case of Venus, it is unpleasing, after calculating all the important advantages afforded by the long day of Venus, to discover that the day of Venus is actually rather less than our own.

This, however, has now been abundantly proved. Schröter, by carefully noting the interval which elapsed between the successive appearances of a certain bright spot close by the southern horn of the crescent Venus, assigned a rotation-period of 23 days 21 minutes and 8 seconds. This was within a minute of the time which had been assigned by the younger Cassini as bringing his father's observations into agreement with Bianchini's. But the Italian observer, De Vico, attacked the question still more earnestly. He and several colleagues studied Venus at the Observatory of the Collegio Romano. They rediscovered Bianchini's spots, and by carefully comparing their own estimate of the planet's rotation with the observed appearance of Venus at such and such hours, as recorded by Bianchini, they were able to deduce a very close approximation to the rotation-period of Venus. They assigned as the actual length of the day in Venus 23 hours 21 minutes 23 seconds and 93 hundredth parts of a second. Without accepting these hundredths as altogether beyond dispute, we may take 23 hours 21 minutes and 24 seconds as doubtless very closely representing the value of Venus's rotation-period.

Here, then, we have a day closely corresponding to that of our own earth, and also to that of Mars. In fact, the day of Venus falls short of our earth's day by about as much as the day of Mars exceeds our earth's. Instead of the year of 9¼ of her own days assigned to Venus by Bianchini, we find that she has a year of about 230 days. There is little reason, then, thus far, for supposing that the seasonal and diurnal changes in Venus differ importantly from those on our own earth.

But undoubtedly, when we inquire into other circumstances on which the seasons and general climate of a planet must depend, we find some difficulty in regarding Venus as likely to be a quite agreeable abode for creatures constituted like ourselves. Before discussing these relations, however, let me as an anticipatory corrective present the enthusiastic description which Flammarion has given of that which he can have seen only with his mind's eye, and that eye gifted with exceptional, and possibly deceptive, powers. 'Some ill-disposed minds,' he says, as translated by Mrs. Lockyer, 'have asserted that although Venus is beautiful afar, it is frightful on a nearer view. I fancy I see my young and amiable readers; and I am sure that not one amongst them is of this opinion. Indeed, all the magnificence of light and day which we enjoy on the earth, Venus possesses in a higher degree. Like our globe, it is surrounded by a transparent atmosphere, in the midst of which are combined thousands and thousands of shades of light. Clouds rise from the stormy ocean, and transport into the sky snowy, silvery, golden, and purple tints. At morning and evening, when the dazzling orb of day, twice as large as it appears from the earth, lifts its enormous disc at the east, or inclines towards the west, the twilight unfolds its splendours and charms.'

This is very pleasant to contemplate; but it is desirable to inquire how far it is warranted by known facts.

To begin with the excessive light and heat which the sun pours upon Venus. I suppose no one doubts that quite possibly this great light and heat may be so tempered as to be not only endurable, but pleasant to people in Venus. But so far as terrestrial experience is concerned, we are assuredly not justified in saying that this *must* be so. Undoubtedly, if the sun began suddenly to pour twice as much light and heat upon the earth as he actually does, the human race would be destroyed in a very few months. In tropical regions the destruction would be completed in a single day. In temperate regions the beginning of the first summer would be fatal. Nor would the denizens of arctic and subarctic regions live through the heat of a midsummer's nightless day.

Suppose, now, we assume that the atmosphere of Venus, as good observers have judged, is considerably deeper than our own. This we may fairly do, because certainly the estimate of observers would be more likely to fall short of the truth than to pass beyond it; so that, when trustworthy astronomers say that they have seen the twilight zone of Venus extending farther than we know our own does, we may fairly conclude that at a nearer view a yet greater extension of this sunlit atmosphere—for such is the real nature of the source of twilight—would be greater yet. Here, again, all that we know of the effects of a deep atmosphere would lead us to believe that the heat in Venus must be intensified by the action of her deep and dense atmosphere. As a matter of fact, it may not be so. All I urge is, that, judging from the only analogy we have to guide us, the depth and density of the atmosphere of Venus seem to promise no relief from the intense solar heat to which she is exposed.

But it is when we consider the effects of her axial slope that we find the most urgent reasons for doubting how far life would be comfortable to ourselves in that beautiful planet which so often adorns our twilight skies.

Bianchini believed in an amount of axial tilt (a tilt of the axis, that is, from uprightness to the path of Venus) which has not been confirmed by De Vico and his colleagues. Still their observations agree in assigning an axial tilt much more than twice as great as the earth's. In other words, the arctic regions in Venus extend more than twice as far from her poles as ours do, and her tropical regions extend more than twice as far as ours from the equator. But we have only to take a terrestrial globe to see that, if we extend more than doubly the range of the tropics and of the arctic regions, these regions will overlap. There will be no temperate zone at all. Instead of it, there will be a region which is both tropical and arctic.

Now, when we remember what is meant when we speak of a region as tropical or arctic, the significance of this statement will be recognised. At a place within the tropics the sun is always twice in each year immediately overhead at noon. At a place within the arctic regions there is always one period in the year when the sun does not rise, and another period when he does not set, all through the twenty-four hours.

Conceive, then, first, the vicissitudes within the zone which is both arctic and tropical. Here we have, at one season, an arctic night—no sun shining all through the twenty-four hours; at another an arctic day—the sun not setting during all those hours. Between these seasons, but nearer to the latter, we have two seasons, when the sun is overhead at noon. The contrast between the bitterness of a season when the sun does not show at all, and the fiercely scorching heat of seasons when either the great sun of Venus does not set, or shines vertically down at noon upon such beings as may be able to endure his fury, is certainly not a pleasant prospect for terrestrial beings to contemplate. The young lady whom Flammarion lauds because she promised 'swiftly to soar to Venus' when her 'imprison'd soul was free,' would have been justified in declining the visit, on the score of expediency, while still encumbered with a body. And if 'now,' as Flammarion suggests, 'she resides in that isle of light, and contemplates thence the earthly abode which she not long ago inhabited, perhaps she hears,' not without amusement, 'the prayers of those who, as she did formerly, allow their hopes to mount sometimes' to those pleasant-looking regions.

Nor are the tropical or arctic regions more likely to be comfortable abodes for creatures constituted like ourselves. The seasonal contrasts and vicissitudes in these regions are always very marked, and recur much more rapidly than on our own earth. If the arctic regions are worse off in having a more marked difference between the greatest heat of the summer and the greatest cold of winter, the tropical regions are worse off in having two summers and two winters within the short year of two hundred and twenty-seven terrestrial days.

I cannot but think that on a fair examination of the physical habitudes of Venus, we are led rather to Whewell's than to Brewster's opinion; though I am by no means ready to admit that either one or the other opinion is strictly sound. It is but barely possible, if possible at all, that Venus may be a suitable abode for creatures like ourselves and our fellow inhabitants of this terrestrial globe. But we have no sufficient reasons for believing with Whewell that creatures so constituted as to exist in comfort in Venus must needs be wholly inferior to those which inhabit the earth.

One word on the celestial scenery visible from Venus. It is a circumstance worth noticing that from all the three planets which have no moons, at least one orb can be so seen as to appear more beautiful than any star or planet in our own skies. Jupiter, as seen from Mars, must appear a most noble orb, since his splendour, owing to the greater proximity of Mars (when most favourably situated for observing Jupiter), must be one half greater than that which he displays to ourselves. His satellites, too, may probably be visible from Mars. In the planet Venus, again, the Mercurials have a noble spectacle. Her lustre, indeed, when seen under the most favourable circumstances, must illuminate the skies of Mercury with a splendour surpassing ten or twelve times that of the planet Jupiter as we see him on a midnight sky. From Mercury also the earth must seem a noble orb, her attendant moon being probably distinctly visible. Venus has not, like Mercury, a view of two planets surpassing Jupiter in splendour. But, on the other hand, the earth as seen from Venus must be the most beautiful spectacle visible throughout the whole range of the solar system. To vision such as ours the earth must present the figure of a disc, because we know that under favourable circumstances we can ourselves recognise the crescent form of Venus with the unaided eye. This disc cannot fail to exhibit varying colours; now appearing greenish, now reddish, according as the terrestrial seas or oceans are more fully turned towards Venus; while at times, when the atmosphere of our earth is heavily laden with vapours, the glory of the earth as a light in the skies of Venus must be greatly enhanced, the earth's lustre being at such times, however, purely white. In the meantime the moon must be distinctly visible, as a disc about one-fourth as large as the earth's in diameter, and not changing in colour as hers does, unless, indeed, it chances that the side of the moon we do not see differs very much in character from the portion we are able to study. The seeming distance separating the moon from the earth when they are farthest apart will be somewhat greater than the seeming diameter of the moon as we see her. It need hardly be said that the light actually received from the earth and moon under these circumstances must be very much greater than that which we receive either from Jupiter or Venus[1]

[1] The actual amount of light received from the earth and moon together, as seen from Venus, probably amounts to nearly the five-hundredth part of that which we receive from the moon at full.

when at their brightest. We know that Mars, when seen under most favourable circumstances (once in about a century), is fairly comparable with Jupiter; but at such times Mars is half as far again from us as we are from Venus. He would show a disc much less than half the earth's if both were seen at the same distance, and he is illuminated less than one-half as brightly, owing to his greater distance from the sun. On all these accounts the earth must shine many times more splendidly than Mars does, even on those exceptional occasions when (as once during the last century) his ruddy orb blazes so resplendently as to be mistaken for a new star. When it is remembered, too, that Venus is seen most brightly when by no means at her nearest, and when showing less than a half disc, whereas the earth is seen most favourably when nearest to Venus, and showing a full disc, it will be seen that the greater intrinsic lustre of Venus is much more than counterbalanced, and that the earth with her companion moon, as seen from the planet Venus, must form a far more glorious spectacle (besides appearing on a far darker sky) than the Planet of Love when most she solicits our admiration.

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THE PLANET OF WAR.

At a meeting of the Royal Astronomical Society a few years ago, a globe was exhibited by Mr. Browning, on which lands and seas were pictured as upon an ordinary terrestrial globe. By far the larger part of these lands and seas were laid down as well-known entities, respecting which no more doubt is felt among astronomers than is felt by geographers respecting the oceans and continents of our own earth. Yet the world which is represented by this globe is one which is never less than one hundred and twenty times farther from us than our own moon.

It is rather singular that the planet Mars—the orb which was represented by Mr. Browning's globe[1]—is the only object in the heavens which is known to exhibit features resembling those of our earth. Astronomers have examined the moon in vain for such features: she presents an arid waste of extinct volcanoes, dreary mountain scenery surrounding lifeless plains (the *seas* of the old astronomers); an airless hemisphere of desolation, in fact, which has no counterpart on the terrestrial globe. The planets Jupiter and Saturn, orbs which far transcend our earth in

[1] The globe was constructed from a chart of Mars formed by the present writer from Mr. Dawes' drawings of the planet.

mass and volume, which are adorned with magnificent systems of subsidiary bodies, and which seem in every respect worthy to be the abodes of nobler races than those which subsist upon our earth, afford no indications which justify us in asserting that they resemble the earth in any of those points which we are accustomed to regard as essential to the wants of living creatures. Nearly the whole of the light which we receive from these splendid orbs is reflected, not from their real surface, but from vaporous masses suspended in their atmospheres. It is, indeed, doubtful whether anything has ever been seen of the real surface of either planet, save perhaps that a small spot has here and there been faintly visible through the dense overhanging mantle of vapour. And strangely enough, the two small planets, which present in other respects the most marked contrast to the giant members of our system, resemble them in this point. Venus and Mercury seem both to be protected from the intense heat to which they would otherwise be exposed through their proximity to the sun, by densely vaporous envelopes, which only permit the true surface of the planets to be faintly seen, even under the most favourable conditions. The planet Mars, however, discloses to us his real surface, and this surface presents indications which cannot reasonably be doubted to result from the existence of continents and oceans resembling those of our own earth in all essential features. Moreover, that wonderfully delicate instrument of research, the spectroscope, has confirmed these indications in a manner which hardly suffers any further doubt to rest upon their meaning. I do not think that my readers will find a brief record of the process of discovery which has culminated in the construction of Martial charts and globes otherwise than interesting.

It does not appear that Galileo, when he applied to Mars the same telescope which had revealed to him the satellites of Jupiter, was able to detect any features of interest in the nearer planet. More than half a century, indeed, appears to have passed, after the invention of the telescope, before anything was detected which led to the suspicion that Mars has permanent markings upon his surface. In the beginning of March, 1666, Cassini, with a telescope 16 feet in length, but not very far inferior in power to many modern tubes one quarter as long, noticed features sufficiently remarkable to enable him to determine roughly the rotation-period of the planet. Not many days later our own countryman, the ingenious Hooke (who had detected spots on Mars in 1665), made two drawings of Mars which will bear comparison with all but the best modern views. These drawings were taken by means of a telescope no less than twelve yards long. At the end of the same month observers at Rome, using Divini's glasses, constructed a drawing of Mars, which aroused the wrath of Cassini: 'for,' says he, 'these observers represent the spots they saw as small, far apart, remote from the middle of the disc, and the eastern spot less than the west, whereas by observations made on the same day at Bonomia, I know that there were two large spots close to each other, in the midst of the disc, and the eastern bigger than the western.' Certain it is that Cassini deduced from his observations a nearly correct rotation-period, while the Roman observers gave a period only one half the true one, having apparently been deceived by a certain resemblance which exists between two opposite hemispheres of the planet.

In 1704–1719 Maraldi made a series of observations of Mars, and two of his drawings are easily recognisable. In one there is seen a triangular or funnel-shaped spot, running nearly north and south, which is doubtless the feature called 'the Hour-glass Sea' by modern astronomers. In the other there is an elbow-shaped spot which powerful modern instruments have broken up into two important 'seas.'

Sir W. Herschel, however, was the first who attempted a systematic examination of Martial features. His object was rather a singular one; in fact, it will hardly appear, at first sight, what relation can exist between that object and the features of Mars's surface. Herschel wished to ascertain *whether the length of our day is constant.* He considered that by watching the rotation of some other member of the solar system he might be set upon the traces of any change which may be taking place in our earth's motion of rotation. He soon found that (as has been already indicated) Mars is the only planet available for this purpose, as being the only planet whose surface bears recognisable marks.

He set himself therefore to construct a series of pictures of the planet.

Herschel was not very successful. I have heard his pictures described as 'caricatures' of Mars. Their defects are not due, of course, to any want of care or skill in this eminent observer, but to the imperfect definition of his large reflectors. These instruments, though admirably adapted for the observation of objects requiring a great degree of light-gathering power, were wanting in that extreme accuracy of definition which would alone suffice to present the surface-details of so distant an object as the planet Mars. And by a singular accident Herschel was not even successful in determining the rotation-period of Mars with the accuracy which might have been deduced from his long series of observations. In comparing views taken at an interval of two years, he accidentally *omitted one rotation*, so that the Martial day, as determined by him, was two minutes too long.

The next series of observations which deserves special comment, is that taken by Messrs. Beer and Mädler, in the years 1830–1837. They used an instrument about four inches in aperture, and rather more than five feet in focal length. With this instrument, which in less experienced hands would have been wholly inadequate for observations of such difficulty, they constructed an admirable series of views, which they subsequently combined in a 'chart of Mars.' They also obtained a close approximation to the length of the Martial day, which they found to consist of 24h. 37m. 23·8s., a result not differing much more than a second from the true value.

I pass over a number of excellent drawings which have been made by Kunowski, De La Rue, Lockyer, Nasmyth, the Padre Secchi, and other observers, to describe the drawings which were constructed by the eagle-eyed Dawes, in 1852–1864. This eminent observer, whose loss astronomy has lately had to deplore, made use in 1852 of an exquisite 6½-inch refractor from the celebrated Munich works. He described this instrument to me as 'absolutely perfect.' Later observations he made with a fine refractor 8¼ inches in aperture, by Alvan Clark, of America.

The first peculiarity which strikes one in examining Dawes' views of Mars, is the multiplicity of the details which they contain. One begins to doubt whether all that is pictured is to be taken as representing what the observer actually saw. For while there are large and well-marked features corresponding with those seen in other drawings, there are a multitude of light streaks and patches which one might well suppose to represent merely the general effect presented to the observer by parts of the planet not rendered quite so distinctly visible as the rest. Then again, on a rough comparison of several views, whether taken on succeeding days or belonging to different years, one does not find the sort of resemblance which one would be led to expect.

It is not a little singular that these peculiarities, which would lead one at first sight to attach little value to Dawes' drawings of Mars, are precisely those which enable us to assign to them their real importance. It is well known that Mr. Dawes was averse to long and tedious mathematical processes. Where his observations required such processes, he left the work to be done by others. Content with doing that which none could do so well as he, he left the interpretation of his observations—where this required mathematical computation of any complexity—to those whose tastes led them to care more for work of that sort. Now, when many observations have been made upon a globe continually varying in its presentation towards the eye, it is a much more difficult and laborious process than might be supposed, to reduce all these observations in such a way that the real configuration of the globe shall become known. Just as our earth in travelling round the sun bows first one pole then the other towards him, and by rotating on its polar axis brings different countries in succession under his rays, so Mars presents a continually varying configuration to the observer on earth. Nay, there is an even greater complexity in the latter variations, because the earth itself, from which we observe Mars, is not at rest. Thus it becomes a perplexing problem to educe, from a mere series of eye-transcripts of the planet, the real features which exist upon his globe. But when this has been carefully done, it clearly becomes possible to determine how far those eye-transcripts may be trusted. If we see that the varying figures presented by the same feature are due merely to the varying presentation of the planet, we not only learn that that feature exists on the planet, but we have satisfactory evidence of the skill of the observer who has made the drawings.

Now, when Mr. Dawes' drawings are tested in this way, it is found that they accord in the most satisfactory manner. Features which present no apparent resemblance are found to resolve themselves into the same well-marked ocean or continent, when each is brought to the centre of the planet's disc. One singular instance of this is worthy of notice. I have spoken of a long sea running north and south on Mars' globe, which was represented by Maraldi as a dark triangle, and which, as seen in modern telescopes, has seemed to merit the name of 'the Hour-glass Sea.' This sea appears in many of Mr. Dawes' drawings, and on account of its extent and peculiar figure there is in most cases very little difficulty in recognising it. But in explaining his tracings to the present writer, Mr. Dawes pointed out the existence of a dark marking near the border of the disc (in two or three drawings) which he compared to the leg of an old-fashioned table. It appeared as a double curve resembling Hogarth's 'line of beauty.' Now, when the requisite calculation and construction had been gone through, it was found that this mark, brought to the centre of the disc, assumed the exact figure of 'the Hour-glass Sea'; and a comparison of the position of the marking with the position of 'the Hour-glass Sea' in another drawing, reference being made to the planet's rotation in the interval, left no doubt that 'the Table-leg Sea' and 'the Hour-glass Sea' were one and the same.

The numerous details in Mr. Dawes' drawings being shown in this way to correspond to real features on the planet's surface, it became feasible to construct a chart which should represent all these features exactly as oceans and continents are represented in the maps of hemispheres which usually accompany terrestrial atlases. This has been done, and two charts have been constructed, in which all the features detected by Mr. Dawes find a place. For convenience of reference, these features have received the names of those astronomers whose researches have added in any way to our knowledge of this interesting planet. These names I shall make use of in giving a very brief sketch of the Martial oceans and continents; in other words, a brief treatise on *Areography*. See *Frontispiece.*

Each pole of Mars is capped by polar ice, which varies in extent according to the progress of the Martial seasons. Around each polar cap there is a polar sea—the northern sea being termed in the charts Schröter Sea, the southern Phillips Sea. The equatorial regions of Mars are mainly occupied by extensive continents. There are four of these: viz., Dawes Continent, Mädler Continent, Secchi Continent, and Herschel I. (Sir W.) Continent. Between Dawes Continent and Herschel Continent flows 'the Hour-glass Sea,' termed in the chart Kaiser Sea, the large southern ocean out of which this sea flows being denominated Dawes Ocean. Between Mädler Continent and Dawes Continent flows Dawes Strait, connecting De La Rue Ocean (southern) and a northern sea, named after Tycho. Herschel Continent is separated from Secchi Continent by Huggins Inlet, flowing from a large southern sea termed Maraldi Sea. In like manner Bessel Inlet, flowing out of Airy Sea (a northern sea), separates the Mädler and Secchi Continents. Between Dawes Ocean and De La Rue Ocean there lie two large islands, Phillips Island, lying within the Martial tropics, and Jacob Island, lying in the southern temperate zone. Dawes Ocean separates into four large seas extending northwards. Large tracts of land lie between these seas, but whether they are islands or not is uncertain, as their south polar extremities are never very clearly defined. In De La Rue Ocean there is a small island which presents so bright and glistening an aspect as to suggest the probability of its being usually snow-covered. It is called in the chart Dawes Snow Island. Three seas, separated by lands of doubtful extent, reach from De La Rue Ocean towards the south pole. I have mentioned the northern seas Tycho and Airy. These are connected, and form, with the third sea, named Beer Sea, a continuous fluid zone around the northern polar regions. In the zone of land which separates this sea from Schröter Sea there lies an extensive sea or lake named after Delambre.

One of the most singular features of the Martial globe is the prevalence of long and winding inlets and bottle-necked seas. These features are wholly distinct from anything known on our own earth. For example, Huggins Inlet is a long forked stream, far too wide to be compared to any terrestrial river, extending for about three thousand miles from its two-forked commencement, near Airy Sea, to the point at which it falls into the Maraldi Sea. Bessel Inlet is nearly as long. Another inlet called in the chart Nasmyth Inlet, is yet more remarkable. Commencing near Tycho Sea, it flows to the east, running parallel to that Sea and Beer Sea. It then turns sharply southwards, and expanding forms Kaiser Sea. Oudemann's Inlet connects (apparently) two bell-shaped seas; but it is not quite clear whether these seas are separated or not by an interval of land from Beer Sea. The bottle-necked seas or lakes are singular features. The seas connected by Oudemann's Inlet probably form a twin pair of seas of this sort. Two very remarkable seas, closely resembling each other in figure, and each of which is separated from De la Rue Ocean by a narrow curved strait, are very noteworthy features. Were it not for their enormous real dimensions—each sea is at least 300 miles long by 150 broad, and the channels which connect them with De La Rue Ocean are fully 250 miles long—one would be disposed to detect in their singular resemblance the evidence of artificial construction. The same remark applies to two closely resembling flask-shaped seas, which flow into Tycho Sea. Another well-marked sea of this sort flows into 'the Hour-glass,' or Kaiser Sea.

On our earth the oceans are three times as extensive as the continents. It may be noticed also that Europe, Asia, and Africa, form a single large island, so to speak; while another large island is formed by the two Americas. On Mars a very different arrangement prevails. In the first place, there is little disparity between the extent of oceans and continents; and then these are mixed up in the most complex manner. A traveller either by land or water could visit almost every quarter of that planet without leaving the element on which he had commenced his journeyings. Thus, he might proceed by water along Nasmyth Inlet for some 2,000 miles; thence southwards for some 1,500 miles along the Kaiser Sea into Dawes Ocean; thence he might coast along the four seas which extend for upwards of 5,000 miles around the southern temperate zone; thence, after circumnavigating Jacob Island and Phillips Island (a journey of about 6,000 miles), he could sail into De La Rue Ocean, and visit the three open seas and the five bottle-necked seas which are connected with it, a journey of some 6,000 miles. After this he could sail down Dawes Strait into the sea which surrounds the northern temperate zone, and after circumnavigating this zone he could sail up Bessel Inlet: the journey after leaving De La Rue Ocean being fully 10,000 miles in length. Thus he would have visited almost every quarter of the Martial globe, and journeyed upwards of 30,000 miles, *always in sight of land, and generally with land in view on both sides.* Again, a traveller by land, starting from Dawes Continent, could go round the extremity of Nasmyth Inlet and pass by a long neck of land called Mädler Land into Herschel Continent; thence rounding Huggins Inlet to Secchi Continent; thence rounding Bessel Inlet to Mädler Continent; and finally, rounding the south-eastern extremity of De La Rue Ocean, he could visit all the lands which surround the southern temperate zone.

In this intricate labyrinthine fashion are the lands and seas of Mars arranged. And perhaps, if we consider the physical relations of the planet, we shall recognise the adaptation of this arrangement to the wants of the planet's inhabitants. It must be remembered that if the lands and seas of Mars had been arranged as those of our own earth, the large ocean masses corresponding to our Pacific and Indian Oceans would never have been swayed by a tidal wave. If Mars has a satellite, it must be an exceedingly minute one; for the most powerful telescopes have been directed towards the planet without discovering any. The effects of the sun in producing tides must be almost inappreciable on Mars. These effects, it is well known, depend on the relation which a planet's diameter bears to its distance from the sun. Our earth's diameter is about 8,000 miles, and its distance from the sun 91,500,000 miles; and the solar tide upon our earth is very small. We can conceive, then, how small the Martial tides would be, when we remember that his diameter is less than 5,000 miles, and his distance from the sun upwards of 150,000,000 miles. Large oceans, unswayed by tides, would become stagnant and impure. It seems probable that the waters on Mars are sufficiently moderate in quantity to circulate freely by the mere processes of evaporation and downfall.

We have been assuming that the dark spots on Mars are really seas, and the light ochrish-coloured spots continents. Some astronomers have expressed doubts on this point; but such doubts may surely be looked on as unreasonable. We can never, of course, feel absolutely certain respecting the habitudes of so distant a globe; but there are many sound reasons for concluding that the surface of Mars is really diversified by land and water.

In the first place, there is the colour of the spots. It was formerly supposed that the greenish tint of the dark spots might be merely the effect of contrast with the brighter spots which give to Mars its ruddy tint, and earned for it the title of *ὁ πυρόεις* among the Greeks. But this opinion has been found to be erroneous, and all modern observers agree that the green tint really belongs to the dark spots. In fact, more doubt rests on the reality of the orange tint than on that of the green. Astronomers have been disposed to ascribe the orange colour to the absorptive qualities of the Martial atmosphere, and it is only within the last few years that the improbability of this view has been established.

Then we have the evidence drawn from the white spots which cap the Martial poles. If these are really masses of ice, resembling those which surround the poles of our own earth, the question must of course be answered in the affirmative; for whence could such enormous masses of snow and ice be formed, save from large seas? Now it is not easy to see on what grounds it can reasonably be doubted that these white spots are rightly called

> The snowy poles of moonless Mars.

Their variation has been found to correspond exactly with the progress of the Martial seasons—and this not for one or two Martial years, but ever since Sir W. Herschel first called attention to the periodicity of the variation. There is something singularly striking in the contrast between the small sharply-defined ellipse of white light round the pole of that hemisphere which is enjoying the Martial summer, and the irregular and wide-spreading tracts of snowy light round the cold pole. In the winter these tracts extend as far from the pole as latitude 45°, a circumstance which indicates an extent of snow-fall corresponding very closely to that which in winter covers the northern tracts of Asia and America. In summer, on the other hand, the icy circle is reduced within a range of about 8° or 10° from the pole; so that arctic travellers on Mars are not likely to approach either pole more closely than Sir Edward Parry approached the North Pole of the earth in his celebrated 'boat and sledge' journey in 1837. Now, when we see features corresponding so closely with those presented by our own earth, and consider further the *à priori* probability that our nearest neighbour among the planets would be constituted much as the earth is, we are led at once to the conclusion that these white patches are in reality snowy masses, and therefore that there must exist large seas and oceans whence the vapours are raised from which these snows have been condensed.

But further, we have distinct evidence of the existence of a cloud-bearing atmosphere around Mars. The features of the planet are often blurred and indistinct when every circumstance is favourable for observation. And it is especially noteworthy that the wintry hemisphere is always much less distinct than the hemisphere which is enjoying the Martial summer. 'A variable envelope,' writes Professor Phillips, 'gathers and fluctuates over a permanent basis of bright and dusky tracts on the surface of Mars, partially modifying the aspect of the fundamental features and even in some cases disguising them under new lights and shades, which present no constancy—a thin vaporous atmosphere probably resting on a surface of land, snow, and water.' It is also remarked that the outer parts of the disc are nearly always much more indistinct than the central parts; the former shine with that white light which we receive from the cloud-belts of Jupiter; and if we remember that the outer parts of the disc contain those regions of Mars which have lately come into sunshine, or are about to pass out of it, we see the meaning of the phenomenon to be this, that *the morning and evening skies of the Martialists are more clouded than the midday sky:* a condition which is known to prevail in certain seasons and latitudes on our own earth also. The indistinctness of the wintry hemisphere points to the prevalence of cloudy skies during the Martial winter; and this peculiarity is not only conformable with recognised habitudes on our own earth, but corresponds with the variations of the Polar snow-caps. 'The enormous transfer of moisture from one hemisphere to the other,' writes Professor Phillips, 'while the snows are melting round one pole and forming round the other, must generate over a great part of the planet heavy storms and great breadths of fluctuating clouds, which would not, as on the quickly rotating mass of Jupiter, gather into equatorial bands, but be more under the influence of prominent land and irregular tracts of ocean.'

But the strongest argument in favour of a similarity in general physical relations between Mars and our own earth, is drawn from the revelations which have been afforded by the spectroscope. This evidence has been discussed with considerable fulness in my work entitled 'Other Worlds than Ours.' Those readers who are anxious to examine the subject more at length, should read Dr. Huggins' paper on the spectrum of Mars, in the 'Monthly Notices of the Royal Astronomical Society' for 1867. The main facts pointed to by his researches are the following:—First, the red colour of Mars is *not* due to an absorptive power in his atmosphere, resembling that in our own air which causes the ruddy skies of twilight. If this were so, the snowy poles would lose their white colour, since we see them through the densest strata of the Martial atmosphere. But secondly, although the atmosphere around Mars is not so abnormally dense as to produce the ruddy tint of the planet, yet that atmosphere does contain gases and vapours corresponding to those which are present in our own air; for lines appear in the spectrum which correspond with those which appear in the solar spectrum when the sun's light traverses the lower strata of the earth's atmosphere. 'That these lines,' says Dr. Huggins, 'were not produced by the portion of the earth's atmosphere through which the light of Mars had passed, was shown by the absence of similar lines in the spectrum of the moon, which at the time of observation had a smaller altitude than Mars;' so that, if the lines had been due to the earth's atmosphere, they should have been stronger in the moon's spectrum than in that of the planet.

It appears, then, from the searching scrutiny of the spectroscope, that Mars has an atmosphere, and that that atmosphere most probably resembles our own in general constitution. Combining this evidence with that which we already possess of the presence of water in its liquid, vaporous, and solid states, upon the surface of Mars, and with the certainty that the red tint of parts of the planet is due to a real ruddiness of substance (corresponding to the tint of certain soils upon our own earth), we cannot but recognise the extreme probability that in all essential habitudes the planet Mars resembles our own earth. One circumstance may at first excite surprise: the fact, namely, that in a planet so much farther from the sun there should exist so close a resemblance to the earth, as respects climatic relations. But if we consider the results of Tyndall's researches on the Radiation of Heat, and remember that a very moderate increase in the quantity of certain vapours present in our atmosphere would suffice to render the climate of the earth intolerable through excess of heat (just as glass walls cause a hot-house to be warm long after the sun has set), we shall not fail to see that Mars may readily be compensated by a corresponding arrangement for his increased distance from the vivifying centre of the solar system.

A MINIATURE SUN.

VENUS herself, when seen under most favourable conditions, scarcely exceeds in brilliancy the giant planet Jupiter;[1] and there are times when, setting aside telescopic aid, even the practised astronomer can only distinguish her from his beautiful orb by a certain faint tinge of yellow which characterises her lustre. Far more distant than she is, both from us and from the sun, and far less brilliantly illuminated, Jupiter makes up, or nearly so, for both these circumstances by his mighty bulk, and also, as will presently appear, by a peculiar light-giving power, which distinguishes him from the Planet of Love.

I purpose to give a brief account of some of the characteristics of the noblest planet of the solar system, and then to consider certain circumstances which have received far less attention than they deserve. And although in the course of this paper I shall have to refer to several details which I have already dealt with at length in 'Other Worlds than Ours,' yet I shall, for the most part, direct the reader's attention

[1] When this paper appeared there were two Evening Stars—Venus and Jupiter shining with rival lustre in the western sky. The two planets were quite close to each other on May 12, 1871, Jupiter afterwards passing away from Venus westwards.

to new evidence—in fact, to considerations which have occurred to me, or have been discussed by others, since the second edition of that work was published.

Jupiter is a globe exceeding our earth some twelve hundred times in volume, but made of matter whose average density is so much lighter than the earth's that his weight exceeds hers but about three hundred times. Let this last point not be misunderstood, however. It can by no means be asserted that the matter composing Jupiter's globe is lighter—bulk for bulk—than our rocks, or even than our metals. It is only on the average that he is of small density. We may put the matter thus. A globe as large as Jupiter *seems* to be, if made of some substance about one-fourth heavier than water—bulk for bulk—would be equal to Jupiter in mass or weight; whereas a globe as large as our earth *is known* to be, would have to be made of a substance more than five times as heavy as water to equal her in mass.

Jupiter is more than five times as far from the sun as our earth is; and, instead of one year, he occupies nearly twelve years in travelling once on his path around the ruling centre of the planetary scheme. As he speeds along his noble orbit he rotates very swiftly; so that, notwithstanding his giant bulk, he turns completely round upon his axis about five times during the interval which this little earth occupies in making two rotations—that is, during two days.

Jupiter is attended by no less than four moons. It has been said that these moons are made of even lighter material than the planet itself. But this is not the case. Their mean densities range from 1·14 to 2·17 (water as 1). Our own moon would outweigh an equal bulk of water much more than three times.

It seems clear that in the case of Jupiter and his attendant family we have to consider relations differing wholly in character from those presented by our own earth and her satellite, the moon. All that we know certainly about Jupiter invites us to the consideration that he is unlike the earth, insomuch that if the telescope revealed features indicative of resemblance, one would expect that astronomers would look with suspicion on the discovery, or would regard it as something to be explained away. Strangely enough, the exact reverse is the case. The telescope, when applied to Jupiter, shows us nothing which can be compared with any known features of our own earth; yet this circumstance, which seems to accord so well with what has been learned about the bulk, density, and rotation of the planet, and with the known peculiarities of the subordinate system he rules over, has been looked upon as a matter to be accounted for by more or less recondite explanations. Thus it has come to pass that astronomers have traced analogies between Jupiter and the earth for which assuredly it is difficult to find any warrant. We shall presently see what the real teachings of the telescope have been; and I think I shall be able to show that they do not accord in a single respect with any known terrestrial phenomena. But what I desire specially to dwell upon here is the fact that in setting out upon our inquiry we ought not to expect such accordance; that, on the contrary, knowing we have to do with a globe every one of whose principal characteristics is quite different from the earth's, we ought to anticipate that the details brought to light by the telescope would indicate a corresponding difference.

The most important peculiarity of Jupiter's structure revealed by the telescope is beyond question the existence of belts around his mighty orb. These belts will, of course, not be confounded with the rings of his brother giant Saturn. The belts are not outside the planet, but on his surface—that is, they are on the surface of the globe *we see.* They may, for anything that is known to the contrary, lie far above his real solid body, supposing he has such a body; but I am speaking now of his appearance. We see him as a disc, and across that disc we see certain bright and dusky belts lying side by side. We can watch, on some occasions, the motion of certain irregularities in the belts; and we see these irregularities carried across the planet's disc precisely as they would be if attached to his surface and carried round by his rotation upon his axis.

These belts are very wonderful phenomena, and to say truth they are worthy of much more study than telescopists have yet given to them. What Schwabe has done for the sun-spots, some astronomer will hereafter do for Jupiter's belts, studying the belts persistently, day after day, and year after year, even from the time when Jupiter is first visible as a morning star until, after passing round to his place as an evening star, the planet is again about to veil himself for a few weeks amid the splendour of the solar beams. I venture to predict that in a few years an observer so working would be prepared to say as Schwabe did when his solar observations were beginning to bear noble fruit: 'I set out humbly, like Saul when he went forth to seek his father's asses, and lo! like him, I have discovered a kingdom.' And then other labourers would be encouraged to continue the work, as Carrington and De La Rue and Stewart continued Schwabe's work, so that we should begin to know much more than we do at present respecting those laws according to which the belts of Jupiter pass through their various changes.

But even the scattered facts which alone we as yet possess are full of interest and significance.

In the first place, as to the general arrangement of Jupiter's belts. There is commonly a bright belt across the middle of the disc, which goes by the name of the equatorial belt. It has been regarded as analogous to the zone of calms which occupies the earth's equatorial regions; but we shall presently see how little analogy there is between the two. It is usually of a pearly white colour, but not always. On either side of this belt there are commonly two dark belts 'of a coppery, ruddy, even purplish tint.' Then usually follow several alternate light and dark streaks up to the polar regions of the planet, the dark belts being ruddy, the light intermediate zones yellowish white near the equator, but greyish towards the polar regions. These regions are commonly bluish, the blue colour being sometimes, and in some telescopes, singularly pronounced.

Now, it would be easy to fill a volume with the mere description of the details seen in the belts of Jupiter. But as I am not writing an astronomical treatise, nor specially for astronomers, it would be wholly out of place to discuss all the records which observers have left us. I shall therefore select, in such order as seems most likely to serve my present purpose, those peculiarities of the belts which appear to throw the fullest light upon their constitution.

We have been so long accustomed to look upon the belts of Jupiter as due to clouds resembling terrestrial clouds in origin and behaviour, that it may seem surprising to the reader to be told that if the belts really consist of clouds these must be wholly unlike any with which our meteorologists are acquainted. Of course the bright belts would be the real cloud belts, because clouds would reflect a much more brilliant light than the actual surface of the planet. A dark spot in a bright belt would therefore come to be regarded as due to a vast opening in a bed or layer of clouds. Furthermore, a long dark streak across a bright belt would represent a long rift through a cloud-zone. Now we can imagine the existence of a vast zone of clouds all round the earth in certain latitudes, though as a matter of fact it is not likely that any such zone has at any time existed even for a single day. And we can further imagine that a circular opening or a long straight rift might appear in such a zone of clouds, and last for months, although, undoubtedly, we should hear of such a phenomenon with great surprise. If the combined testimony of many travellers informed us, for instance, that from the west of France to the east of Manchooria the weather had been cloudy for several months, save only over a certain space as large as Switzerland, where the weather had been persistently fine, we should certainly regard the information as of a most startling nature. Yet, surprising as it would appear, we can still conceive that plausible explanations might be suggested. But what would be thought if the open space in the clouds travelled steadily and swiftly, for months, over the above-named region; if it were possible to announce, either eastwards or westwards, that fine weather was coming, or cloudy, as the case might be? We certainly cannot conceive that without a total subversion of all known meteorological laws a rift in a great cloud-belt could travel for weeks until it had traversed a continent, or perhaps the best part of a hemisphere.

Now in the year 1860 a most remarkable phenomenon was discovered by observers of Jupiter. On February 29 of that year, Mr. Long, of Manchester, noticed across a bright belt—that is, across a zone of clouds—an oblique dusky streak. Its position might be compared to that of the Red Sea in a view of the earth, for it ran neither north nor south, nor east and west, but rather nearer the former than the latter direction. The length of this dark space—of this rift, that is, in the great cloud-belt—was *about ten thousand miles*, and its width at the least five hundred miles ; so that its superficial extent was much greater than the whole area of Europe. But wonderful as this rift appears when thus regarded, its mere dimensions and its singular position were by no means the most remarkable features it presented. First of all, it remained as a rift certainly until April 10, or for six weeks, and probably much longer. It passed away to the dark side of Jupiter, to return again after the Jovian night to the illuminated hemisphere, during at least a hundred Jovian days ; and assuredly nothing in the behaviour of terrestrial clouds affords any analogue of this remarkable fact. The arrangement of our clouds depends far more directly on the succession of day and night than that of the Jovian clouds would appear to do. But this is far from being all. This great rift *grew*, lengthening out until it stretched across the whole face of the planet. And it grew in a very strange way ; for its two ends remained at unchanged distances from the planet's equator, but the one nearest to the equator travelled forwards (speaking with reference to the way in which the planet turns on its axis), the rift thus approaching more and more nearly to an east and west direction. And the rate of this motion was perhaps the most remarkable circumstance of all. I quote the account given by Mr. Baxendell, one of the observers of these strange changes, and one of our most experienced telescopists:—'Since Mr. Long first observed the oblique streak on February 29, it has gradually extended itself in the direction of the planet's rotation, at an average rate of 3,640 miles per day, *or* 151 *miles per hour,* the two extremities of the belt remaining constantly on the same parallels of latitude. The belt has also gradually become darker and broader.' As pictured on April 9th, the dark rift cannot be estimated at less than a hundred thousand miles in length, or long enough to extend four times around the earth's equator.

The whole behaviour of this dark rift is so totally different from any cloud-phenomena we are acquainted with, as to seem to dispose of the belief that the belts of Jupiter are of like nature with our cloud-regions. The one great point of distinction is this, that in all their phenomena our cloud-regions are found to depend on the action of an external body—the sun—whereas all the changes which took place in the great rift above described, as well as the long duration of the rift as such, imply as clearly as possible that the belts of Jupiter are due to some cause inherent in the planet itself.

But there is one circumstance in the behaviour of this rift which is deserving of special attention. We hear it often stated that the belts of Jupiter and Saturn indicate the existence of trade-winds within the atmospheres of these planets, the more rapid rotation of the planets accounting for the more marked character of their wind-zones. But the way in which the rift shifted in position will serve to tell us whether this view is just or not. Let us remember how the trades and counter-trades come about. An air-current from polar towards equatorial regions seems to travel westwards because—bringing with it the slow rotation-movement of polar regions—it encounters the more rapid (eastward) rotation-movement of equatorial regions. On the contrary, an air-current from equatorial towards polar regions seems to travel eastwards, bringing with it as it does the more rapid eastwardly motion of equatorial regions. But both forms of air-current, if we could recognise their course from some distant station outside the earth, would give the effect of a slower motion of rotation of the earth's equatorial regions; for in one case we have air from the poles falling more and more behind as it approaches the equator, and in the other we have air from the equator moving farther and farther forwards as it approaches the poles.

Now the great rift exhibited the direct reverse of this, for we have seen that the end nearest the planet's equator travelled swiftly *forwards*.

We may note too in passing how vastly the rate of motion exceeds anything we recognise in the trades or counter-trades. Both these classes of winds are of small velocity, whereas the imagined winds of Jupiter must have rushed along at the rate of 150 miles per hour—a rate three times exceeding that of our swiftest express trains, and far greater than that of any recognised aërial currents. A velocity of 92 miles per hour is indeed equivalent, Sir John Herschel has stated, to a hurricane producing universal desolation, sweeping away buildings and tearing up trees. Such hurricanes last, too, but for a few hours. But here we have, in the case of Jupiter, winds blowing for six weeks at a stretch (in a direction the direct reverse of that corresponding to the motion of our trade-winds), with a velocity more than two-thirds greater than that of our most desolating hurricanes. Assuredly, if the Jovian hurricanes bear the same relation to these persistent winds that our terrestrial cyclones bear to the trade-winds, then we should have to regard the real *storms* of Jupiter as holding a place midway between terrestrial storms and those solar cyclones of which the spectroscope has given us such startling intelligence.

But being thus led to compare the Jovian with the solar cyclones, a circumstance which really does seem to bring the two orders of phenomena into somewhat intimate association attracts our notice. The solar spots do not pass round the sun with a uniform rotational movement—that is, they are not carried round as a country, island, or sea on our own earth is carried round by her rotation. Spots near the sun's equator travel faster than spots nearer the poles. Nor is the difference of rate by any means slight. Carrington—our best authority on this matter—has shown that a point on the sun's equator is carried round in four days' less time than a point midway between the equator and the southern pole. A point on the equator would go once round and a sixth (or gain no less than 430,000 miles), while the point towards the south would make but one circuit (about four weeks). Now this velocity of advance is equivalent to no less than 637 miles per hour, or is more than four times as great as even that swift advance which Baxendell had noted in the case of the equatorial end of the great Jovian rift. The significant fact is, however, that, both in the case of Jupiter and in that of the sun, we find the equatorial parts of the atmosphere travelling with a far swifter rotational movement than the other portions —that is, not merely moving more swiftly on account of the greater circles they describe, but performing their circuit in a shorter space of time.

It certainly seems not unreasonable to infer that this feature of resemblance implies some real resemblance of condition between the two globes. Taken alone, perhaps, the peculiarity might not suffice to justify such a conclusion ; but, when it is remembered that there is a mass of evidence pointing the same way so clearly as seemingly not to require any additional testimony, then the strange facts above recorded will assuredly seem to admit of but one interpretation. I would not, indeed, assert that as respects details we can at present interpret them at all. But this general conclusion, I think, is forced upon us—that the phenomena of Jupiter's belts are wholly distinct in origin and nature from any which terrestrial meteorology brings under our notice; that they are not primarily due to solar action, but to forces inherent in the planet; and that to some extent such forces resemble those which are at work in the solar atmosphere.

On this last point we have recently received some singular information, which, though by no means demonstrative, seems certainly to suggest relations of a very unexpected nature.

During the years 1869–71 the planet Jupiter presented an extraordinary appearance. The great equatorial belt, which is usually white, was sometimes ruddy, sometimes orange, then coppery, ochreish, greenish yellow, and in fact passed through a number of hues, mostly tints of red and yellow; but at no time, so far as observation went, exhibited what may be called its normal tint. Then again this belt and the two belts on either side of it changed very rapidly in form; great dark projections were flung (I speak always of appearances) into the great equatorial belt, which thus seemed at times to be divided into a number of ovals. The whole aspect of the planet suggested the idea that mighty processes were at work, tending to modify in a most remarkable manner the condition of the planet's atmospheric envelope. We have this on the evidence of many skilful observers.

Now it certainly is a remarkable circumstance that at the very time when Jupiter was thus disturbed, the solar atmospheric envelope also was subject to an exceptional degree of disturbance. The face of the sun, during the years 1869–71, was marked by many spots; some of these spots were of enormous magnitude, even so large as to be clearly visible to the naked eye, and the spots were of such a nature, so long-lasting, and so variable in figure, as to imply the existence of long-continued processes of disturbance acting with extraordinary violence. It may seem at first that the very circumstances of the case should prevent us from tracing any connection whatever between the solar disturbances and those which seemed to be taking place in the atmospheric envelope of Jupiter. Two orbs, separated, as the sun and Jupiter are, by an interval of about four hundred and fifty millions of miles, cannot be simultaneously affected, it would seem, by any disturbing forces. Nay, it seems so reasonable to infer that both in the case of Jupiter and of the sun, the forces at work to produce change lie far beneath the atmospheric envelope of either planet, that the idea appears at once disposed of that these forces can operate simultaneously, except by mere coincidence.

Yet such considerations have not prevented thoughtful men from examining a little further into the observed correspondence. The true man of science is seldom inclined to say either 'this or that must be so,' or 'this or that cannot be so.' His rule rather is to see whether the imagined relation has a real existence, to compare fact with fact, until the reality of the relation is established or confuted. Mr. Browning and others have not been deterred by the seeming improbability of any connection between Jovian and solar disturbances from following out this excellent plan. Professor Herschel, referring to Mr. Browning's examination of this subject, writes (to him):—'I see that you are raising very interesting questions about the appearance of Jupiter's belts, which may lead to very important results if it is found that the coloured and disturbed appearances of the belts are subject to periodical maxima and minima at about the same time as those of the spots in the sun.' He then gives the following interesting account of the appearance presented by the planet in January 1860, when the sun was passing through another of those periods of great disturbance indicated by the frequency of spots:— 'On a fine night in January 1860,' he says, 'I turned Mr. Pritchard's 6¾-inch equatorial, by Cooke, for about half an hour on Jupiter. The planet was so well defined, and the details of the markings on the equatorial belt were so peculiar, that I made a sketch of them, noting at the same time the remarkable brown colour of the equatorial belt. One of the edges of the belt (I think the southern) was beaded or divided into egg-shaped masses, which must have been of brighter or lighter colour than the background of the belt, to have given them so much prominence.'

On this, Mr. Browning remarks, that three days before he received Professor Herschel's letter, or on January 7, he made 'a careful coloured drawing of the planet, and the description given by Professor Herschel of the appearance of the coloured belt in January 1860, would apply exactly to the appearance of the belt in this drawing.'

It may be well, also, to compare the account given by Mr. Webb of the appearance of the planet in November 1869, when the disturbances now apparently going on had probably but lately begun. 'The southern portion of the equatorial zone,' he writes, 'was so progressively toned down into shadow from the north as to give the impression of a hollow, lighted obliquely in the opposite direction; yellow spaces being enclosed by elliptical arches above and similarly-shaped festoons below, being most luminous in their upper part, and being shaded off into the festoons beneath, received the opposite effect of actual convexity. The illusion was remarkable; solid ellipsoids seemed to stand out of, or be freely suspended in, a depressed channel; or it might be compared to a modification of the moulding known as 'bead and hollow' in architecture—a broad concavity placed horizontally, studded along its upper half with longitudinal bosses almost like backs of spoons, and illuminated with an oblique soft half-light. So singular was the deception that it required an effort of the judgment to rectify the mistaken conviction of the sight.'

Mr. Ranyard, favourably known in scientific circles for the zeal with which he has carried out the onerous work of collecting the records of the late Eclipse Expeditions, has examined all the accounts of past observations which were available (though more will, doubtless, now be looked up); and his results seem to confirm the startling theory that Jupiter's atmosphere sympathises with the solar atmosphere, in so far that periods of disturbance in one seem to synchronise with periods of disturbance in the other.

Now what sort of disturbances should these be, which thus appear to affect simultaneously two orbs separated by so vast a distance? Perhaps if we inquire into the laws according to which the solar spots seem associated with the planetary motions, we may recognise the nature of the action which, in a sense, encourages solar disturbance. But as yet this is by no means so simple a matter as many imagine. It is very commonly stated in books on astronomy that the periods when the sun's face shows the greatest number of spots correspond with the period when Jupiter is nearest to the sun; and even so careful a writer as Amédée Guillemin has stated that 'there exists a certain correlation between the proximity of Jupiter and the most numerous apparitions of sun-spots.' But this correlation is so far from being established, that in the very picture (borrowed from Carrington's noble work on the sun) which illustrates Guillemin's remarks, there are shown no less than eight successive correspondences between the *greatest* distances of the planet and the greatest frequency of sun-spots; and Carrington himself dwells rather on this relation than on the converse relation so commonly referred to as 'an established fact.'

What we may fairly accept, however, as at least probable is *this*, that the planets influence the sun's atmospheric envelope in some as yet unexplained manner, and that Jupiter has a large share in the work; while also it seems shown that whenever

Jupiter is so situated as to be at work most effectively in disturbing the sun, then he is himself most disturbed. Precisely as, if the moon had oceans, the tides raised in those oceans by the earth would be largest at the very time when the tides raised by the moon in *our* oceans were largest, so also the action of Jupiter on the sun and the sun's action on Jupiter would seem to wax and wane together.

But we are thus brought to regard Jupiter as himself in some sort a sun. He seems certainly to be subject to processes of disturbance comparable with those by which the sun is affected. There is assuredly nothing in the meteorology of our own earth comparable with the association we have been considering above. From no station in the solar system would our earth, watched by assiduous observers, be found to present changes of appearance synchronising with the solar disturbances. Nor again would the progress of any changes, apart from those due to the seasons, indicate any influence due to her greater or less proximity to the sun as she circuits round her orbit.

If the conception shall appear startling that Jupiter is the scene of some forms of action resembling, only much less violent, the processes at work in the sun, yet let it be remembered that there is much in the appearance of Jupiter which cannot readily be otherwise explained. It is very well to compare his belts, for instance, with our wind-zones—our trade and counter-trade regions. Such an explanation sounds highly plausible; and it has so long passed current, that we are apt to forget the circumstance that we have not a particle of evidence in its favour. To get trade-winds or counter trade-winds, we require currents of air travelling, in the first place, north and south, or nearly so; and again, to get such currents, we require great differences of temperature, resulting in great disturbances of atmospheric equilibrium. The intense heat of our equatorial and tropical regions may well be understood to cause an indraught of cooler air from regions a thousand miles or so nearer the poles. But if a distance of ten thousand miles and more separated the cooler from the more heated regions, the indraught would be very much feebler. If we had two coiled springs, one a foot long and the other ten feet long, it is clear that a compression by some given amount—say one inch—would affect the shorter very much more than the longer; and, *mutatis mutandis*, the above-considered differences of temperature are very fairly illustrated by this relation. Jupiter being more than ten times greater in all his linear dimensions than the earth, it is clear that he must have just such a diminution of all those effects of indraught or overflow by which we explain our own trade and counter-trade winds. He rotates more swiftly, it is true; but against this may fairly be set the fact that he is five times farther from the sun, and (if other things are equal) must needs receive but a twenty-fifth part of the heat which, falling on the earth, rouses our winds into action. It seems to me amazing that under these circumstances the sun should ever have been regarded as the exciting cause of those processes which shape the atmospheric envelope of Jupiter into the bright and dusky zones.

The explanation *obviously* suggested (not necessarily, however, the correct one) is that the formation of the belts of Jupiter is due to the violent uprush of vapours from vast depths below his visible surface. For vapours thus flung upwards, coming as they would from regions nearer to Jupiter's centre and therefore moving more slowly to regions farther away and therefore moving more rapidly (precisely as the rim of a wheel moves more rapidly than the middle of a spoke), would be left behind, and, as seen from a distant station, would form a trail, so to speak, lying, as the belts do, parallel to the planet's equator. Nor are we without evidence of the action of some such eruptive forces as are here suggested. For white spots, spoken of by the observers as specks, yet two or three thousand miles across at the least, have been seen from time to time, and but for a time, upon the belts; and these can in no way be interpreted so readily as by supposing them due to explosive action casting up enormous masses of vapour into the higher regions of Jupiter's atmosphere.

Before concluding, I would remind the reader that the evidence here adduced is altogether independent of that which I have brought forward elsewhere. I have shown in my 'Other Worlds,' (1) that the equatorial bright belts both of Jupiter and Saturn are in no sense comparable with our zone of calms or *doldrums*, being persistently equatorial, whereas our zone of calms travels far to the north of the equator in summer, and far to the south in winter; (2) that the amount of light received from Jupiter, Saturn, and Uranus is very far in excess of the amount due to the size and position of these orbs—a fact suggesting the theory that a portion of their light is inherent; and (3) that we have evidence of a very strong, nay, all but irresistible nature, to show that even the seeming *figures* of Jupiter and Saturn are liable to change. These and other remarkable circumstances recognised by astronomers, combined with the evidence adduced in the present essay, and the striking resemblance of the outer planets to the sun in the matter of mean density, do certainly seem to suggest in a very forcible manner that these outer planets are in a condition very different from that of our own earth; and though it may be going too far to say that they are actually minor or subordinate suns, yet such a view seems likely to be nearer to the truth than that which regards them as habitable worlds like our own.

Regarding Jupiter in this way, we need by no means consider that he is never to be inhabited. The processes we see at work out yonder may be fitting him for the support of myriads of races of living creatures. For anything we know to the contrary, he may be passing through stages which our own earth has long since passed through. In his case the processes of change may take up more time, indeed, but this is fitting when the vastness of his bulk is considered. For it must not be forgotten, that light though his substance may be on the average, he has in him the materials for 300 globes such as our earth; that out of his substance every other planet now existing in the solar system might be fashioned, and yet abundant matter be left for making other worlds; that, in fine, in whatever condition he subsists now, or at any future time, he must always be the noblest of all the members of the sun's family.

The *St. Pauls Magazine* for April, 1871.

SHOOTING-STARS, METEORS, AND AEROLITES.

On a calm, clear night, when

> All the stars
> Shine, and the immeasurable heavens
> Break cpen to their highest,

the contemplation of the celestial vault raises in the least thoughtful mind vague suggestions of infinity, eternity, and omnipotence. A knowledge of the wonders which have been revealed by modern astronomical investigations, largely enhances these emotions. Looking into the starlit depths of heaven, the astronomer knows that the objects presented to him shine from distances so great, that not only are they inconceivable themselves, but that the very unit by which he attempts to gauge them is inconceivable. He knows that what he sees is not that which *is*, but that which *was*,—years ago as respects the nearer parts of the heaven-scape, but long ages ago, he doubts not, as respects faintly shining stars visible only by momentary scintillations. He has good reasons, indeed, for surmising that the diffused illumination, which on the darkest night lights up the background of the view, had been travelling towards the earth myriads of ages before she had assumed her present state, or had been inhabited by races now subsisting upon her surface. So long, he believes, has light—which would eight times girdle the earth in a second—been occupied in journeying towards us from the depths into which he is gazing. Thus the same view exhibits to him eternity of time and infinity of space. He sees also omnipotence in the operation of those laws—the impress of the Almighty mind—under whose action all that he sees is undergoing a process of change, vast, resistless, unending, yet so solemn in its grand progress that man knows no apter type for immutability.

To an observer impressed with these emotions, the contrast is startling when there is a sudden exhibition of life and motion in the calm realms of night. We cannot, however, look for any long interval of time towards any quarter of the sky, without perceiving indications more or less distinct of objects other than the fixed stars. Now on one side, now on another we seem to catch momentary glimpses of moving light, disappearing too rapidly to be detected. But before many minutes have elapsed we receive less doubtful evidence. There sweeps silently and swiftly across the starlit depths a palely gleaming light, which disappears after traversing an arc of greater or less extent. I know not how it may be with others, but to myself the impression conveyed by the apparition of a shooting-star, is that no apter emblem can be conceived of the finite and the feeble.[1] The

[1] 'The spinstress Werpeja,' says a Lithuanian myth, 'spins the

suddenness with which these objects appear, their hasty movements, and their short duration, alike conduce to render as marked as possible the contrast they present to the fixed stars.

But though shooting-stars are short-lived, and apparently insignificant, yet we shall presently see that the relations they present to other celestial objects are not unimportant. We are brought by means of them into contact, so to speak, with external space. 'Accustomed to know non-telluric bodies solely by measurement, by calculation, and by the inferences of our reason,' writes Humboldt, 'it is with a kind of astonishment that we touch, weigh, and submit to chemical analysis, metallic and earthy masses appertaining to the world without.' The vulgar sense sees, in shooting-stars, nothing but 'dying sparks in the clear vault of heaven;' the reflecting mind will find much to arouse interest, and much that is worthy of close study and investigation.

I proceed to present results of observations—(i.) casual and (ii.) particular—which have been made on shooting-stars, meteors, and aërolites.

A careful observer directing his attention towards any quarter of the sky on a clear night, will see on an average six shooting-stars per hour. We may assume therefore that about fifteen appear above the horizon of any place during each hour. More appear

thread of the new-born child, and each thread ends in a star. When death approaches, the thread breaks, and the star falls, quenching its light, to the earth.'—Grimm: *Deutsche Mythologie.*

after than before midnight, the most favourable time for observation being from one o'clock to three. In tropical regions shooting-stars are seen oftener, and shine far more brilliantly than in our northern latitudes. This peculiarity is due no doubt to the superior purity and serenity of the air within and near the tropics, not to any real superiority in the number of falling-stars. Sir Alexander Burnes, speaking of the transparency of the dry atmosphere of Bokhara, a place not farther south than Madrid, but raised 1,200 feet above the sea-level, says—'The stars have uncommon lustre, and the Milky Way shines gloriously in the firmament. There is also a never-ceasing display of the most brilliant meteors, which dart like rockets in the sky; ten or twelve of them are sometimes seen in an hour, assuming every colour; fiery-red, blue, pale, and faint.' In our climate about two-thirds of all the shooting-stars seen are white; next in frequency come yellow stars, one yellow star being seen for about five white stars; there are about twice as many yellow as orange stars, and more than twice as many orange as green or blue stars.

Meteors or fire-balls are far less common than shooting-stars. They are magnificent objects, their brilliancy often exceeding that of the full moon. Some, even, have been so brilliant as to cast a shadow in full daylight. They are generally followed by a luminous train, which seems to be drawn out of the substance of the fire-ball itself. Their motion is not commonly uniform, but (so to speak) impulsive;

they often seem to follow a waved or contorted path, their form changes visibly, and in general they disappear with a loud explosion. Occasionally, however, a meteor will be seen to separate without explosion into a number of distinct globes, accompanying each other in parallel courses, and each followed by a train. 'Sometimes,' says Kaemtz, 'a fire-ball is divided into fragments, each of which forms a luminous globe, which then bursts in its turn; in others the mass, after having given vent to the interior gases, closes in upon itself, and then swells out anew to burst a second time.' Meteors which move impulsively generally burst at each bound, giving forth smoke and vapours, and shining afterwards with a new lustre. In some instances the crash of the explosion is so great that 'houses tremble, doors and windows open, and men imagine that there is an earthquake.'

Aërolites, or meteoric stones, are bodies which fall from the sky upon the earth. They are less common than meteors, but that they are far from being uncommon is shown by the fact, that in the British Museum alone there are preserved several hundreds of these bodies. They vary greatly in size and form; some being no larger than a man's fist, while others weigh many hundreds of pounds. Marshal Bazaine brought from Mexico a meteorite weighing more than three-quarters of a ton; but this weight has been far exceeded in several cases. Thus a meteorite was presented to the British Museum in 1865, which weighs no less than three and a half tons. It had been found near Melbourne, and one half of the mass had been promised to the Melbourne Museum. But fortunately it was saved from injury. A meteorite weighing one and a quarter tons, which had been found close to the greater one, was transferred from the British to the Melbourne Museum, and the great meteorite forwarded unbroken to our national collection. A yet larger meteorite lies on the plain of Tucuman in South America; it has not been weighed, but measurement shows that its weight cannot fall short of fourteen or fifteen tons. It is from seven to seven and a half feet in length.

There have been twenty well authenticated instances of stone-falls in the British Isles since 1620. One of these took place in the immediate neighbourhood of London, on May 18th, 1680. Besides these, two meteoric stones, not seen to fall, have been found in Scotland.

The Chinese, who recorded everything, give the most ancient account of stone-falls.[1] Their record of these phenomena extend to 644 years before our era, their accounts of shooting-stars to 687 B.C. We need not remind our classical readers of the stone which fell at Ægos Potamos, B.C. 465, and which was as large as two millstones. In the year 921, there fell at Narni a mass which projected four feet above the

[1] The fall of stones said by Livy to have taken place on the Alban Hill, can hardly be accepted as an historical fact. There are, however, indubitable records, not due to human agency, of much more ancient stone-falls; since *fossil meteorites* are found imbedded in the secondary and tertiary formations.

river, into which it was seen to fall. There is a Mongolian tradition that there fell from heaven upon a plain near the source of the Yellow River, in Western China, a black rocky mass forty feet high. In 1620, there fell at Jahlinder a mass of meteoric iron, from which the Emperor Jehangire had a sword forged.

These traditions had long been known, but men were not very ready to accept, without question, the fact that stones and mineral masses actually fall upon the earth from the sky. In 1803, however, a fall of aërolites occurred which admitted of no cavil. On the 26th of April, in that year, a fiery globe was seen to burst into fragments, nearly over the town of L'Aigle, in Normandy. By this explosion thousands of stones were scattered over an elliptical area seven or eight miles long, and about four miles broad. The stones were hot (but not red-hot) and smoking; the heaviest weighed about seventeen and a half pounds. The sky had been perfectly clear a few moments before the explosion. With a laudable desire to profit by so favourable an opportunity, the French Government sent M. Biot to the scene of the fall. His systematic inquiries and report sufficed to overcome the unbelief which had prevailed on the subject of stone-showers.

Another very remarkable fall is that which took place on October 1, 1857, in the department of Yonne. Baron Seguier was with some workmen in an avenue of the grounds of Hautefeuille near Charny, when they were startled by several explosions quite unlike thunder, and by strong atmospheric disturbances.

Several windows of the château were found to be broken. At the same time a proprietor of Château-Renard saw a globe of fire 'travelling rapidly through the air towards Vernisson.' Baron Seguier heard shortly after that at the same hour a shower of aërolites had fallen a few leagues from Hautefeuille, and in a locality lying precisely in the direction towards which the proprietor of Château-Renard had seen the meteor travelling. A mason had seen the fall, and narrowly escaped being struck by one of the fragments. This piece, which was found buried deep in the earth, near the foot of the mason's ladder, was presented to the Academy of Sciences by Baron Seguier.

Aërolites often fall from a clear sky. More commonly, however, a dark cloud is observed to form, and the stony shower is seen to be projected from its bosom. It is probable that what appears as a bright train by night is seen as a cloud by day. Something seems to depend on the position of the observer. The meteor which burst over L'Aigle appeared wholly free from cloud or smoke to those who saw it from Alençon, while to observers in L'Aigle the phenomenon was presented of a dark cloud forming suddenly in a clear sky. In a fall which took place near Kleinwinden (not far from Mühlhausen), on September 16th, 1843, a large aërolite descended with a noise like thunder, in a clear sky, and without the formation of any cloud.

The length of time during which fire-balls which produce aërolites, are visible, has been variously stated; but we have no evidence which would lead us to accept the story of Daïmachos, that the fiery cloud from which the stone of Ægos Potamos was projected had been visible for seventy days in succession. The story seems to identify the author with a certain Daïmachos of Platæa described by Strabo as a 'vendor of lies.'

There is another singular fiction respecting fire-balls. It was said that shooting-stars and meteors were in reality fibrous gelatinous bodies, and that such bodies had been found where meteors had been seen to fall. Reference is not unfrequently made to this fable by writers ancient and modern. Thus Dryden, in his dedication to *The Spanish Friar*, speaking of Chapman's *Bussy d'Ambois*, says,—'I have sometimes wondered in the reading, what has become of those glaring colours which amazed me in *Bussy d'Ambois* upon the theatre; but when I had taken up what I supposed a fallen star, I found I had been cozened with a jelly; nothing but a cold dull mass, which glittered no longer than it was shooting.'

One circumstance remains to be mentioned among the results of casual observation. On certain occasions shooting-stars have been observed to fall in much greater numbers than on ordinary nights. Among the earliest records of such a phenomenon is the statement by Theophanes, the Byzantine historian, that in November, 472, at Constantinople, the sky seemed to be alive with flying meteors. In the month of October, 902, again, so many falling stars were seen that the year was afterwards called the 'year of stars.' Condé relates that the Arabs connected this fall with the death of King Ibrahim Ben-Ahmed, which took place on the night of the star-shower. The year 1029 was also remarkable for a great star-fall, and in the annals of Cairo it is related that, 'In the year 599, in the last Moharrun (October 19, 1202), the stars appeared like waves upon the sky, towards the east and west; they flew about like locusts, and were dispersed from left to right.' A shower of stars, accompanied by the fall of several aërolites, took place over England and France on April 4th, 1095. This was considered by many as a token of God's displeasure with King William II.: 'Therefore the kinge was tolde by divers of his familiars that God was not content with his lyvynge; but he was so wilful and proud of mind that he regarded little their saying.'

In modern times, also, some very remarkable star-showers have been observed. Amongst these one of the most noteworthy was that seen by Humboldt, when travelling with M. Bonpland in South America. He writes:—'On the morning of the 13th of November we saw a most extraordinary display of shooting-stars. Thousands of bolides and stars succeeded each other during four hours. Their motion was very regular from north to south. From the beginning of the phenomenon there was not a space equal in extent to three diameters of the moon, which was not filled each instant with shooting-stars. All the meteors left phosphorescent traces behind them.'

In 1833, also, there was a magnificent display of meteoric fireworks. It was accompanied by a brilliant exhibition of the aurora borealis. The same phenomenon was seen also at Bremen, in 1838, during a fall of meteors and shooting-stars.

Before proceeding to detail some of the singular results which have rewarded the modern examination of this interesting subject, it may be well to exhibit the guesses and theories which were suggested of old, to explain the observed phenomena.

The Greeks, as usual with them, guessed boldly, sometimes acutely. Among the earliest of their theories we find the view that shooting-stars are generated by vapours ascending from the earth,—an hypothesis that has been sustained quite recently by Egen, Fischer, and Ideler. Aristotle supposed that aërolites were masses of stone which had been raised by tempests from the earth's surface. He explained in this way the appearance even of the gigantic mass which fell at Ægos Potamos. Others again, seeing that meteorites fell in full sunlight, conceived the notion that they were projected to us from the sun. Amongst those who held this opinion was Anaxagoras of Clazomene. This philosopher, we are told, predicted the fall of aërolites from the sun,—a tradition registered and ridiculed by Pliny. But some among the Greeks held opinions which, though somewhat vaguely expressed, may be looked upon as (at the least) very good guesses. We may cite, for instance, the following remarkable passage in Plutarch's life of Lysander:—

'The opinion held by those who thought that shooting-stars are not mere emanations from ethereal fire, becoming extinguished quickly after being kindled, is a probable one; nor are falling stars produced by the inflammation and combustion of a mass of air which had moved away towards the higher regions; rather they are *celestial bodies* which are precipitated through an intermission of the centrifugal force, and fall, not only on inhabited places, but in even larger numbers into the great sea, where they are never seen.' We find in this passage a tacit reference to the opinion of Anaxagoras that the heavenly bodies are masses of rock torn from the earth by the centrifugal force of the surrounding ether, and set on fire in the heavens. The opinion of Diogenes of Apollonia is not dissimilar; he says, 'Together with the visible stars there move other invisible ones, which are therefore without names. These sometimes fall on the earth and are extinguished, as took place with the star of stone which fell at Ægos Potamos.'

In the Middle Ages the phenomena presented by shooting-stars were explained in a somewhat authoritative, but not very satisfactory, manner. The judicious use of a few set phrases sufficed to clear up all difficulties. We hear of humours and exhalations attracted by affinity to the upper regions of air; of condensation, concretion, ultimate repulsion, and so on; and all this not in a doubtful hypothetical tone, but in the authoritative manner of men possessing all knowledge. On one point especially the writers of those days are very positive,—meteors are in no way to be regarded as astronomical phenomena. They marked out peremptorily the bodies they consented to look upon as celestial. Their knowledge of the laws regulating these bodies was far too exact, in their opinion, for any doubt to exist that a number of erratic short-lived bodies, moving in a hasty and undignified manner across the sky, were not to be admitted as members of the stately family of planets, still less as copartners with the stars of the crystalline. One, even, who saw opening out before him a new system, who aided to overturn the old, and to lay the foundation of modern astronomy—the ingenious Kepler—yielded to the old idea on this point, to the fascinating phantasy that things are to be seen as men would have them, not as indeed they are. In his case, perhaps, this is hardly to be wondered at. He had discovered and rejoiced in the 'harmonies of the planets;' he had written in his enthusiasm,—'Nothing holds me; I will indulge my sacred fury; I will triumph over mankind, for I have stolen the golden vases of the Egyptians.' And it would doubtless have seemed as a strange thing to him to conceive that he had heard but a few stray notes of the music of the spheres, that he had not yet—as he had hoped—

> Come on that which is, and caught
> The deep pulsations of the world,
> Æonian music measuring out
> The steps of Time.

Turn we to the investigations of modern scientific men,—of men whose principle it is, or ought to be, that theory-framing should be preceded by systematic observation, by careful calculation and examination, and if possible by experiment. They have successfully attacked problems which seem to the uninitiated wholly insoluble,—determining the heights at which shooting-stars appear and disappear, the velocity with which they move, their size and weight, nay, the very substances of which they are composed; they have discovered laws regulating the numbers and paths of these visitors; they have analysed aërolites chemically and microscopically; and, lastly, they have sought to determine whether it is possible to construct artificial meteorites.

The determination of the height of shooting-stars is a problem which has been successfully attacked by Brandes, Heis, Schmidt, Olbers, and others. From the results of observations made by these astronomers, Professor Newton and Mr. Alexander Herschel have calculated that shooting-stars appear, on an average, at a height of seventy-two miles, and disappear at a height of fifty-two miles. The Padre Secchi, at Rome, on the nights of 5th–10th August, carried on a series of simultaneous observations, by telegraphic communication between Rome and Civita Vecchia. The result obtained by him was that shooting-stars appear at a height of seventy-four and a half miles, and disappear at a height of fifty miles,—a result almost coincident with the former. It appears, then, that shooting-stars are some twenty miles nearer when they are just disappearing than at their first appearance.

When the distance of a shooting-star is known, it is easy to determine the velocity of the star's motion. It appears from a careful series of observations that shooting-stars describe a visible arc many miles in length, with an average velocity of about thirty-four miles per second. This velocity is nearly twice as great as that wherewith the earth describes her orbit about the sun. Moving with such a velocity, a body would pass from the earth to the moon in about a couple of hours, or from London to Edinburgh in about ten seconds.

Meteors, as might be expected, approach nearer to the earth than shooting-stars. They do not in general move quite so rapidly. A remarkable meteor which appeared on April 29, was seen by two practised observers, Messrs. Baxendell and Wood, at Liverpool and Weston-super-Mare respectively. From a careful examination of their observations it results that the meteor appeared when at a height of fifty-two miles vertically over Lichfield, that it travelled in a southerly direction at the rate of about twenty miles per second, and disappeared when over Oxford at a height of thirty-seven miles, having traversed a distance of nearly seventy-five miles. The meteor appears to have belonged to the detonating class. Eight minutes after its appearance Mr. Wood heard a sound 'which resembled the momentary roar of a railway-train, at some distance, crossing over a bridge.' It is worth noticing that Mr. Wood must have heard the roar of the meteor inversely, that is, the first part of the sound which he heard was the part generated last, and *vice versâ*. A detonation was also heard at Stony Stratford, a place lying nearly under the path of the meteor.

To determine the actual size of a meteor is not easy, nor indeed can much weight be attached to such determinations. From observations of the apparent dimensions of several meteors which have travelled at known distances, it would seem that these bodies vary in diameter from 100 to 13,000 feet.

Singularly enough, it is easier to determine the weight of a meteor or shooting-star than its size. The method of doing so could not be very well explained in these pages; it will be sufficient to say that it depends on the observation of the amount of light received from a body travelling with known velocity through a resisting atmosphere. From such observations it appears that shooting-stars weigh on an average but a few ounces, while some meteors weigh hundreds of pounds. We have seen that aërolites of much greater weight occasionally reach the earth.

Still more strange is the fact that we are able to determine the substances, or some of them, which enter into the composition of meteors or shooting-stars. This is done by means of a spectroscope so constructed as to take in a large part of the heavens. For instance, when an instrument of this sort is turned towards the Great Bear the spectra of the seven principal stars of that constellation are seen at one view. Mr. A. Herschel observed with such an instrument the spectra of many of the shooting-stars which appeared on the nights

9th–11th August. He found that some of these bodies exhibit a continuous spectrum, showing that they are probably solid bodies, heated to ignition. Others exhibit a greyish-white spectrum, indicating (probably) a nucleus and train of heated sparks. But the greater number of meteors give a spectrum consisting of one or more lines, showing that during apparition most of these bodies are gaseous. The gaseous meteors exhibit with remarkable distinctness a strong yellow line, perfectly agreeing in position with the well-known line given by the ignited vapour of the metal sodium. Other lines, due to the presence either of potassium, sulphur, or phosphorus, are also frequently seen. It is noteworthy that the sodium line is exhibited in the spectrum of lightning, so that it is not quite certain that this line in the meteor-spectrum is due to the presence of sodium in the chemical composition of meteors. However, it cannot but be considered as highly improbable that any traces of sodium exist in the atmosphere at the great height at which meteors travel; still less probable is it that such considerable quantities of sodium exist as would account for the strongly-marked character of the yellow line shown in meteor-spectra. Mr. Herschel notes especially of those trains which fade most slowly that they consist of *nothing else but soda-flames* during the latter portion of the time that they continue visible. 'Their condition is then exactly that of the flame of a spirit-lamp, newly trimmed, and largely dosed with a supply of moistened salt.'

One of the most remarkable facts which observation has revealed respecting shooting-stars, is the recurrence of star-showers of greater or less intensity on certain days of the year. It was observed long ago that on the nights of August 9–11 stars fell in much greater numbers than usual. For instance, there is a legend in parts of Thessaly, that near the time of the festival of St. Laurence, the heavens open and exhibit shining lights (*κανδήλια*); and in an ancient English church calendar, the August star-showers are described as 'fiery tears.' We find the 10th of August also characterised by the word *meteorodes*, in a MS. called *Ephemerides rerum naturalium*, preserved in Christ's College, Cambridge. The great November shower was not recognised so soon. This shower is characterised by an alternate increase and decrease of intensity, the interval between successive maxima being thirty-three or thirty-four years. For several years before and after the true year of maximum intensity the shower is in general distinctly exhibited. My readers will not need to be reminded of the recurrence of this shower in November 1866, as predicted by astronomers. The year 1866 was spoken of in these predictions as the year in which the November shower would exhibit its maximum of splendour. My own opinion is that 1867 will turn out to be the true year of maximum intensity, and that fine showers will be seen during the years 1868 and 1869. Whether, however, such showers, should they occur, will be as well seen in England as that of November 13th 1866, is problematical, since it has frequently happened that magnificent showers are seen in certain longitudes, and but a moderate display in others.[1] Besides the August and November showers, there are the showers of October 16–23, of December 6–13, of April 9–10, of July 25–30, and others. There are in fact nearly 'one hundred recognised star-showers, as well determined in the majority of cases as are the older and better known showers of August and November.' While on this point, I may note that aërolites have their favourite seasons for visiting the earth, and that of the twenty which are known to have fallen on the British Isles, three fell on May 17–18, four on August 4–9, two on July 3–4, and two on April 1–5. Of the other nine, three are undated.

Another singular law has been detected in the motions of shooting-stars which appear at the same season. It is found that when their paths are produced backwards they pass through or near one point on the celestial sphere, and that this point has no fixed relation to the horizon of the observer, but is fixed among the stars. Sometimes the shooting-stars which appear on the same night may be divided into two sets, each having a distinct 'radiant point,'—as astronomers have named these centres of divergence. Each of the fifty-six star-showers spoken of above has its radiant point. Humboldt states that the radiant points of the November and August showers are those

[1] A full account of the fulfilment of these anticipations, and additional information respecting meteors, is given in my *Essays on Astronomy*, pp. 105–162.

points precisely towards which the earth is travelling at those seasons respectively. He has been followed in this statement by many writers on astronomy. But the statement is not true. In fact, these radiant points do not lie on the ecliptic, whereas the point towards which the earth is travelling at any moment, necessarily lies upon the ecliptic.

Aërolites have been analysed, and it is found that they contain many elements known on earth. These usually appear combined in the following types:—metallic iron, magnetic iron, sulphuret of iron, oxide of tin, silicates, olivine, &c. In one aërolite only, namely, in a stone which fell on April 15, 1857, near Kaba-Debreczin—'a small quantity of *organic* matter akin to parafine' has been detected,—a very noteworthy circumstance. It is also remarkable that no new element, and only one or two new compounds (compounds, at least, which have not yet been recognised among terrestrial formations) have ever been detected in meteorites.

The microscopical examination of aërolites has also revealed much that is interesting and instructive. The crystals of the mixed minerals which appear in aërolites are found to differ in some important respects from those of volcanic rocks, 'but their consolidation must have taken place from fusion in masses of mountain size.' The alloy of metallic iron and nickel which is a principal component of meteorites is often found to be as regularly crystallised as a mass of spar.

M. Daubrée has attempted to produce artificial meteorites by combining together suitable elements and compounds. In doing so he has discovered a very singular fact. The crystals he obtained resembled the long needles which are seen to form on water when it is *slowly frozen*; whereas the black crystalline crust with which all meteorites are covered has a granular structure resembling snow or hoar-frost, which we know to be formed by the *sudden* passage of water from the vaporous to the solid state. This phenomenon shows that meteoric masses have been subjected to actions altogether different from those which the chemist is able to bring into operation.

The result of the series of observations which we have here recorded is that we are able to attempt the formation of a theory of shooting-stars with some confidence.

In the first place, we are able to reject decisively certain theories which have found favour at different times.

The immense height at which shooting-stars appear enables us to reject the atmospheric origin which has been suggested, for we have every reason for supposing that the air at a height of seventy miles above the earth is of extreme tenuity, and therefore quite incapable of supporting in sufficient quantity those vapours from which shooting-stars, on this theory, are assumed to be generated.

Two other theories, which have not hitherto been mentioned, are also overthrown by the results of modern observation. Both may be called *volcanic*, but one assumes that shooting-stars are bodies which have been projected from volcanoes on the earth, while the other assumes that they have come from volcanoes on the moon. Observation has shown that when Mount Etna is in full activity, the masses of stone thrown from its crater have a velocity of less than 1,600 feet per second, which is but one-112th part of the mean velocity with which shooting-stars are observed to move. The theory that falling-stars come from the moon was first propounded by Terzago, an Italian, in the seventeenth century. It appears, however, to have been not unknown in ancient times, since we learn that the Syrian astronomers were in the habit of looking for shooting-stars when the moon was full; while Greek astronomers considered the most favourable season to be at the time of lunar eclipse, that is, when the moon is full, but the sky dark. Bizarre as it may seem, this fanciful explanation has been thought worthy of strict mathematical examination by such astronomers as Laplace, Olbers, and Poisson. It appears, from their calculations, that the velocity with which stone showers should be propelled from the moon in order to reach our earth with the velocities observed among shooting-stars may be considered to be utterly beyond the powers we could concede to lunar volcanoes, even if it were proved (which is far from being the case) that any active volcanoes now exist on the moon's surface.

The three theories just considered have been effectually overthrown by the simple observation of the height and velocities of shooting-stars. When we add to this consideration the recurrence of star-showers, not in particular states of the earth's atmosphere, not connected in any way with the activity of terrestrial volcanoes, nor conceivably with the action of assumed lunar volcanoes, these theories appear yet more inadequate to explain observed phenomena. The phenomenon of radiant points, lastly, is so wholly inexplicable on any of these theories, that we may dismiss them finally as utterly untenable.

We must, therefore, turn to the theory which had already been suggested by Greek philosophers—that shooting-stars and meteors are extraneous bodies dragged towards the earth by the force of her attractive influence. But modern scientific discoveries enable us to exhibit this theory in a more inviting form, and at the same time to offer analogues obviously tending to confirm the hypothesis. The discovery of a zone of planetoids, the inquiry into the nature of the zodiacal light, and the mathematical examination of the 'stability' of the Saturnian ring-system, have led astronomers to recognise the existence in the solar system of minute bodies travelling in zones or clusters around a central orb. There is, therefore, nothing unreasonable in the supposition that there are zones and clusters of such bodies travelling round the sun in orbits which intersect the earth's path. When in her course around the sun she encounters any of the bodies forming such zones and clusters, they are ignited by friction as they pass through the upper layers of the air, and become visible as shooting-stars or meteors according to their dimensions; or they may even fall upon her surface as aërolites.

The recurrence of star-showers is a necessary consequence of the hypothesis we are considering. For, if we suppose the zones of meteors, or the orbits of meteor-clusters, to have a fixed position in the solar system, or to be subject to those slow, progressive or retrogressive shiftings with which the study of the solar system familiarises us, there will necessarily result a regular recurrence of showers either on fixed days, or on days uniformly shifting round among the seasons. This is precisely what is observed with the fifty-six recognised star-showers.

The earth does not necessarily (or probably) pass centrally through a meteor-cluster every year, nor probably are the meteor-zones uniformly rich throughout. Thus we can readily understand periodic undulations in the intensity of star-showers, or even periodic intermittences.

The phenomenon of radiant points also is not merely reconcilable with, but obviously indicates the hypothesis we are considering. For during the brief interval occupied by the earth in passing through a well-marked zone or cluster, the bodies composing such zone or cluster may be considered to be moving (relatively to the moving earth) in parallel lines. Therefore, by a well-known law in perspective, their apparent paths, viewed from the earth, must have a 'vanishing point' on the celestial sphere,—that is, a 'radiant point' among the fixed stars.

The remarkable velocity with which shooting-stars travel is satisfactorily accounted for by the modern theory. If we suppose zones and clusters of cosmical bodies (pocket-planets we may term them with Humboldt) to be travelling in different directions around the sun, it is clear that the members of those zones which travel in the same direction as the earth, will overtake, or be overtaken by her, with the *difference* of their respective velocities, while those which travel in the contrary direction will encounter the earth with the *sum* of their own and the earth's velocity. Now, just as in walking along a crowded road we *meet* many more people than we overtake, or are overtaken by; so, clearly, by far the larger number of observed shooting-stars must belong to the latter class named above, and therefore the average observed velocity will not fall very far short of the sum of the velocities of the earth and the shooting-star system.

Fairly considered, the modern theory may be looked upon as established: for, first, all other available hypotheses have been shown to be untenable; and, secondly, the most remarkable shooting-star phenomena are shown to be consistent with, or rather to point directly to, the modern hypothesis. It remains only that some minor peculiarities should be noticed.

It has been remarked that shooting-stars are much more commonly seen in the months from July to December, than in those from January to June. Remembering that this remark refers to observations made in our northern hemisphere, it is easily reconciled with the modern theory, when we consider that the north pole is on the *forward hemisphere* of the earth (considered with reference to her orbital motion) during the first-named period, and on the *rear* (or *sheltered*) *hemisphere* during the second.

Again, it has been remarked that shooting-stars are seen more commonly in the hours after midnight, and that aërolites fall more commonly before noon. In other words, these extraneous bodies reach the earth (or her atmosphere) more frequently in the hours from midnight to noon than in those from noon to midnight. Humboldt suggests in explanation we know not what theory of variation in the ignition-powers of different hours. But it is clear that the true explanation is founded on the principle presented in the preceding paragraph, since the *forward* hemisphere contains places whose local time lies, roughly speaking, between midnight and noon, while places whose local hour lies between noon and midnight lie on the *sheltered* hemisphere.

If we remember that the earth is but a point in space, we may fairly conclude that the number of bodies composing meteor-zones is all but infinite. Large, therefore, as the numbers of these bodies which fall on the earth may be, there is no reason to suppose (perhaps if we knew the true functions of these bodies, we might say—there is no reason to fear) that the supply of meteors will ever be perceptibly diminished. Although the contrary opinion is often expressed, it is demonstrable that a very small proportion only of the shooting-stars which become visible to us, can escape from the earth's atmosphere. The result is of course that they must reach the earth, probably in a dispersed and divided state. It seems to me indeed not wholly improbable that some of those elements which the lightning-spectrum shows to exist in the atmosphere, may be due to the perpetual dissipation and precipitation of the substance of shooting-stars.

The remarkable discovery lately made, that the great November star-stream travels in the track of a telescopic comet (whose period is 33¼ years), that the August stream, in like manner, follows the track of the great comet of 1862 (whose period is 142 years), and that other noted shooting-star systems show a similar relation to the paths of other comets, opens out the most startling views of the manner in which cosmical space—or at least that part of space over which the sun's attractive power bears sway—is occupied by myriads on myriads of bodies more or less minute. If those comets—not one in fifty even of discovered comets—whose orbits approach that of the earth, are attended by such important streams of cosmic matter: if, for instance, the minute telescopic comet (known as I., 1866), in whose track the November meteors travel, is attended by a train capable of producing magnificent star-showers for nine hundred years—what multitudes of minute planets must be supposed to exist in the complete cometary system!

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METEORS AND METEOR SYSTEMS.

One of the most remarkable features in the history of scientific progress has been the slowness with which the full significance of important discoveries has been recognised even by the professed students of science. When a great discovery is made, one can understand that some delay should occur before the newly learned fact is accepted as a recognised truth; but when some great new truth has been admitted on all hands, it might be supposed that all the consequences which follow from that truth would at once be accepted; or rather that the students of science would vie with each other in pushing the search for such results to its utmost legitimate limits.

This however seldom happens. Whether it is that a discovery effected by another is regarded as not presenting an inviting subject of study and contemplation; or whether it is that men are ready to hope more from their own original researches than from work devoted to the investigation of the discoveries of others; or whether, lastly (but surely this cannot be the true interpretation), it is feared that all credit for results obtained by studying a truth discovered by another will be assigned to *him*—it is certain that we very seldom find the students of science willing to analyse the results obtained by other men. There are of course exceptions, and noble exceptions. The investigation by the Continental mathematicians of the results flowing from the law of gravity, the study by spectroscopists of the results flowing from Kirchhoff's great discovery, and some other cases may be cited. But unless a new truth is, as in these instances, of a very striking and even imposing nature, it is left very much to itself, and only by slow degrees are its fruits gathered in.

The discoveries recently made by Schiaparelli, Adams, and others, respecting the bodies called meteors (under which name may be conveniently included shooting-stars, aërolites, bolides, and the like), afford a very apt illustration of the peculiarity I have referred to. The consequences which flow directly from these discoveries, and still more those which may be legitimately deduced from them by careful reasoning, are full of interest, and bear in a most important manner on the economy of the solar system; nay, it needs but a moderate study of the subject to see that questions affecting even the relations of the interplanetary spaces are suggested by the discoveries which have recently been made respecting meteors and their motions. Yet but few among modern astronomers have been willing to make researches into these matters. Professors Herschel and Newton, Mr. Stoney, Sir John Herschel, and a few others, have dealt with the subject; but the great body of astronomers would seem almost to have forgotten that

Schiaparelli and Adams had made any important discoveries at all in this matter.

I propose briefly to describe the discoveries referred to, and then to consider some of the conclusions which may be deduced from them.

Not many years ago a comparatively insignificant position was assigned to meteors, regarded as members of the solar system. It was but recently, indeed, that these bodies had come to be looked upon as belonging to the solar system at all. From being regarded as a species of exhalations consumed during some sudden processes of change in the upper region of air, they had risen to the rank of volcanic missiles from the moon. Next, the occurrence of meteoric showers at certain definite times of the year—that is, as the earth traverses certain definite parts of her orbit—had compelled astronomers to recognise the fact that meteor systems must exist, which, regarded as systems, occupy a relatively fixed position in the solar system. The individual meteors may or rather must be in swift motion; and if a meteor system includes a swarm of meteors, then that swarm must also be in swift motion: but regarding the system as a whole, it must have the same sort of relative fixity which the earth's orbit itself has. Otherwise the occurrence of annual showers would remain unaccounted for; since we require that near a certain point the earth's path should be crossed or closely approached by the track of the meteors belonging to a system—and this not for a single year, but for many years or even centuries in succession.

Now, directing their search to other parts of the solar system, astronomers presently found what they took to be the analogue of the meteor families traversed by the earth. The zone of asteroids consists of a number of relatively minute bodies travelling around the sun. Distinct in all its characteristics from the family of smaller planets circling close by his globe, and equally distinct from the moon-attended family of outer planets, the asteroidal zone may be regarded as forming a family apart. We do not know how many members there may be in this family, nor do we know what may be the extreme limits of its range, either outwards towards the family of major planets, or inwards towards the terrestrial family of planets. But what we know respecting it teaches us to infer that if all the asteroidal orbits could be seen as rings of light around the sun from some distant station, the combined system of rings would form a ring system whose densest portion would appear as a nearly circular and somewhat flat zone between the orbits of Mars and Jupiter, and close to the plane of the ecliptic. And we have only to conceive the case of a large planet circling around the sun along the thick part of this ring to see that results analogous to those presented as the earth circles amidst the imagined meteor systems would inevitably follow.

Hence astronomers inferred that a number of meteor systems travelling in orbits of no considerable eccentricity occupy the region through which the earth's orbit passes. The boldest reasoned that in all probability the whole space between the earth's orbit and the sun was more or less occupied by these meteor systems, and that some of the systems might even pass to moderate distances outside the earth's orbit. The phenomenon known as the zodiacal light was associated with the existence of such meteoric systems, though the idea of such an association was scouted by not a few as wild and chimerical.

But the actual facts, as revealed by recent researches, are far more wonderful, whether we regard their direct significance or the conclusions which may fairly be deduced from them.

The history of these researches reads almost like a romance, so strange are the coincidences and the examples of 'good luck' which it presents to our consideration.

In the first place, the approach of the expected display of November meteors in 1866 led many astronomers to direct their attention very specially to the subject of meteoric astronomy. Not only were observations made much more systematically than in previous years, but the records of former phenomena were carefully searched, and in particular those which relate to the November system, or, as these meteors have been termed, the *Leonides*.[1] It was during this process, rendered necessary, as it seemed, by the difficulty of determining when and where the shower of 1866 would be seen, that the significant fact of a slow progression of the place where the earth encounters the system became known. Of old, setting

[1] Because they seem to radiate from the constellation Leo.

apart the difference of style, and also the effect of precession, the earth crossed the November system somewhat earlier in the year. In other words, she has now to travel somewhat farther forward (measuring her motion from the spring equinox,) before she encounters the *Leonides.*

There is nothing specially remarkable about this peculiarity. It corresponds to what is observed in the case of planetary orbits. These are continually—though slowly—shifting in position, their inclination changing within certain limits, and the line in which their plane crosses the medial plane of the whole system travelling round, though not without occasional cessation, and even retrogression, yet, on the whole, in one direction. But mathematicians began to feel hopeful of determining the true shape and position of the November meteor system when they thus saw that a measurable amount of perturbation affects the motion of its members.

A more striking feature of the system had before this attracted notice; I mean the recurrence of great displays at intervals of about a third part of a century. It was, indeed, this feature which had caused astronomers to look forward with so much interest to the display of 1866.

What might this periodic recurrence of great displays be assumed to mean? The natural answer to this question would have been that the meteor system circles once around the sun in about thirty-three years, but for a circumstance which was very justly held to be strongly opposed to such a solution of the problem. The period of revolution of any body round the sun tells us with absolute certainty the mean distance of the body's orbit. We have only, in fact, to multiply by itself the number representing in years the body's period, and to take the cube root of the product, in order to obtain the number representing the body's mean distance (the earth's mean distance being taken as unity). Now, when we multiply thirty-three by itself we get 1089, and the cube root of this is greater than ten; so that if the period of these November meteors were thirty-three years or thereabouts, their mean distance would be more than ten times the earth's; greater, therefore, than the mean distance of Saturn. But this by no means indicates the full extent of their range in space on the supposition implied; for since the earth encounters them, they come at least so near to the sun in one part of their course as to have their distance represented by *one*, or reduced from the mean distance by as much as *nine*. Hence at the opposite part of their course their distance must be just as much greater than the mean distance, *ten*, or must be as great as *nineteen* times the earth's mean distance. This would place the most distant part of their track as far off as the orbit of the planet Uranus.

Now, it certainly does appear that astronomers were justified in rejecting such a view as this in favour of hypotheses which gave a less extended sweep to the meteor orbits. Nor was it difficult to suggest other modes of explaining the observed peculiarity. Supposing the rich part of the system, instead of circling once round the sun in thirty-three years, or thereabouts, circled *once more* or *once less* than thirty-three times in that interval, then the recurrence of maximum displays at intervals of thirty-three years would be fully accounted for. It needs but a momentary consideration of the matter to see that this is so. In one case, at the end of the first year after a great display, the rich part of the cluster would have gone once round and a thirty-third part, at the end of the second it would have gone twice round and two thirty-third parts, and so on. Each year would find it advanced one thirty-third part farther, and at the end of the thirty-three years it would have completed the extra circuit. Indeed, the supposition, on the face of it, brings the rich part back to the place of encounter at the end of thirty-three years; but the above way of considering the matter shows also that this part of the system cannot be at the place of encounter, when the earth is there, during any of the intermediate years. And the like would occur on the second supposition, only in this case the rich portion would at the end of each successive year be found one thirty-third part behind instead of in advance of its original position.

It is well to notice, before passing to the work by which the true theory was educed, that Professor Newton, of America, was able in 1865, imperfect as our knowledge of the November meteor system then was, to calculate very closely the epoch of maximum display in November 1866. No other difference appeared in the observed and the predicted results, except that we in England, instead of Newton and his fellow-countrymen in America, witnessed that wonderful display. In other words, though the last great display had occurred more than thirty years before, though no one knew along what course the rich part of the meteor system had since been travelling, and though the extent and nature of the rich aggregation were also wholly unknown, the encounter between the earth and the long-absent cluster preceded the time predicted for it only by the brief interval separating the successive passages of England and America across a given rotation-space. If we imagine that from some distant orb a being were watching the event, knowing the nature of Newton's prediction and uncertain as to the result, then this being would have seen the Leonides rushing onwards to the scene of encounter on the one part, and the earth sweeping towards the same point on the other; he would have seen Asia passing round from the dark to the illuminated hemisphere, then Europe following, and the Atlantic coming round to that portion of the dark hemisphere on which the Leonides were preparing to fall. But while he would still be doubtful whether America would come round to the same side before the encounter took place, and so the prediction of the American astronomer be exactly fulfilled, he would see that all over Europe and the western parts of Asia, and in a less degree over the foreshortened Atlantic, the meteors were already falling; the display would grow richer and richer under his eyes as the meteors kindled in their swift flight through the resisting air; but after awhile it would diminish in splendour; and finally, just as America began to show on the exposed hemisphere, the encounter would come to an end, the earth passing onwards to the relatively barren regions lying beyond the meteor orbit.

In the meantime the astronomers of Europe had gazed their fill upon the strange scene. Thenceforward the true import of the before neglected falling stars began to be felt in full force. From one and from another came opinions—some fanciful, some well considered. And then began the work of survey. Observation had brought together a sufficient number of facts, and careful study was to educe from those facts something of their true value.[1]

It happened that one of the most striking facts learned about this time resulted from what was in

[1] Can anything be stranger than that in our day, after all the instances which should be so familiar to us of the practical value of the analysis of observations, there should still be some who venture to speak of such work with contempt? Yet again and again we hear those who study the results of observation decried as theorisers, and observation lauded as the sole means of advancing science. Those who speak thus are in reality those who have the lowest opinion of the value of observation, while those who examine and re-examine the evidence gathered together could not better show the high esteem in which they hold the work of observers. If Flamsteed had ridiculed Sir Isaac Newton for analysing the observed motions of the moon and attempting to found upon them the theory of gravitation, he would not have acted more unwisely, meseems, than those who urge, in our day, that observation alone is capable of advancing science. He could, indeed, have better justified himself for so acting; since, in his day, the evidence in favour of careful theorising was much less forceful than that which we possess.

truth but a daring guess. It referred to the August meteors, called sometimes the *Perseides,* because they seemed to radiate from the constellation Perseus.

The Italian astronomer Schiaparelli had been led to notice that a comet which appeared in 1862—a comet less brilliant, indeed, than either Donati's or the comet of 1861, but still a remarkable object, and conspicuous to unaided vision—had an orbit which approaches the earth's path nearly opposite the place she occupies on August 10—the date when the Perseides appear. It occurred to him to inquire whether, if one only supposed the meteors to travel on as eccentric a path as the comet, the deduced orbit would resemble that of the comet in position also. Let us briefly inquire what assumption was to be made, and what likelihood there was, that without any real association existing between the comet and the meteors, the remaining features of the orbits would seem to resemble each other pretty closely. The period of the comet was known to be about a century and a half, and the figure of its orbit is therefore so eccentric that in aphelion the comet is much farther off than Neptune. Hence the velocity of the comet where it passes close by the earth's track was readily calculated to be greater than the earth's velocity, about as fourteen is greater than ten. All that Schiaparelli assumed was that the August meteors have the same velocity (about twenty-six miles per second). But knowing their real velocity, we could, from their apparent course as they enter the earth's atmosphere, determine the real direction in which they approach us. And so, their real direction and their real velocity at the earth's distance being known, their orbit around the sun would be completely determined. The real velocity at that distance would not *alone* suffice to determine the orbit; for we can conceive bodies projected with that velocity from the point where the earth is, on or about August 10, in a myriad different directions, and all those bodies would travel on different paths, only possessing this common feature that their mean distances would be all equal to that of the comet of 1862. The longer axes of the paths thus determined might be directed towards any part of the heavens (towards any star, if we please). We see then that the antecedent probability was minute indeed, that when the real direction of the August meteors, as shown by the part of the sky they seem to come from, was taken into account, their orbit would seem to accord exactly with that of the comet of 1862, unless some real association existed.

But the paths were found to agree perfectly.

Schiaparelli himself, Leverrier, and a few other mathematicians of high repute, were at once convinced that the comet of 1862 and the August meteors are in some way associated. It is true that there was an initial assumption, and so far the force of the evidence seemed diminished. But to mathematicians accustomed to weigh the value of probabilities it was at once obvious that the evidence remained altogether too strong to be resisted. Accordingly we find that Leverrier about this time, seeing that the August meteors travel in an orbit of such enormous eccentricity, no longer regarded the 33-year period as an unlikely one for the November meteors; and he proceeded to calculate their path on the assumption that the recurrence of maximum displays three times in a century indicates the true period of the system.

But in the mean time the same result had been arrived at in a more direct and convincing manner by Professor Adams, co-discoverer with Leverrier of the distant Neptune. I have spoken of the slow progressive motion of the point in which the November meteor system crosses the earth's orbit. In this motion Professor Adams recognised a means of determining the actual orbit of the meteor system. For on any given assumption respecting the figure of this orbit it was possible to calculate the perturbing action of the planets, and so to deduce the rate at which the plane of the orbit would shift. It was possible, I say; but on certain assumptions as to the figure of the orbit, the calculation was by no means easy. In fact, it may be justly said, that to determine rightly the degree of perturbation of a ring of bodies whose orbit is assumed to extend from the earth's orbit to the orbit of Uranus would be a problem which would overtax the powers of the most subtle modes of mathematical analysis yet devised. I do not enter here into the nature of the methods by which Professor Adams dealt with this subject, my purpose being rather to discuss results than the means employed to obtain them. Suffice it that he showed, beyond all possibility of question, that the only orbit which will satisfy the observed relations is the one already referred to as carrying the meteors beyond the orbit of the planet Uranus.

It need hardly be said that the orbit thus assigned to the meteors was identical with that determined by Leverrier—the work of Adams altogether surpassing Leverrier's, however, in importance, since the English astronomer alone had *proved* that the meteors travel on this widely extended orbit. In fact, Professor Adams was the first astronomer who had ever undertaken to deal with meteors as members of the solar system, and the first, also, to determine from the perturbations of celestial bodies, the dimensions of the path along which they travel.

A telescopic comet, only discovered in 1866, was found to travel in the same path as the November meteors. In this case there was not even that doubtful feature which had seemed to render the aspect of Schiaparelli's discovery questionable to those unskilled in judging of probabilities. No assumption had been made as to the path of the November meteors. The path had been established first, and the comet searched for afterwards. The path of the comet, too, had not remained uncalculated until the orbit of the meteors had been determined. The two orbits were determined altogether independently of each other, and then—each complete in all its details—they were brought into comparison with each other. The agreement was practically perfect.

Now it is at this point that the above-considered unreadiness to trace out the consequences of an important discovery begins to be apparent. The work of Schiaparelli, Leverrier, Adams, and others having brought meteoric astronomy to this point, astronomers seem to have been, for the most part, willing to leave the matter *there.* Thus the August meteors are admitted to be bodies travelling along an orbit of enormous extent carrying them much farther away from the sun than the orbit of Neptune; the November meteors are admitted to be bodies travelling in an orbit extending beyond the orbit of Uranus; and the association of both meteor systems with the comets I have spoken of is also admitted. But as for the remaining meteor systems which the earth encounters on her course round the sun—fifty-six of which were known when Adams's researches were made, and more than a hundred of which are at present recognised—very little indeed (most astronomers seem to think) is to be regarded as known. Some few of these systems may *appear* to be associated with certain comets, and others again may appear, from the periodic recurrence of considerable displays, to travel in long periods, and therefore, in eccentric orbits around the sun; but until these things have been demonstrated they are not even to be admitted as probable. Still less are we to form any opinion respecting the possible existence of other meteoric systems than those which the earth is actually known to encounter.

This seems to me as little reasonable as though the law of gravitation had been restricted to the moon's motions round the earth until astronomers had been able to examine as closely the motions of every other celestial body as they have examined those of our satellite; or as though the laws of Kepler had been restricted to Mars until the same processes which Kepler had occupied nearly a quarter of a century in applying to that planet had been extended in all their fullness to the other planets.

We ought rather to take the general negatives implied by what has been discovered respecting the August and November meteors than to consider those particular positive results which Schiaparelli and Adams have established. Let us start from such propositions as these:

The meteor systems encountered by the earth are not necessarily of small eccentricity;

Their paths are not necessarily limited to the portion of space traversed by the earth and the three other minor planets, Mercury, Venus, and Mars.

And let us add to these propositions, which are the direct results of the discoveries considered above, one positive proposition also deduced directly from them, viz.:

The meteor systems hitherto considered (and therefore, presumably, others also) are associated with comets;

and this further proposition known before any of the others, but not at that time considered with sufficient attention,

The orbits of the meteor systems are inclined at all possible degrees of slope to the plane of the ecliptic —from coincidence with *direct* motion, through all angles up to a right angle; and beyond such an angle, down through all angles to coincidence again with retrograde motion.

Then these four propositions, rightly considered, will lead us directly, as I take it, to results of extreme importance, tending to modify altogether our conceptions of the condition of those portions of the sun's domain which we have been in the habit of regarding as untenanted.

Taking first a general view of the four propositions, we see that they all agree in assigning to the meteoric systems the characteristics of cometic orbits. There is no rule limiting the motions of the meteors in any of those ways in which the planetary motions are limited—that is, either as respects the shape, extent, or position of their orbits, or as respects the direction in which they travel.

Now, consider the case of a meteor system, whose orbit is to be assigned altogether at random, in accordance with what is taught by our four propositions. First, as to the shape and extent of the meteor system. If in aphelion its distance be greater than the earth's orbit, while in perihelion its distance is less, the system is such a one as the earth's orbit *may* intersect; but if both these conditions are not fulfilled, the earth's orbit cannot by any possibility intersect the system. The antecedent probability against an intersection of the orbits is on this account alone enormous. Next as to the slope and position of the meteor system. The plane of the system will intersect the plane of the earth's path in a line through the sun; and the meteor system itself will cross this line at two points on opposite sides of the sun. If the distance of one of these two points agree with the earth's distance from the sun, the meteor system will intersect the earth's orbit; otherwise not. The antecedent probability against intersection on this account also is enormous. Now the actual antecedent probability against intersection is obtained, not by *adding* these two enormous probabilities together, but by multiplying them together. For instance, if we set the chance that each condition would be fulfilled at one-1000th (altogether too high, be it noted), then the chance of intersection will not be one-2,000th, but one-1,000,000th.

Taking this value of the chance for the sake of illustration, as fairly representing a chance which in reality is very far smaller, we see that an enormous number of these random meteor systems would be required in order that there should be a reasonable probability of the earth encountering one of them. If we had a teetotum with a million faces, all different, we should require a large number of trials in order to have a fair chance of turning up some specified face.[1]

[1] It is easy to calculate the number of trials required in order that the chance may be exactly one-half. We have only to determine to what same power the numbers 999,999 and 1,000,000 must be both raised

And the case of the meteor systems (on the assumed and certainly unexaggerated estimate of the probability in question) is exactly parallel.

Now, considered in connection with the facts here dealt with, the observed facts that—

First, the earth encounters two meteor systems which are certainly very eccentric, very widely extended, and considerably inclined—after the manner of cometic orbits.

And secondly, the earth encounters more than a hundred meteor systems, which are probably no less cometic in all their characteristics, and many of which are certainly cometic as respects one characteristic,

in order that the raised value of 999,999 may be exactly half the raised value of 1,000,000. The number expressing this power will represent the number of trials required. It is easily shown that about three-quarters of a million trials would be required to give an even chance. It is worthy of notice that nearly every person unfamiliar with the theory of probabilities, to whom this question is submitted, will answer that half a million of trials will give an even chance. It is said that a notorious swindler once took advantage of the mistaken views commonly entertained on such points, in the following way. He agreed to give an opponent always three throws with a single die to turn any named face, these sets of three trials to continue for a sitting of many hours, and a stated sum to be won or lost by his opponent according as he succeeded or not in turning the named face. The swindler won in the long run a large sum; for three throws of a die are not sufficient to give an even chance of turning any specified face. Had the number of throws been four, the result would have been the other way; four throws giving more than an even chance. There is a very ready way of showing that three throws are insufficient. They give clearly the same chance as if three dice were thrown at once. Now *if* three dice thus thrown *necessarily showed different faces*, it is obvious that the chance that one of those faces would be the specified one, would be exactly one-half; so that the chance of the thrower has to be increased by this particular condition, to make it equal to the chance against him. Under the actual circumstances, then, the odds are against him.

being inclined considerably to the plane of the earth's orbit.

The legitimate conclusion from the estimated antecedent improbability that a meteor system supposed to be placed and shaped at random would cross the earth's orbit, and these observed facts, is beyond all question *this,* that the meteor systems actually encountered by the earth form but a minute proportion of the total number of meteor systems actually belonging to the solar domain. There is absolutely but one way in which this conclusion could be avoided, and that way, as we shall see, is barred. If we had reason to suppose that our earth had the power of forcing meteor systems to take up a position intersecting her orbit, we should no longer be compelled to believe that for each such system millions of others exist within the solar scheme. For in the course of long past æons a large proportion of the meteor systems might have been swayed into a partial subjection to the earth. If the earth, like Jupiter, for instance, surpassed in bulk and weight the combined mass of all the planets, we might suppose that to her own attractive energies alone the fact was due that so many meteor systems cross her path. Jupiter *has* swayed, and *still continues* to sway, comets in this way; and doubtless he has swayed, and still continues to sway, meteor systems into partial submission. But the earth's influence over meteor systems is relatively all but evanescent. She has had no part or share in swaying to their present position either of those two systems whose paths have been determined. Indeed, Leverrier has shown that the November system has been swayed into its present course by the planet Uranus. Most probably too—or rather, almost certainly—not one of all the meteor systems crossed by the earth has been brought appreciably nearer to her orbit by her own influence.

But as a commentary on these relations, let us consider how the two meteor systems whose orbits have been determined stand related to other planets besides the earth. There is the August system, as an illustration of a system taken at random. It does not pass near the orbit of any known planet of the solar system save the earth alone. Jupiter, mighty as he is, has had no power to bring the August system close to his orbit; Saturn, Uranus, and Neptune, have been equally ineffective. If these planets have inhabitants, the August meteor system is utterly unknown to them. The November meteor system, again, does not pass within millions on millions of miles of the orbits of any of the planets, save only the earth and Uranus.

Two, then, of the known meteoric systems—and two taken at random so far as the question we are upon is concerned—are so placed that their existence must remain wholly unknown to the inhabitants of all the other planets, except (as respects one system) the planet Uranus. Is not the conclusion legitimate, or rather—for there is no real question of the justice of the conclusion—is it not rendered perfectly clear, that an enormous proportion of the meteor systems recognisable from other planets are so situated that we can never become cognisant of their existence? A large proportion of the actually existent meteor systems also must be so situated that they cannot be recognised from any of the planets at all.

Here then we have, as a direct and legitimate conclusion from admitted facts, a view of the solar system which four years ago would have been justly regarded as too startling for belief. We see the vast gaps which separate planet from planet no longer untenanted, or only traversed by an occasional comet, but literally crowded with meteor systems. If a vast model of the solar system could be constructed, all the parts being justly proportioned on some such scale as is considered in the well-known description given in Herschel's *Outlines of Astronomy*, and if the orbit of every meteor system were represented in this model by an oval hoop made of the finest possible wire, justly placed and shaped, then the space around the sun, to a distance far exceeding the radius of the earth's orbit, would be an absolute network of these wire orbits. Nor would it be easy to say how far from the two-feet globe representing the sun some of the closed orbits would extend; while myriads of parabolic and hyperbolic wires would have to be introduced to indicate the paths of those meteoric clusters which assuredly approach from outer space and return to the star-depths, never again to visit our sun's neighbourhood. Amidst these millions of orbit-wires the paths of the planets, if similarly indicated, would be almost lost; though individually the least of the planets probably surpasses the combined mass of all the members of those meteor systems which belong specially to the solar domain.

It would, indeed, be difficult to determine, even approximately, the weight either of any meteor system or of the system of such systems circling around the sun. We know that among those meteoric masses which actually reach the earth there are some few of great weight. One at least has fallen whose weight amounts to about fifteen tons. Nor again can we regard the masses which are found in the earth after the explosion of a bolide as more than the fragments of much larger masses. But, on the other hand, the bodies which form such systems as the Leonides and the Perseides are for the most part exceedingly minute, insomuch that the weight of some of these bodies has been estimated at less than a single grain. Between these limits lie the meteors which explode in the upper regions of air, but without casting their fragments to the earth's surface in a solid form.

It is worthy of notice, however, that even if we set on one side meteoric displays properly so called, and the great aërolites which from time to time crash down upon the earth, we yet find abundant reason for believing that our earth alone, small as she is, grows yearly in weight by many tons through the downfall of meteoric matter. Professor Newton has calculated from perfectly reliable data that on an average in the course of a single day, 7,500,000 meteors large enough to be visible to the naked eye are consumed in the earth's atmosphere, and about 400,000,000 meteors such as could be seen with a telescope of moderate power. Now, if we consider the latter set to be equivalent to the former, and assign a single grain as the weight of each meteor visible to the naked eye, we deduce fifteen millions of grains as the earth's daily increase of weight. This is rather less than a ton. So that in the course of about three years the earth's weight must increase (even on the very low value here assigned to a meteor's weight) by a thousand tons; and in the course of the three thousand years during which astronomy has been a science the earth's weight must have increased a million tons.[1] The moon's mass in the same time would be increased by about a sixteenth part of this amount.[2]

If, then, the earth alone, in circling once around the sun, gathers up tons of meteoric matter, it will be conceived how vast must be the weight of that meteoric matter (light though its particles be) which

[1] This is a mere trifle compared with the earth's own weight, which is 6,000 millions of millions of times greater. Indeed, it may easily be shown that the actual increase of the earth's radius in this interval of 3,000 years would be about the 70,000,000th part of an inch.

[2] The dynamical effect of these increments would be an increase in the rate at which the moon circles around the earth. But the increase would be inconceivably minute; and the earth's mass must have been increased to a much greater extent if the actual observed excess of acceleration of the moon's motion over what the theory of gravity can account for, is to be explained in this way. Doubtless the retardation of the earth's rotation is the real explanation of the greater part of this acceleration, which comes therefore to be regarded as apparent only.

in the course of a year has been within the earth's mean distance from the sun, and how enormous must be the combined weight of all the meteor systems, when this abundance of matter is continually maintained, though the matter present in any one year has for the most part passed away before the next, and though year after year a proportion of the meteoric matter is withdrawn from orbital motion around the sun, and forced to form part either of his own mass or of the mass of some one of the orbs which attend upon him.

But the considerations which now urge themselves upon our attention are far too numerous and too important to be discussed at the close of an essay like the present. I leave to another occasion the study of details which bear in the most striking manner on the economy of the solar system. I would particularly point to the fact that the new discoveries altogether change the aspect of the planetary scheme. The solar system as seen by Kepler and Newton may be compared to the trunk and main branches of a mighty tree, which modern discoveries present to us as adorned with lesser branches, twigs, and foliage, a tree still living and still growing It may well be that as the study of astronomy proceeds we may recognise far more clearly and satisfactorily than now, the origin and the principles of the development of this mighty system.

Fraser's Magazine for February 1871.

PROFESSOR TYNDALL'S THEORY OF COMETS.

Astronomers have not hitherto been fortunate in their theories respecting comets. These mysterious objects present so many perplexing appearances, and seem regulated by laws apparently so incongruous, that it has not been found possible to form an hypothesis which shall account even for the most important cometic characteristics. Although some comets are the largest objects in the solar system, surpassing even the sun himself in volume, yet the most brilliant comets are outweighed (perhaps many million-fold) by the tiniest asteroid, or even by the least of those minute satellites which make up the ring of Saturn. Obeying the attractive influence of the sun as submissively as the most orderly of the planets, comets yet seem subject to other influences, repelling a portion of their substance with a force which seems a thousand-fold more intense than the attractive influence of gravitation. Lastly, while we have the clearest evidence that a portion of the light we receive from comets is reflected solar light, exactly like that which we receive from the planets, we yet have equally decisive proof that comets are also self-luminous objects. So contradictory and perplexing are the peculiarities of these mysterious entities.

It is clear that the problem presented by comets is one which requires for its solution a rare combination of powers and a widely extended range of research. The most profound acquaintance with physical laws is as necessary as a thorough grasp of the astronomical significance of cometic peculiarities. The ablest astronomer cannot hope to solve the problem by the unaided resources of his own science; nor can the physicist alone, however sound his knowledge, however clear his perceptions of the bearings of physical facts, or how great soever his skill in co-ordinating those facts into systematic hypotheses, hope to be more successful than the astronomer. The two, by working together, may at length succeed in mastering the problem which has, above all others, excited the curiosity of men of science, and more than any other has foiled their skill and ingenuity.

It is pleasing, therefore, to find one of the most eminent physicists of our day turning his thoughts to the solution of this interesting problem. As Sir John Herschel remarked, when Professor Tyndall first began to investigate another well-known scientific *crux*, so may we say with reference to Tyndall's researches about comets:—'The subject is one eminently calculated to set one thinking, and it seems to have had that effect upon Professor Tyndall to an excellent purpose.' We must rejoice that 'he has been brought into contact with' comets, 'and still more so if he should be led to any satisfactory explanation' of their phenomena.

It will be gathered that I am not able to recognise in the theory which I am about to describe the complete or even a satisfactory solution of the problem which has so long perplexed men of science. It was scarcely, indeed, to be expected that the class of researches which guided Professor Tyndall to the views he has put forward should lead at once to a solution of a problem of so much difficulty. Yet I believe that he has set us on the track of a useful and promising process of research, which, for anything that appears to the contrary, may eventually lead to the long-desired solution of that problem.

Let it be premised that the fundamental idea running through all the noble series of researches carried out by Professor Tyndall, depends, if I understand his words and works aright, on the analysis of the ultimate particles of matter by the action of æthereal waves. Professor Tyndall has grasped, perhaps more fully than any living physicist, the fact that the undulations of the æther—that subtle medium whose existence is only known through its effects—afford the best if not the only available means of analysing what Newton called 'the more secret and noble works of nature within the corpuscles.' What science is waiting for is the Newton of the infinitely minute, and Professor Tyndall will one day, perhaps, be recognised as the Kepler of the great system of science, which is only awaiting the fulness of time to reveal itself to us in all its grandeur. However this may be, it is certain that his researches are gradually unfolding before us highly important laws of molecular and atomic action.

Now amongst the most important considerations associated with this branch of inquiry, is that which assigns their various qualities to the three forms of undulation to which the æther is subject, viz., light-waves, heat-waves, and actinic-waves.

We commonly speak of light as if it were a simple emanation from certain bodies. But in reality the light emitted from the sun (to take an example) is intimately associated with the heat received from that luminary, and also with that particular form of force which is termed Actinism. We may look upon the sun, in fact, as a centre whence waves of disturbance are propagated in every direction through the æther. And these waves are of every degree of length between limits as yet undetermined. Speaking generally, the longest waves are the heat-waves, the medium waves are the light-waves, and the shortest waves are the actinic- or chemical-waves. But waves between certain limits of length combine all the three properties.

Now, to illustrate these waves, which are altogether too minute to be recognised by the senses (otherwise than through their effects), let us imagine a wide sea traversed by waves of various length, from the long mile-wide roller to the tossing billow, and thence to the ripple which courses swiftly along the heaving surface of billow and roller. Consider how various the effects of these various forms of disturbance. A *Great Eastern* on such a sea would remain uninfluenced by the billows, which would simply break against her sides as against a rock. But to the slow heave of the rollers the monster ship would sway responsive, and that with a force and energy of movement which would seem surprising to those who had watched her behaviour in a billow-tossed sea. A smaller ship would act differently. The long rollers would scarcely affect such a vessel. She would, of course, rise and sink as the crest and the valley of the roller successively passed under her, but she would not be *swayed* by the movement. It is to the rush of the billow that such a ship would respond. Wave after wave would add to or maintain the swaying motion, and the time of oscillation would indicate the particular length of wave corresponding to the swing of the ship. A chip or a cork floating on the same sea would be swayed neither by the roller nor by the billow, but would respond only to the ripples which suited its small oscillations.

Just so it is with the waves which traverse æther. Let light-waves or actinic-waves be poured in ever such enormous quantity upon a piece of ice, and it will remain unaffected by their action. Its molecules will not respond to the waves which produce luminous or actinic impressions. But the moment we suffer heat-waves to stream upon our piece of ice, its molecules begin to respond to the comparatively slow swing of the heat-waves, and when the energy of this molecular vibration has become sufficiently great, the ice melts. So also would it be with a mass of cloud or vapour. Mere light would not disperse the cloud, but to heat the cloud molecules respond at once, and after a while the liquid particles assume the state of invisible vapour.

Consider again the effect of light upon the eye. The molecular structure of the retina of the eye refuses to vibrate responsively to the longer forms of heat-wave, or again to the shorter forms of the chemical-wave. 'I have often permitted waves to enter my own eye,' says Professor Tyndall, 'of a power which, if differently distributed, would have instantly and utterly ruined the optic nerve, but which failed to produce any impression whatever upon consciousness, because their periods were not those demanded by the retina.'

Lastly, there are forms of matter, and it is with such forms that we have principally to deal in considering Tyndall's theory of comets, which respond neither to the heat-waves nor to the light-waves, but are influenced immediately by the action of the smaller actinic-waves. We know, indeed, that the photographer owes entirely to this peculiarity his power of obtaining sun-pictures of objects, since the actinic or chemical rays alone can produce those changes on which photographic action depends.

I may note also, in passing, the relation between æthereal wave-lengths and *colour*. The heat-waves belong to the red end of the prismatic spectrum, but extend considerably beyond it; the light-waves occupy the whole of the spectrum, as is proved by the fact that we can *see* every part of the spectrum, but they are most intense in the middle or yellow part of the rainbow-coloured streak of light; the chemical-waves belong to the violet end of the spectrum and extend considerably beyond it.

Now the discovery on which Professor Tyndall has based his theory of comets is this:—

Having charged tubes of glass with certain gases and vapours, which he wished to submit to the action of radiant heat, he thought it desirable, in order to render visible what took place within the tubes, to illuminate their interior with an intensely brilliant light. He made use, for this purpose, of the electric light. Now he found that as a general rule the vapours remained perfectly transparent. In some cases, however, a faint cloudiness showed itself within the tube. At first this appearance perplexed him; and it was some time before he was able to convince himself that the cloud *revealed* by the electric light was also *generated* by that light. Then he felt that 'the observation opened a new door into that region inaccessible to sense, which embraces so much of the intellectual life of the physical investigator.'

Let us read his own description of the processes by which he conceives the cloud to be rendered visible. 'To all appearance,' he remarks of the tube in which the vapour has been introduced, 'the tube is absolutely empty. The air and the vapour are both invisible. We will permit the electric beam to play upon this vapour. The lens of the lamp is so situated as to render the beam slightly convergent, the focus being formed in the vapour at about the middle of the tube.

You will notice that the tube remains dark for a moment after the turning on of the beam, but the chemical action will be so rapid that attention is requisite to mark this interval of darkness. I ignite the lamp; the tube for a moment seems empty; but suddenly the beam darts through a luminous white cloud which has banished the preceding darkness. It has, in fact, *shaken asunder the molecules of the vapour*, and brought down upon itself a shower of liquid particles which cause it to flash forth like a solid luminous spear.' 'It is worth while,' he adds, 'to mark how this experiment illustrates the fact that however intense a luminous beam may be, it remains invisible unless it has something to shine upon. *Space*, though traversed by the rays from all suns and all stars, is itself unseen. Not even the æther, which fills space, and whose motions are the light of the universe, is itself visible.'

And here let us pause for a moment to inquire how far what we have hitherto seen bears upon known facts respecting comets.

The light of the sun shines upon all parts of the space which surrounds him. There might be transparent vapours in enormous masses in any part of that space, sweeping around the sun with motions of inconceivable rapidity, and yet not a trace of their existence would be revealed to us, so long as the sun's rays were unable to change those vapours into clouds. Such vapours would resemble those which remain transparent when subjected to the action of Tyndall's electric beam. But if vapours resembling those which become transmuted into cloud under the same action existed in any part of the solar domain, there can be no doubt that his rays would render them visible precisely as the beam of the electric lamp renders visible the 'solid luminous spear' of Tyndall's experiment. Here, then, the fact is suggested as at least possible that comets may resemble the clouds which make their appearance when the electric light transmutes certain transparent vapours into visible nebulous matter.

And one peculiarity of comets accords well with this view. Professor Tyndall found that when he had reduced the amount of transparent vapour in the tube to a quantity bearing an indefinitely minute proportion to the mass of the air in the same tube, the cloud still made its appearance under the action of the electric light, but was so exceedingly delicate that the faintest light seen through it remained altogether undimmed. Now it is well known that comets present a feature precisely corresponding to this peculiarity of Tyndall's clouds. They have been known to pass over nebulæ of excessive faintness, not only without obliterating them, but without appreciably diminishing their light. This is the first of the interesting series of analogies on which Professor Tyndall's theory of comets has been founded.

According to this view, then, we are to look upon a comet as composed of a vapour which the sun's light is able to decompose: in fact, as an actinic cloud formed by the sun's decomposing power. The tail of the comet is not matter projected from the head, either by some power inherent in the comet, or by the repulsive influence of the sun, but is matter precipitated upon the solar beams which traverse the cometary atmosphere. It must be understood, according to this theory, that the comet's atmosphere extends not only to the tail, but to an equal distance on every side of the comet's head.[1] The sun's rays after passing through the comet are assumed to have a power which they do not ordinarily possess,—the power, namely, of drawing down upon themselves from the cometary atmosphere the matter which renders them visible. Let us see how Professor Tyndall accounts for this new power.

The condensation to which the formation of the visible cloud is due he finds to depend entirely on the action of the actinic-rays, and these rays are absorbed in passing through the vapour. Light-rays and heat-rays have no power so produce the effects described. Nay, the heat-rays have the power of dissipating the visible cloud when the actinic-rays are weakened. A sort of contest may in general be supposed to be going on between the heat-rays and the actinic-rays; and where one or other preponderates, there visible cloud is absent or present. Now Professor Tyndall assumes that the head and nucleus of a comet have the power of intercepting all or nearly all the heat-rays. Hence in the part of space which is screened by the head and nucleus, the actinic-rays are relatively more

[1] In many cases this would imply that some of the longer-tailed comets have had atmospheres surrounding and including the sun and all the planets within the orbit of Mars.

powerful, and are thus enabled to bring down from the interplanetary spaces the matter which renders the tail visible. Elsewhere the heat-rays prevent the formation of any such visible cloudy matter.

It will be observed that this theory accounts for many facts which had seemed very perplexing. When we remember that many comets have approached the neighbourhood of the sun with a tail streaming millions of miles (in one case two hundred millions of miles) behind them, and after passing perihelion (in some instances only a few hours later), have been seen with a precisely similar tail carried in front of them, so that, as Sir John Herschel remarked, the apparent motion of the tail resembles that of a stick whirled around by the handle, we cannot but look with satisfaction on a theory which promises to remove so serious a difficulty. For undoubtedly the formation of a tail in one direction, and the destruction of all vestiges of former tails which had projected in other directions, would be processes which might take place with all the rapidity with which light flashes through space, if Professor Tyndall's theory be true.

Unfortunately the theory is surrounded with many and grave difficulties.

In the first place there are cometic phenomena of which it fails to give account. The formation of the luminous envelopes which the nucleus throws off as the comet approaches the sun, is a process which by no means takes place with the rapidity which Professor Tyndall's theory seems to require. I would not lay much stress on this point, however. The envelopes are frequently separated from the head of the comet by dark spaces. Now the cloudy matter existing under the conditions described by Professor Tyndall might as the comet approached the sun be in part converted by the increased heat into invisible vapour. But no sufficient reason suggests itself why this vapour after rising towards the sun should be reconverted into visible cloud. Still more perplexing (remembering always Professor Tyndall's assumption as to the nature of the vapour) seems the repetition of this process, often seen to result in the formation of several distinct envelopes.

Nor must we conceal from ourselves the fact that the appearance presented during the development of the tail is as though the matter of the envelope were being driven away by some powerful repulsive influence proceeding from the sun. It is impossible to look upon some of the drawings which experienced observers have made of comets, without feeling that processes of considerable violence are at work in the formation of the tail. I am aware that appearances of this sort are very apt to be deceptive, and therefore lay the less stress upon the evidence they afford. Still these appearances require to be considered in forming a theory of comets. There is nothing in Professor Tyndall's theory to afford any satisfactory explanation (so far as I can see) of the strange variety of forms observed in the heads, envelopes, and tails of comets.

According to the theory, 'old tails' are dissipated by the heat-waves, so soon as these pass clear of the head towards the space occupied by the part of the tail which is to be dissipated; and Professor Tyndall accounts for the apparent bending towards the end of the tail as arising from the finite though small period occupied by the heat-waves in travelling down to the tip of the tail. It is not, therefore, the ordinary progress of the heat-waves which is in question. For heat-waves travel as fast as light-waves, and would therefore traverse the length of a comet's tail of unusually large dimensions in less than ten minutes (in which time light, as we know, would travel more than one hundred millions of miles). Hence the utmost curvature we could allow the tail from this cause is such that the direction of the tip of the tail, instead of pointing towards the actual position of the head, would point to the position the head had occupied ten minutes before. Such a deviation would be altogether inappreciable (save in one or two exceptional instances, in which, however, the contrary would only hold for a very brief interval of time); yet we know that comets' tails are often curved in a very perceptible manner, and this during the whole time of the comet's visibility. But this difficulty is removed, if, as Professor Tyndall believes, the rate at which the tail is rendered apparent (or formed, we may say) may be comparatively slow or practically instantaneous; the like holding of the rate at which the old tails are destroyed. A little consideration will show that in this case, comets' tails ought in many instances to present the appearance of transverse streaks. As a matter of fact, the tail of Donati's comet did present such an appearance; a fact which seems to supply somewhat remarkable evidence in favour of Tyndall's theory.

It may be mentioned that Benedict Prévôt long ago suggested a view so closely resembling Professor Tyndall's (though inferior in the all-important respect that it was a mere speculation, not an hypothesis founded on observed relations) that the same arguments available against one may be urged with apparently equal force against the other. He considered that the head of a comet is converted by the sun's heat into invisible vapour extending to an enormous distance from the head in all directions. Behind the head this vapour is cooled, because it is sheltered from the sun's heat. It therefore condenses into cloud, which reflects light, and forms the comet's tail. This cloud he assumed to be dissipated precisely as Professor Tyndall assumes the old tails to be destroyed.

Dr. Huggins, whose spectroscopic researches have given us the first real facts we have obtained respecting the structure of comets, remarks that Prévôt's theory is 'obviously inconsistent with the observed appearances and forms of the tails, and especially with the rays which are frequently projected in a direction different from that of the tail, with the absence of tail immediately behind the head, and with the different degrees of brightness of the sides of the tail.'

The last two peculiarities seem wholly inexplicable on Tyndall's hypothesis, and therefore it may seem unnecessary to consider the first. I may as well remark, however, that there is a possibility of explaining the existence of subsidiary tails in certain directions, as due to the refractive power which irregularities in the head may exert on rays passing through it; or we may even suppose that the brighter planets (which undoubtedly reflect actinic rays, since it has been found possible to photograph these bodies) may in certain cases have caused these smaller tails by pouring their rays through the head of the comet in the same manner as the sun is supposed to do according to the theory, though with less energy.

The existence of subsidiary tails or multiple tails generally is indeed at least as inconsistent with the idea of a repulsive force exerted by the sun, as with the 'negative shadow' theory. We *can* understand that light should be so refracted in its passage through the head of a comet (with its envelopes within envelopes and central spherical nucleus) as to be sent off, according to the part of the head on which it fell, in the various directions actually observed in several instances; whereas a repulsive action exerted by the sun on the matter thrown off from the head seems wholly inconsistent with subsidiary tails stretching directly from the comet's head at a considerable angle with the principal tail.

That the luminous envelopes have the power of absorbing or reflecting certain rays and suffering others to pass through them is accordant with observation. It is certain, for instance, that the brilliant comet called Donati's (which appeared in 1858) did not reflect the actinic rays, since Dr. De la Rue was unable to photograph this object. He exposed a sensitised collodion plate to the action of the comet's light, in the focus of his ·13-inch reflector, for three minutes, without obtaining the slightest trace of an image, though a small star which happened to be close to the comet left its impression twice over (the clock-work having received a slight disturbance). And again, after exposure for fifteen minutes, during which time the faint luminosity of the sky had appreciably affected the collodion-plate, the comet obstinately refused to leave any trace of its figure. We see then that in this case (and doubtless in many others, if not in all cases) the actinic rays passed freely through the matter which reflected the light-waves to us, and so rendered the comet visible.

We must not forget the evidence which the spectroscope has afforded respecting the structure of comets.[1] We have learned, by means of Dr. Huggins's observations with this instrument, that the nucleus of a comet consists (at least in every case yet observed) of self-luminous gas. In one case it has even been found possible to determine the exact nature of the gas, and thus we are able to pronounce that Winnecke's comet (which appeared in 1868) consists of the luminous vapour of *carbon*. The *coma*—that is, the faint light

[1] See the preceding paper.

around the nucleus—is found, on the other hand, to shine in part by reflecting solar light. Of the tails of comets we have as yet learned nothing, and we must wait for the appearance of a brilliant and long-tailed comet before hoping for definite information respecting the nature of these appendages.

Another fact, which must not be left out of consideration in forming a theory of comets, is that which was discovered in 1866-67 by the united labours of Peters, Tempel, Schiaparelli, Adams, and Leverrier, but must be held to be more intimately associated with the name of Professor Adams than with that of any other astronomer. I refer to the remarkable relation between comets and meteor-systems, according to which meteoric bodies are found to travel in the same orbits as certain comets. How it comes about that the track of vaporous bodies like the comets should be followed by numbers of minute solid bodies such as the meteors, it would be difficult to explain in the present state of our information respecting comets. But no theory of comets can be considered complete in which this relation is left unaccounted for.

It is evident that he who would form a consistent and satisfactory theory of comets will have no easy task. In the absence of definite information on many points, it seems at present even hopeless to attack the question. Doubtless, as Dr. Huggins has remarked, 'we must wait for further positive knowledge of the nature of cometary phenomena, until the searching method of analysis by the prism can be applied to the series of changes presented by a brilliant comet.' Then we require further knowledge respecting the relation between meteors and comets, and between both these classes of bodies and that strange phenomenon the zodiacal light, the peculiarities of which will be found, I venture to predict, to be much more intimately associated with cometic phenomena than is at present commonly supposed. Yet again, we must make an approach towards mastering the relations which exist between the sun's action as a centre of many forms of force, and the phenomena of terrestrial magnetism, looking upon these phenomena as indicative of processes which affect the whole solar domain. When we remember that the appearance of intensely brilliant light-patches on the sun's orb, has been found to be accompanied by an instantaneous thrill of the whole magnetic frame of the earth, presently followed by the appearance of auroral lights in both hemispheres, we recognise the action of solar influences which must be capable of largely affecting such bodies as the comets.

But again, in forming a theory of comets, account must be taken of every phenomenon of importance which these bodies have exhibited to the telescopic observer. The jets of light which the nucleus seems to throw out towards the sun, the mode in which the envelopes are formed round the head, the peculiar distribution of light and shade across the breadth of the tail, the dark space behind the head, the strange configuration of the tail, and the occurrence of multiple, and sometimes even of *abnormal* tails, must all be taken fully into account. The yet more perplexing phenomenon of the breaking up of a comet into two distinct comets, each with its own nucleus, coma, and tail, and even—if ancient records can be trusted —the formation of a multiple system of comets out of a single comet, must also be interpreted. And many other matters, which it would be tedious to enter upon here, must be explained satisfactorily before any theory of comets can take its place in the rank of physical truths.

In conclusion, I must remark that it would be unfair to form an estimate of Professor Tyndall's views—or at any rate to decide finally on their value—until he has had time to arrange and co-ordinate them with reference to all the facts which lie at his disposal. It is not to be expected, and he doubtless would be the last to suppose, that a discovery so recently made as the one on which the theory is founded, should in a moment remove all the difficulties and reconcile all the incongruities presented by cometary phenomena. If we were to estimate the theory as at present exhibited, we could hardly look upon it (based though it be on observed facts) as other than a highly ingenious speculation. It is because I look upon the views which Professor Tyndall has brought before the scientific world, as affording promise of further researches on the same subject, and that such researches made by such a physicist as Professor Tyndall cannot fail to bear useful fruit, that I have dealt at length with views which, however ingenious, must be looked upon at present as speculative. It must be remembered, also, that astronomers have not been so successful in theorising respecting comets, that they can claim (or afford) to reject the assistance which one of the most eminent of living physicists is offering them in the treatment of a question which they have been too much in the habit of considering as peculiarly their own.

Fraser's Magazine, October 1869.

COMETS AND COMETS' TAILS.

Among the many startling suggestions recently thrown out by men of science, not one, perhaps, has seemed more amazing to the general public than the idea put forward by Sir W. Thomson in the able address with which he inaugurated the late meeting of the British Association—that life on the earth may have had its origin from seeds borne to our planet by meteors, the remnants of former worlds. Coupling this startling theory with the partly-admitted view that the tails of comets and comets themselves consist of meteoric flights, he presented the 'hairy stars' which men so long viewed with terror in a somewhat novel light. Regarded not so many years ago as probably the vehicles of the Almighty's wrath, comets are made by this new hypothesis to appear as the parents of universal life. How would Whiston, and those who thought with him that a comet in old times effected the destruction of all living things (save a chosen few) with water, and that a comet at perhaps no very distant future would destroy the whole earth with fire, have contemplated a theory according to which the seed-bearing fragments of a comet's tail peopled the earth with all the living things which at present exist upon its surface? The 'fear of change' with which in old times comets perplexed the nations must be replaced, it would seem, by another sort of fear. We need not dread the approaching dissolution of the world through cometic agency, though the thought of a vast catastrophe may be suggested by the consideration that we see in the comet but the fragments of another world. But if this new theory should be accepted, we have reason to regard with apprehension the too close approach of one of these visitants; because, if one comet supplied the seeds of the living things now existing on the world, another may supply myriads of seeds of undesirable living things; and perhaps the sequent struggle for life may not result in the survival of the fittest.

It is hardly necessary for me to say, perhaps, that I am not troubled by such misgivings. I can scarcely bring myself to believe, indeed, that the eminent professor was serious in urging his hypothesis of seed-bearing meteors. Englishmen speak sometimes of the slowness with which a Scotsman apprehends a jest; but the Scotsman may return the compliment—so far, at least, as the southern estimate of Scottish humour is concerned. For a true Scot makes his jests with a gravity and *aplomb* unequalled among Sassenach humorists. It is far from improbable that the seriousness with which the seed-bearing meteorites have been discussed proved infinitely amusing to the gathering of the clans in Edinburgh. Thomson and Tait, Andrews, Geikie, and Stewart, in fine, all the Scottish men of science who were present at the gathering, may be ready to retort Sydney Smith's gibe, maintaining henceforth that nothing short of a surgical operation will enable an Englishman to appreciate Scottish humour.

For it will be noticed that the explanation of the origin of life upon our globe leaves the real question of the origin of life where it was. The theory, in this respect, resembles that undoubtedly humorous account which the Hindoo sages gave of the manner in which our earth is supported; and precisely as the Hindoo student of science might ask how the tortoise who supports the earth is himself supported, so may we ask how the worlds which, by bursting, supplied space with seed-bearing meteors, were themselves peopled with living things. This circumstance of itself throws an air of doubt over the new hypothesis, as a seriously-intended account of the origin of life on our earth. It may seem superfluous to add that in a collision by which a world was shivered into fragments the seeds of life would have what may be described as a warm time, since the collision could hardly fail to vaporise the destroyed world. The fiery heat generated by the collision, followed by a voyage during myriads of millions of ages through the inconceivable cold of space, and lastly, by the fierce heat which accompanies the fall of meteoric masses upon our earth, would seem so unfavourable to the germs of life, that Pouchet himself might accept with confidence the belief that all such germs had been completely destroyed before reaching this planet.

But while the theory of seed-bearing meteors can hardly be regarded as a complete solution of the perplexing problem of the origin of life, the facts to which the eminent Scottish professor referred while discussing it are of singular interest and importance. The whole history of recent scientific research into the subject of the relation between meteors and comets is full of instruction. The subject is discussed in two preceding papers; and in my 'Essays on Astronomy' there are four papers containing a full account of the researches of Schiaparelli, Adams, Leverrier, and those other men of science who have placed meteoric astronomy in its present position. I propose here, therefore, to take for granted many of the conclusions dealt with in my former paper. This will enable me to discuss with greater freedom, as regards space, the views respecting comets, and more especially respecting cometic appendages, which seem to be suggested by observed phenomena, taken in connection with the association recently recognised between comets and meteors. The subject is as yet too new for the enunciation of definite theories, and still less can we safely dogmatise respecting it. But much has been established which will well bear careful investigation, and I believe that the conclusions which may be fairly deduced from observations already made are much more important than is commonly supposed.

The phenomena presented by comets have long perplexed astronomers. Setting aside the fact that the head of a comet strictly obeys the law of gravitation, there is scarcely one known fact respecting comets which astronomers have succeeded in interpreting to their satisfaction. The facts recently ascertained, striking and important though they undoubtedly are, yet not only fail to explain the phenomena of comets, but are absolutely more perplexing than any which had before come to light. The present position of cometic astronomy is, in fact, this:—Many facts are known, and many others may be inferred; but these facts have yet to be combined in such a way as to afford a consistent theory respecting comets.

It is now known that the comets which are so brilliant as to attract general notice are but a few among those which actually approach the earth. The telescope detects each year (with scarcely an exception) more than one comet. It is probable, indeed, that if systematic search were diligently made, many comets would be detected yearly.[1] Already, however, nearly seven hundred comets have been discovered, of which by far the greater number have been the reward of modern telescopic research.

Of observed comets, only the more brilliant are adorned with tails of considerable length. But nearly all comets show, during their approach towards the sun, a certain lengthening of their figure, corresponding to the change which, in the case of larger comets, precedes the formation of a tail. So that a tail may be regarded as a normal, or at least a natural, appendage of comets—though special conditions may be

[1] A prize has been offered to the astronomer or telescopist who shall first succeed in discovering eight comets within the year.

requisite for the evolution of the appendage. This will appear the more probable when the fact is noted that, in all cases where a tail is formed, this tail appears as an extension of the part of the head known as the *coma* or hair—the fainter light surrounding the *nucleus* of the comet—and no comet has ever appeared without showing a coma during one period or another of its existence. Commonly, the coma continues visible as long as the comet itself can be discerned, though there have been instances in which the comet seems to have been shorn of its hair; and, in one noteworthy instance, a comet of considerable splendour lost in a few days both its tail and hair.

Now when we consider the remarkable appearance which the tails of comets have presented, the great variety of their aspect, and the wonderful changes which have been noted in the appearance of one and the same comet, we begin to recognise the enormous difficulty of the problem which astronomers have to solve. It will be instructive to discuss some of these peculiarities at length, because they seem to oppose themselves in a very striking manner to theories which have been somewhat confidently urged of late.

In the earliest ages of the history of our subject, the fact was noted that the tails of comets commonly lie in the direction opposite to the place of the sun. Appian, indeed, was the first European astronomer who observed this peculiarity, but M. Biot has succeeded in proving that the discovery had been made long before by Chinese astronomers.

If the tail of a comet strictly obeyed this rule, if it were always directed in a perfectly straight line from the sun's place, the peculiarity might admit perhaps of a tolerably simple explanation. This, however, is not in general the case; in fact, I do not know of a single instance in which a comet's tail has extended exactly in the direction of a line from the sun throughout the tail's whole length. The tail of an approaching comet generally seems to bend towards the track along which the comet has recently passed, and the effect, when the tail is long, is to give the appendage a slight curvature. To cite only one instance out of many, it will be sufficient to refer to the splendid comet which appeared in 1858, and was known as Donati's. Soon after the first appearance of the tail a slight curvature could be recognised in the appendage; and this curvature became gradually more and more conspicuous, until, to use Sir John Herschel's words, the tail 'assumed at length that superb aigrette-like form, like a tall plume wafted by the breeze, which has never probably formed so conspicuous a feature in any previous comet.'

Here is a peculiarity which at once serves to dispose of the theory according to which the tail of a comet is to be compared to a beam of light such as a lantern throws amid darkness. The theory seems so naturally suggested by the general fact that a comet's tail tends from the sun, as to lead many to forget that the so-called beam of light thrown by a lantern is in reality due to the illumination of material particles;

and that in the case of a comet we can neither explain why particles *behind* the comet (with regard to the sun) should be more brilliantly illuminated than others, nor how the particles come to be there at all. Despite these and other difficulties, the 'negative shadow' theory, as it has been called, has been again and again urged, though only to be again and again refuted.

Let it be noted, however, before other peculiarities are considered, that the curvature of comets' tails is no argument against the ingenious theory by which Professor Tyndall has endeavoured to explain their direction from the sun. According to this theory, the passage of light through and beyond the head of the comet is the real cause to which the appearance of the tail is to be ascribed. But a physical process is supposed to occur as the light traverses the region behind the comet; and the rate at which this process takes place need not necessarily correspond to the enormous velocity with which light travels. So that, instead of the whole tail being exactly in a straight line with the head and the sun, as it must be (appreciably) if the phenomenon were a mere luminous track, the end of the tail (the part formed earliest) would lie in the direction of a solar ray through the place occupied *some time earlier* by the head. This, in fact, corresponds somewhat closely with observed appearances; and so far Professor Tyndall's theory receives undoubted support from recognised facts.

Indeed, we seem almost driven to the conclusion that some such action as Tyndall has conceived takes place in the formation of a comet's tail—that either light, or electricity, or some swiftly-travelling cause, is at work—by the marvellous rapidity with which in some instances the tail of a comet has seemingly changed its position. The comet of 1680, commonly known as Newton's comet, affords a remarkable instance of this. I take the following narrative from Sir John Herschel's 'Familiar Lectures,' article 'Comets,' noting that the student of the subject, and especially the student of those theories which have of late been advanced respecting comets, would do well to study that paper carefully, as well as the chapter on 'Halley's Comet,' in Herschel's volume on his Cape Observations:—'The comet passed its perihelion (that is, the point of its course nearest to the sun) on December 8, and when nearest to the sun was only one-sixth of the sun's diameter from his surface'—travelling at the rate of 1,200,000 miles an hour. '*Now observe one thing,*' says Herschel; 'the distance from the sun's centre was about one 160th part of our distance from it. All the heat we enjoy on this earth comes from the sun. Imagine the heat we should have to endure if the sun were to approach us, or we the sun, to one 160th part of its present distance. It would not be merely as if 160 suns were shining on us all at once, but 160 times 160, according to a rule which is well known to all who are conversant with such matters. Now that is 25,600. Only imagine a glare 25,600 times fiercer than that of an equatorial sunshine at noonday, with the sun vertical. And again, only conceive a light 25,600 times more glaring than the glare of such a noonday! In such a heat there is no substance we know of which would not run like water—boil—and be converted into smoke or vapour. No wonder the comet gave evidence of violent excitement, coming from the cold region outside the planetary system, torpid and icebound. Already, when arrived even in our temperate region, it began to show signs of internal activity; the head had begun to develop and the tail to elongate till the comet was for a time lost sight of. No human eye beheld the wondrous spectacle it must have offered on the 8th December. Only four days afterwards, however, it was seen; and its tail, whose direction was reversed, and which, observe, could not possibly be *the same tail* it had before—(for it is not to be conceived as a stick brandished round, or a flaming sword, but fresh matter continually streaming forth)—its tail, I say, had already lengthened to an extent of about ninety millions of miles, so that it *must* have been *shot out* with immense force in a direction *from* the sun, *a force far greater than that with which the sun acted on and controlled the head of the comet itself, which, as the reader will have observed, took from November* 10 *to December* 8, *or twenty-eight days, to fall to the sun from the same distance, and that with all the velocity it had on November* 10 *to start with.*'

My readers will doubtless remember that in his address to the British Association Sir W. Thomson referred to the above passage, with the express object of commending the simplicity with which a theory lately suggested by Professor Tait seems to explain all the facts referred to by Sir John Herschel. According to this theory the tail of a comet consists of a multitude of meteors, travelling in a sort of flat flight, like sea-birds; and the seemingly rapid extension of a comet's tail is not due to the rapid projection of matter in a direction from the sun, but merely to a shifting of our position with respect to the level of the meteoric flight. Precisely as a flight of birds, scarcely visible when its level is slanted, may become visible along its entire length when the level is turned edgewise towards the observer, so a change of the earth's position, bringing her nearer the level of a meteoric flight, might cause the whole length of the flight to become visible, and thus an appendage of the nature of a tail might seem to grow with inconceivable rapidity, although in reality it had existed with the same degree of extension before it became visible to us.

This theory—to which, says Professor Thomson, the name of 'the sea-bird analogy' has been given—has not yet found a place in treatises on astronomy; and, with all deference to its author, I would submit that astronomers are not to be blamed for rejecting it. Its simplicity is great, no doubt; but its adequacy to account for cometic phenomena may be more than questioned. It seems barely equal to explain the visibility of a comet's tail, account being had of the enormous number of meteors which would be required that the reflected light might be recognisable even when the flight was seen edgewise. But it offers no explanation whatever of the direction in which comets' tails are commonly seen—still less of the generally observed curvature of the tail. And if we take the special account from which Sir W. Thomson has drawn reasons for favourably commenting on Tait's theory, we shall certainly find much in Sir John Herschel's narrative to throw doubt on the 'sea-bird' theory. For the tail of the comet (regarded as a real entity) swept round like a brandished stick—so that either continually new flights of meteors were seen successively edgewise, the order of succession being such as to correspond to the changing position of the tail, *or else* the same flight—remaining throughout so placed as to be seen edgewise—swept round as described. Now the latter view may be dismissed at once. It is the essential point of Herschel's reasoning, and is clearly demonstrable according to the laws of motion, that no meteors which were behind the comet before its approach to the sun could be 90,000,000 miles in front of the comet only four days after that approach—in other words, no meteors forming the tail in the first position could have reached a position undoubtedly occupied by *some* meteors (on the supposition we are considering) four days afterwards. As for the former view, according to which the tail after the comet's passage by the sun was formed of other flights of meteors than had formed the tail before this passage, it must be rejected on account simply of its being utterly incredible. If the comet had been thus girt about by meteor systems, the sun himself would have been darkened as the comet swept past. And even if we admitted these multiple flights in this and other instances (for Newton's comet was not the only one which has exhibited this peculiarity), it still remains utterly unintelligible why the flights behind a comet should be visible while the comet is approaching, and those in front of the comet while the comet is passing away.

The actual facts respecting the seeming motions of a comet's tail are, indeed, not always adequately realised by students of astronomy. We so often hear a comet's tail described as a vast stream of light extending behind the comet—like the wake behind a swiftly-sailing ship—that we are apt to forget that in reality it is only while a comet is approaching the sun that the tail even approximates to this rearward position. So soon as the comet has commenced its journey away from the sun, the tail is carried in advance—more and more in advance as the comet gets farther and farther away—until at length the tail lies nearly on the track which the comet is about to follow. At this time the comet's head is moving almost as if it were about to rush into the body of the tail.

But it is noteworthy that the tail of a comet at no time agrees in position with any part of the path of the comet. So that if we accept as strictly true the theory that certain meteor systems—as notably those which produce the August and November showers—follow *exactly* in the path of certain comets, we are bound to accept the conclusion that whatever the connection between the comet and meteor system may be, the meteor system is certainly not the comet's tail.

We are thus led to inquire into the circumstances which attend the formation of a comet's tail. We have seen how the tail behaves, and how its motions appear to suggest the idea of a force of some sort exerted repulsively by the sun. Let us inquire whether the telescopic scrutiny of the comet's head appears to confirm this idea.

No comet was ever studied so carefully with high telescopic powers as the splendid comet of 1858 already referred to. The remarks of Sir John Herschel on the subject of the drawings executed by Professor Bond,[1] of America, may still be quoted without a word of change; the series of engravings in which the comet is represented in every stage of its progress still 'leaves far behind—in point of exquisite finish and beauty of delineation—everything hitherto done in that department of astronomy.'

Like all large comets, Donati's, when studied with powerful telescopic means, showed a capping or envelope of light around the bright central nucleus. This envelope was separated by a dark interval from the nucleus; but a connection could be traced between the two in the form of jets of light which seemed to issue from different parts of the nucleus, 'giving rise,'

[1] The telescope employed by Professor Bond, of America, was a fine refractor, 15 inches in aperture, similar in all respects to the celebrated Poulkowa refractor, and to the fine telescope which is generally called the Great Equatorial of the Greenwich Observatory.

says Sir John Herschel, 'by their more or less oblique presentation to the eye, to exceedingly varied appearances—sometimes like the spokes of a wheel or the radial sticks of a fan, sometimes blotted by patches of irregular light, and sometimes interrupted by equally irregular blots of darkness.' A month and a half after the first appearance of the tail, the nucleus was seen to be surrounded by no less than three distinct envelopes, each of the two outer being related to the next inner envelope in the same way that the innermost was related to the nucleus; that is, there was a dark intervening space crossed by radial streaks of light. Professor Bond considered that these 'had been thrown off in intermittent succession, as if the forces of ejection had been temporarily exhausted, and again and again resumed a phase of activity; the peculiar action by which the matter of the envelopes was ultimately driven into the tail, taking place, not on the surface of the nucleus, but at successively higher levels.' But Sir John Herschel, from whom the above account of Bond's ideas has been taken, considered rather that the matter forming the envelopes was, as it were, *sifted* 'by solar action—the *levitating* portion of it being hurried off, the *gravitating* remaining behind in the form of a transparent, gaseous, non-reflective medium.'

Only a few days after the formation of these three envelopes, a striking change took place in the telescopic aspect of the comet, or rather in the aspect which it presented when seen, even with the naked eye, in a clear atmosphere. A new tail made its appearance beside the main or primary tail. The new tail was perfectly straight, and very narrow, and, unlike the primary tail, was directed almost exactly from the sun. Soon after another tail, similar in its general appearance, but somewhat fainter, was discerned. This tail was seen on one or two subsequent nights; but only when the atmospheric conditions were very favourable. 'These appearances were presented,' says Sir John Herschel, 'from the 28th September (1858) to the 11th October. They are peculiarly instructive, as they clearly indicate *an analysis of the cometic matter by the sun's repulsive action*—the matter of the secondary tails being evidently darted off with incomparably greater velocity (indicating an incomparably greater intensity of repulsive energy) than that which went to form the primary one.' Sir John Herschel does not notice the seeming connection between the appearance of these new tails and the formation of the additional envelopes. The three envelopes were first seen on the 24th September, and remained visible until the 10th October. The new tails were first noticed on the 28th September, as though some little time had been occupied in their formation from the matter of the outer envelopes, and they continued visible till the 11th of October, or one day longer than the envelopes, as though some interval were required for their dissipation. This circumstance seems highly significant, more especially when it is considered in connection with the condition of the head during the continuance of the triple envelope. For during this interval, 'and especially,' says Herschel, 'from the 7th to the 10th of October—that is to say, when the full effect of the sun's perihelion action had been endured—the nucleus offered every appearance of most violent and, so to speak, angry excitement, evidenced by the complicated structure and convolutions of the jets issuing from it. From this time,' he adds, 'until the comet's final disappearance, the violence of action gradually calmed down, while the comet itself went southwards, and at length vanished from our horizon.'

I would notice in passing that the circumstances here related seem to throw some light on a phenomenon which has hitherto proved most perplexing—the appearance of comets having multiple tails. The accounts which have been given of such comets seem utterly inexplicable, unless we adopt a theory resembling that which Sir John Herschel has touched on in the passages I have quoted. The comet of 1807 had two tails, neither of which agreed exactly with a line tending directly from the sun. The comet of 1823 had in like manner two tails; but the position of one of these was wholly abnormal, since this tail was directed *towards*, instead of from the sun. This might perplex us, were it not for the observed fact that the repulsive energy by which (in whatever way) the sun seems to sweep from his neighbourhood the matter of comets' tails, seems to struggle in the first place with a tendency in the matter of the comet's head to form one or more jets *towards* the sun. We may suppose that the tail directed towards the sun was simply a jet of this sort, able (owing to some exceptional feature in its constitution) to resist the sun's repulsive action. Side tails have been noticed in several instances—a fact which seems readily explicable by Herschel's theory. Less intelligible at first sight is the account of the great comet of 1843 as seen at Chili; for this comet is said to have had 'a lateral tail issuing from the original one at a distance of ten degrees from the head, and extending to a much greater length than the other.' It seems reasonable to suppose that in this instance two sorts of matter had been entangled together, as it were, when first swept away from the head, a separation only taking place after they had already been carried together to a considerable distance; thenceforth, it would seem, each kind of matter obeyed its own special law of retreat from the nucleus. We should, therefore, still have a process of sifting, complicated, so to speak, by the condition in which the repulsed matter left the head of the comet in the first instance.

But perhaps the comet which of all others seems to afford the most striking evidence of the justice of Herschel's theory is the remarkable comet of 1744. According to Chéseaux this comet had no less than six tails spread in the manner of a fan. Now, in a case of this sort, we must not forget to take special notice of the fact that a comet is not a flat object painted, so to speak, upon the surface of the celestial vault, but an object occupying a certain region of space. We are forbidden, therefore, to regard the six seeming tails of the comet of 1744 as being in reality six distinct tails, unless we are prepared with some explanation of their symmetrical adjustment. So far as I am aware this circumstance has not hitherto been noticed adequately, or at all, in our treatises on astronomy. When we see a straight-tailed comet, like that of 1811, showing two well-marked and nearly parallel striations, which seem to extend from either side of the head, and enclose between them a space of comparative darkness, we are not led to regard these bounding streaks as two distinct tails. We accept, on the contrary, the explanation suggested by the aspect of the comet, and regard the tail as shaped like a hollow cone. This accords well, be it noted in passing, with Herschel's theory; for the envelope round the nucleus, if swept away by the sun's repulsive energy, would form a conical shell of matter behind the head, much as a vertical jet of water, caused to spread during its upward motion, descends in a hollow conical[1] shell of spray beneath the level of the jet. But while we thus interpret the appearance of a straight-tailed comet, we are apt to apply a different, and, in reality, inadmissible mode of interpretation to comets whose structure seems more complex. Now, if we extend to the six-tailed comet

[1] I have purposely avoided here the proper technical words for describing the shape of the spray-fall. The actual shape of any portion of the shell beneath a certain level is fairly described as conical—that is, this portion of the shell corresponds in shape to a portion of a cone's surface.

of 1744 the same principle of interpretation that we apply to the straight-tailed comet of 1811, we shall be led to regard the former as not in reality *six*-tailed, but *three*-tailed. Three conical shells of luminous matter, one inside the other, and separated from each other by dark spaces, would present an appearance resembling that of the multiple tail of the comet of 1744. Nor would the curvature actually seen in the tails of that comet render this interpretation less satisfactory, since this peculiarity corresponds precisely with what is observed in less complex cometic appendages. Now, in order to account for the existence of three tails, one inside the other, we need only conceive that the comet of 1744 had three envelopes, like those seen round the nucleus of Donati's comet, and that precisely as the matter of a single envelope swept away by solar repulsion produces a single tail, so the matter of these three envelopes similarly swept away produced three tails, the inner enveloped by the two outer. It is not absolutely necessary, however, to assume that the three tails thus formed successive shells; for each envelope of the head may have had its own distinct tail thrown off in its own distinct direction. Indeed, the aspect of the three tails of Donati's comet would seem to render this view the more probable, for the two fainter tails came from one side of the head, as though they severally formed but the halves of complete shell-formed tails, the other halves being, perhaps, hidden from our view by the primary tail.

It must not be forgotten that the theory which I have here employed as the basis of these several ideas was one which Sir John Herschel regarded as demonstrated by the evidence he obtained while observing Halley's comet in 1836. When Sir John Herschel spoke of a theory as demonstrated, one might fairly conclude that overwhelming evidence had been obtained in its favour—for few surpassed him in scientific caution. Now the terms in which he spoke on this subject are undoubtedly most positive—far more so, I believe, than in any other passage which can be quoted from his works. I refer here specially to the words used at p. 406 of Herschel's great work, 'The Results of Astronomical Observations made at the Cape of Good Hope.' But his account of the comet, and of later comets, in his charming series of 'Familiar Essays,' leaves no doubt on the reader's mind that the great astronomer, after more than twenty years' further study of the subject, still retained his conviction. 'The whole series of the phenomena presented by this comet has given us,' he says, 'more insight into the *interior economy of a comet,* and the forces developed in it by the sun's action, than anything before or since.' And further on he remarks that clearly the tail of a comet is neither more nor less than the accumulation of a sort of luminous vapour, *darted off in the first instance towards the sun,* as if it were something raised up, and as it were exploded by the sun's heat, out of the kernel, and then immediately and forcibly turned back and repelled *from* the sun.

Nor does this account of the formation of a comet's tail seem otherwise than perfectly reconcilable with the observed association between meteors and comets. Indeed, it is well worthy of notice that in the great work already referred to, Sir John Herschel does, in the most distinct way, anticipate this remarkable discovery, besides supplying a partial interpretation of the association. 'Supposing the approach of a comet to the sun,' he says, 'to be such as to enable the repulsive force to overcome the attractive in those portions of its tail remote from the nucleus, they would, of course, be driven off irrecoverably. The separation of a portion of the tail, here contemplated, could hardly be accomplished without carrying off some portion of the gravitating matter.'

It happens singularly enough[1] that one of the two comets which have alone as yet been fairly associated with meteoric systems was observed by Sir John Herschel—'with septuagenarian eyes,' he mentions—and that his remarks respecting its appearance bear in an interesting manner on the subject of the connection between comets and meteors. I refer to the great comet of 1862, which has been shown by Schiaparelli to travel in the same path, or very nearly so, as the August meteors. With Sir John Herschel's account of this comet I shall conclude this paper, already drawn out to a greater length than I had proposed. It will be noticed that the observed appearances serve to connect several of the facts already referred to. After

[1] One of the many strange coincidences in the history of meteoric and cometic astronomy of late years.

noting the circumstances under which this comet came into view, Herschel remarks that 'it passed us closely and swiftly, swelling into importance, and dying away with unusual rapidity. The phenomena exhibited by its nucleus and head were on this account peculiarly interesting and instructive, it being only on very rare occasions that a comet can be closely inspected at the very crisis of its fate, so that we can witness the actual effect of the sun's rays on it. In this instance, the pouring forth of the cometic matter from the singularly bright and highly condensed nucleus, took place in a single compact stream, which, after attaining a short distance, equal to rather less than a diameter of the nucleus itself, was so suddenly broken up and dispersed as to give, on the first inspection, the impression of a double nucleus. The direction of this jet varied considerably from day to day, but always declined more or less from the exact direction from the sun.' It seems far from improbable that what was here witnessed represented the actual generation of new August meteors, and that at some more or less distant epoch portions of the matter thus swept away from the comet of 1862 may take their part in producing a display of falling stars.

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THE SUN'S CORONA.

In a paper which appeared in 'Fraser's Magazine' for February 1870,[1] I called attention to certain results which seemed fairly deducible from the observations made by American astronomers and physicists during the eclipse of August 7, 1869. The news of those observations reached me while I was engaged on that paper (entitled 'Strange Discoveries respecting the Aurora'), and seemed to add a new importance to the discoveries which I had already recorded. The aurora had been analysed with the spectroscope, and the results were full of interest. The zodiacal light had been similarly analysed, with results indicating an association between this phenomenon and the terrestrial aurora; and this circumstance seemed even more interesting than the facts revealed respecting the aurora itself. But scarcely had these results been recorded when there came the news that the solar corona had also been analysed with the spectroscope during the eclipse of 1869, and that its spectrum presented the same bright lines which appear in that of the terrestrial aurora! Three phenomena severally interesting, as well as severally perplexing, were thus

[1] See 'Light Science for Leisure Hours.'

brought into seeming association; and though the nature of any one of them was by no means definitely revealed, yet considerations of the most significant nature were suggested—considerations at once enhancing the interest of these several phenomena and promising to afford one day a means by which all three might be interpreted.

Let us examine what is the present state of our knowledge respecting the sun's corona, noting specially what new light, if any, has been thrown upon the problem by the recent eclipse expeditions, but also not forgetting that vast mass of evidence which former observers have accumulated for our use. It may be noted, indeed, that if we are in a position to theorise at all respecting the corona's nature, we shall certainly not theorise safely unless we consider *all* the evidence we have. To take this or that fact, however striking, and on it to found a theory respecting a phenomenon so remarkable, and presenting so many complex relations, would be unwise indeed. We must endeavour to bear in mind all that has been learned, to apportion to each observed fact its due weight, and where observed facts seem opposed to each other to analyse them with special care, since nearly always the most definite and striking evidence is afforded by those observations which seem most perplexing.

Let us first examine what is known about the sun and his surroundings, in order that we may the more satisfactorily weigh the evidence respecting phenomena as yet unexplained. Such a course is also rendered advisable by the fact that there will be frequent occasion to refer to the prominences and other like features in speaking of the corona and the problems it presents to us for solution.

The rainbow-tinted streak which forms the basis (so to speak) of the solar spectrum tells us that the sun's light comes in the first place from matter which is incandescent, and is either solid or liquid; or, if gaseous, exists at a very great pressure. The innumerable dark lines which cross the rainbow-tinted streak show that outside this matter there are the vapours of many well-known terrestrial elements, existing at a lower temperature than the matter which gives the continuous background of the spectrum. Of the exact position of these absorbing vapours we know (or, perhaps, I should say we *knew*, before the recent eclipse) comparatively little; but they must necessarily lie above the regions whence the really white light proceeds.

Outside these absorbing vapours is that region into which the coloured prominences are projected. But far lower than the summits of the prominences there lies the region to which the Astronomer-Royal gave (in 1842) the expressive name of *the sierra*. It appears in solar eclipses as an arc of red light around the sun. Its border is well defined and serrated. In colour it resembles closely the prominences; and the researches of spectroscopists have shown that it consists in the main of the same gases.

Then, lastly, outside the prominences and the sierra there had been recognised the corona, a glory of light surrounding the sun during total eclipses. Precisely as the coloured matter is divisible into lofty prominences and the low sierra, so this corona had been seen to consist of two distinct portions, viz., projecting radiations extending sometimes to a distance from the sun far exceeding his apparent diameter, and a lower, brighter, and more uniform portion extending to a distance of little more than a fifth of the sun's apparent diameter. Since the recognition of this peculiarity has been described by those little familiar with the history of solar eclipses as the most important result of the recent eclipse expeditions, it may be as well to remark in this place that the fact has been known for at least 164 years. For in 1706 MM. Plantade and Capiés recognised the existence of a ring of very white light around the moon, within the limits of which ring 'the light was everywhere equally vivid; but beyond the exterior contour the light was less intense, and was seen to fade off gradually into the surrounding darkness, forming an annulus round the moon of about eight degrees in diameter.' I quote from 'Grant's Physical Astronomy,' to which excellent treatise I would refer the curious reader for many other accounts respecting the ring-formed portion of the corona.

It is this seemingly compound object—the solar corona—that astronomers have been so anxiously seeking to interpret during the last two or three years. The recent acquisition of new powers of research, as well as the new knowledge lately obtained respecting the constitution of the solar system, at once suggested hopes that this problem might be at length mastered, and encouraged the expectation that the results would throw a most important light on the economy of those regions of space which immediately surround the solar orb.

It may be said that the first attempt to apply the new means of research to the phenomena presented by the corona was made during the eclipse of 1860, when Dr. De la Rue and Fr. Secchi photographed the eclipsed sun. The success of these physicists was not great, however, as respects the corona. They succeeded in obtaining excellent photographs of the coloured prominences; but only faint indications of the corona are shown even in the best of their pictures. The photograph which showed the widest extension of the corona was one of Fr. Secchi's; and he was enabled to draw from this view the conclusion that the corona is somewhat brighter and more developed over the solar spot-zone than near either the equator or poles of the sun.

Eight years passed, and then the approach of the great Indian eclipse, one of the most remarkable which have ever occurred, led astronomers to hope that the powers of the spectroscope might reveal something of the true nature of the coronal glory. Indeed more was hoped from the study of the corona than from that of the prominences. This is evident from the words in which Capt. Herschel describes the moments preceding totality. Addressing Dr. Huggins, he says, 'You may conceive 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 result by no means corresponded with the expectations of astronomers. The prominences were successfully analysed by spectroscopists, whereas the corona on this occasion baffled their exertions. Major Tennant alone succeeded in obtaining any definite result. He found that the light of the corona gave a faint continuous spectrum, without either bright or dark lines; but he was not so satisfied on this point as to feel able to draw the conclusion which would inevitably flow from such an observation if satisfactorily made—the conclusion, namely, that the corona consists of solid or liquid particles, incandescent with intensity of heat.

Let us briefly consider in what respects Major Tennant's observations were unsatisfactory to him.

The sun's light when analysed prismatically is resolved into a rainbow-tinted streak crossed by dark lines. If the corona were a solar atmosphere shining by reflecting solar light, its spectrum should resemble the solar spectrum, only of course the coronal spectrum would be very much fainter. When we turn a spectroscope towards the sky, we see always the solar spectrum with its dark lines, if the spectroscope be but properly adjusted. I take up from beside me, as I write, one of Browning's miniature spectroscopes, and direct it towards a white cloud. I see a rainbow-tinted streak, but I can distinguish no dark lines. I have only to turn the collar, however, which adjusts the slit, in such sort as to make the slit narrow enough, and then at once there start into view the principal dark lines, or Fraunhofer lines, as they are called. The fact then that Major Tennant had seen no dark lines might not necessarily prove that the coronal spectrum is aught but a faint solar spectrum such as we get from the light which our own atmosphere reflects to us. Turning to his account we learn those particulars which are to guide us in forming an opinion. 'Thinking,' he writes, 'that want of light prevented me from 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. *What I saw,*' he proceeds, '*was undoubtedly a continuous spectrum, and I saw no lines.*' (The italics are his.) '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.'

During the year which elapsed before further attempts were made to solve the problem, certain results were achieved which seemed to bear indirectly on the subject of the corona. The bright lines belonging to the spectrum of the prominences were found to be visible when the sun is not eclipsed, if only spectroscopes of adequate dispersive power (able, therefore, to sufficiently reduce the light of our own atmosphere, which blots out the prominences themselves) were made use of. Now amongst these bright lines are those of the gas hydrogen, and physicists knew something of the laws according to which the lines of this gas vary, with varying circumstances of pressure, temperature, and so on. Applying these principles, they were able to conclude that in all probability the atmospheric pressure close by the sun's surface is not nearly so great as we should expect it to be if the corona is a solar atmosphere. Wullner, a most experienced spectroscopist, was able to deduce from his own observations this interesting result, that close by the visible limits of the solar disc, the atmospheric pressure lies between that corresponding to two inches of the mercurial barometer at the earth's surface, and that corresponding to twenty inches; or, in other words, falls considerably short (even at the highest assignable value) of the pressure of our own atmosphere near the sea-level. For this last-named pressure corresponds, on the average, to about thirty inches of the mercurial barometer.

Now, without admitting this conclusion as in reality affording sufficient evidence respecting the atmospheric pressure at the sun's surface, it yet shows that at a depth of many thousands of miles below the apparent boundary, even of the inner corona, the pressure is by no means such as we should anticipate on the theory that the corona is a solar atmosphere. Remembering that our own atmosphere is commonly supposed to be less than 200 miles high, and that it is subject to the relatively puny forces of terrestrial gravitation alone, it will be recognised how enormous the pressure of a solar atmosphere would be, if that atmosphere were similarly constituted and reached many thousands of miles from the solar surface, while the attractive force to which it would be subjected would be the mighty energy of solar gravitation.

There was grave reason, then, for doubting whether that theory of the corona was true, according to which it was regarded (even as respects those parts which present a radiated structure) as a solar atmosphere.

Let us pass on, however, to the evidence which American astronomers obtained during the eclipse of August 7, 1869.

It had been reported to them (by mistake) that Major Tennant had found the coronal spectrum to be simply a faint solar spectrum. One and all expressed their surprise that they could find no dark lines on the faint continuous spectrum presented by the corona; but they detected *bright lines.* Professor Harkness found one bright line. Professor Young recognised the same line and suspected the existence of two others. Professor Pickering saw three bright lines.

Here was a result which seemed to exhibit the corona as formed, in part at any rate, of luminous vapour.

The important discovery made by the American astronomers was questioned by many. Some held that by mistake prominence-matter had been examined, others that the observers had been deceived as to the real existence of these lines. And an attempt was even made to explain away these lines, regarded as really seen in the spectrum of light from the direction in which the corona appeared. In some respects the attempt was not wanting in ingenuity.

It was reasoned that a spectrum apparently continuous may be made up in reality by the combination of two spectra—a spectrum crossed by dark lines and a spectrum consisting of bright lines only. Thus, if a certain small proportion of sun-light—such, for instance, as we get from the sky on a dull day—were combined with a large proportion of light from the coloured prominences and the sierra (the bright lines in whose spectrum correspond with the principal solar dark lines), the solar spectrum from the former would have its principal dark lines filled up by the prominence-spectrum from the latter. This, it was suggested, is in all probability the case during total eclipse: the solar light received by the atmosphere at that time is greatly diminished, it was argued; the light from the prominences is not diminished at all; hence results the effective obliteration of the dark lines in the solar spectrum, while the brighter lines belonging to the prominence-spectrum become visible as such, and so cause the appearance of those three bright lines which the American astronomers had mistakenly assigned to the corona.

Only one thing was needed to render this ingenious theory acceptable. The theory required that a certain quantity of solar light should fall on that part of the atmosphere which lies towards the moon's place during totality, and it was unfortunately demonstrable that nothing of the sort can happen. It is not necessary now, however, to insist on the arguments by which the theory was opposed as soon as urged, because the evidence obtained during recent eclipses has sufficed to dispose altogether of the hypothesis. Indeed, a year or so after it was propounded a view far less open to attack was substituted. It was suggested that the sun's light reached our atmosphere after being reflected by a solar atmosphere. This solar atmosphere was regarded as the true corona, and the coronal radiations as due to the illumination of our own atmosphere by the light from the inner corona. Against this theory there are none of those obvious objections which oppose themselves to the others. That our atmosphere is illuminated by light from the inner and brighter parts of the corona is evident to the senses; for during total eclipse we *see* this inner part of the corona—that is, its light reaches us—and if *us*, then, of course, *the air around and above us*. Nor is it at all unlikely that precisely as we often see a halo round the uneclipsed sun, so the uneclipsed inner corona may produce a similar phenomenon.

Nor, again, did the same reasons present themselves for oppugning this theory which had appeared when the former was propounded. For clearly the enunciation of the theory that the light of the corona is simply due to the passage of the solar rays through our own atmosphere might be expected to lead the observers of solar eclipses to attach but small importance to the corona. If the corona were of this nature —a mere optical phenomenon—it would be no better worthy of study than those solar beams which pass from between openings in clouds. But no such harm could accrue from the later theory, even though that theory were false; because it could only lead to the careful analysis of the corona's structure, and the results of such analysis could scarcely fail to prove highly instructive.

But while recognising—what had indeed been demonstrated many years before—a real difference between the inner and outer parts of the corona, and while also recognising the fact that no inconsiderable portion of the light received from the sky during total eclipse *must* have undergone both reflection and refraction in our own atmosphere, the theory that the coronal radiations are merely terrestrial phenomena seemed to me (as I believe to all familiar with the history of former eclipses) altogether untenable. A single radiation, or several seen during past eclipses, might be explained in this way. But the accounts given of some extensions of the coronal light were such as could be by no means explained away as merely phenomena of our own atmosphere. Negative evidence, it is to be remembered, could prove little or nothing. That during any given eclipse, or that at any given station, no radiations appeared, would prove indeed the extreme delicacy of the light received from the coronal rays, but not that there are no such rays. Again, the apparent motion or disappearance of a ray, even if accepted as proving that some atmospheric phenomenon was in question, would by no means prove that all coronal rays have the same atmospheric origin. We may, in fact, accept both these circumstances as proved. Unquestionably the coronal radiations are phenomena of exceeding delicacy, and almost unquestionably *atmospheric* radiations are sometimes visible; but the real existence of coronal radiations is not therefore disproved, or even rendered improbable.

On the other hand, positive evidence, even if small in quantity, must needs be absolutely demonstrative. For let us see what is required if the radiations are really solar appendages. A beam in our own atmosphere would of course move swiftly during the progress of the moon across the solar disc; but matter in the upper regions of the air, if illuminated during the whole duration of totality, might present the appearance of fixity, and so simulate the nature of a coronal radiation. To prove that a radiation was not of this nature it would be necessary that it should be seen at stations several miles apart. This then is the first requisite: in order to show that any radial projection of the corona belongs to a real solar appendage, it should be seen unmistakably from stations widely separated. But this is not all. If a beam or radiation were caused by the solar light falling on some matter close by the moon's places [1]—after the manner of the beam from an electric lamp falling on dusty air—then this beam would shift rapidly, as the moon shifted. Hence, this is the second requisite: a radial projection, if a real solar appendage, must remain unchanged in position as seen from one and the same station.

If both of these conditions are satisfied in any single instance, if the same radiation is seen from stations wide apart and remains unchanged in position during the whole continuance of totality, there can be no further question that the corona has an extension corresponding to at least the visible limits of the radiation. And a single demonstrated instance of this sort removes all reasons for doubt as to those other instances where it had only seemed strongly probable that the coronal radiations were solar appendages.

Now the first points to be noticed in the accounts of the eclipse of December 1870, although full of interest and importance, yet do not bear on this particular consideration.

In the first place we received from many quar-

[1] I refer to this view because Oudemann has lately urged the theory that certain phenomena of the corona may be thus explained. For reasons elsewhere stated, however, I regard Oudemann's theory as wholly untenable. To mention no other objection, if matter extends from the sun beyond the earth's orbit, as Oudemann supposes (which indeed I do not question), and if this matter is of such a nature that the part between the moon and the earth can send us an appreciable quantity of light, as Oudemann's theory requires, then the part lying beyond and in the same visual direction, right up to the sun's neighbourhood, would send at least 1,000 times as much light, which coming from the same quarter would wholly prevent us from recognising the former portion.

ters abundant evidence that the observations made by the American astronomers in 1869 were trustworthy. The double character of the coronal spectrum was proved in a manner admitting of no question. The continuous spectrum without dark lines was seen at four different stations, while at as many the bright lines which had been seen in 1869, and other bright lines not then discernible, were clearly recognised. It is not my purpose, nor indeed does space permit me, to give an account of the several observations made in Spain and Sicily. The following extract from a valuable paper by Mr. Langley shows what the American physicist, Professor Winlock, noted near Cadiz, and the account may be regarded as typical of the general results deduced by the observers: 'Using a spectroscope of two prisms on a five and a half inch achromatic (directed by Mr. A. Clark at the finder), Professor Winlock found a faint continuous spectrum without dark lines. Of the bright lines the most conspicuous was "1474 Kirchhoff"' (a line belonging to the spectrum of the aurora), 'which was followed round the sun to at least twenty minutes from the disc. It may be here remarked,' adds Mr. Langley, 'that all the spectroscopes showed this as much the most conspicuous coronal line. A number of other lines were also noted, and their position recorded.' [1]

[1] An observation was made by Professor Young which is highly interesting in itself, and personally interesting to me as agreeing perfectly with anticipations I had myself expressed. Placing the slit of his spectroscope so as to include a linear space forming a tangent to the sun's disc, he found that 'at the moment of obscuration, and for one or

It was demonstrated, then, that the coronal light is, at least in part, distinct from that of the prominences or sierra, since in their light the line '1474 Kirchhoff' is by no means the most prominent, nor even at all times visible. Confirmation was also given to that startling theory which I urged in 'Fraser's Magazine' so far back as February 1870 [1] (and which had been earlier urged by others), *that the corona is of the nature of a solar aurora.*

But if we consider the evidence, we find that it does

two seconds later, the field of the instrument was *filled with bright lines.* As far as could be judged, during this brief interval, every non-atmospheric line of the visible spectrum showed bright, an interesting observation confirmed by Mr. Pye.' 'From the concurrence of these quite independent observations,' says Mr. Langley, 'we seem to be justified in assuming the probable existence of an envelope surrounding the photosphere, and beneath the chromosphere usually so called, whose thickness must be limited to two or three seconds of arc, and which gives a discontinuous spectrum consisting of all or nearly all the ordinary lines, showing them, that is to say, *bright* on a dark field.' In a note at p. 295 of the first edition of my treatise on the sun (proof sheets to p. 384 were in the hands of some of the observers who went to Spain) I wrote, '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, one hundred miles above the photosphere, and yet no instruments we possess could suffice' (I refer to observations made on the uneclipsed sun) 'to reveal any trace of its existence. . . . 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.' It had in fact always seemed to me that those who urged such arguments forgot how minute an angle one hundred miles at the sun's distance subtends, and that in fact their instrumental means could not avail to render a layer of such a depth even sensible, far less to analyse its structure.

[1] See the Essay on the Aurora Borealis, in my 'Light Science for Leisure Hours.'

not throw any satisfactory light on the chief question at issue—the question, namely, of the corona's extension. Illuminated as our own air must needs have been by that intensely bright part of the corona which lies close to the sun, the coronal spectrum might well be given by the light from our atmosphere. There was, indeed, a way of determining whether this was so or not. Of course, the portion of the atmosphere lying directly towards the moon would be illuminated by the corona as fully as the portion outside; unless, indeed, there were haze in the air, in which case the figure of the coronal ring would be in some sort represented, though with considerable expansion, in the resulting halo. If, then, the spectroscope were directed to the moon's seemingly dark disc, the bright lines of the corona ought to be visible about as clearly as when the spectroscope was directed outside the true limits (whatever they may be) of the corona. Captain Maclear found that the bright lines of the corona were visible in the light received from the direction in which the moon's centre lay; but the lines were not half so bright as those seen when the spectroscope was directed to a distance from the moon's edge (outside of it) equal to about one-fourth of the moon's apparent diameter. This would imply that in the latter case the spectroscope was still directed to a spot within the real limits of the corona, or rather of that portion of the corona which is partly gaseous.

It will be seen, however, that considerable doubt rests on the spectroscopic observations, so far as they bear on the question of the real extension of the corona. In this respect, indeed, they can scarcely give either negative or positive evidence which can be trusted. For even if the bright line spectrum were not given by light beyond a certain distance from the sun, it would by no means follow that the coronal light in that direction did not come from a real solar appendage. The gaseity of the corona might be limited to certain distances from the sun, although the corona itself extended very much farther. Nor, again, can the positive evidence supplied by the visibility of the bright lines at considerable distances from the sun be trusted implicitly, since, as we have seen, our atmosphere may reflect the light which supplies those bright lines.

Thus the whole question of the corona's extension depended on the success of those who sought for evidence of the fixity of coronal radiations seen at any given station, and of the identity of radiations *so* seen from different stations.

So far as ordinary methods of observation were concerned, there was little reason for hoping that this particular eclipse would give better results than former ones. If any eclipse could have settled the question, one would have supposed the American eclipse of 1869 would have done so. For then the corona was seen from a number of stations along a track crossing the whole breadth of North America; favourable weather was nearly everywhere experienced; skilful observers were prepared to note the appearance of the coronal radiations; and finally, it was hoped that photographers might succeed in obtaining good pictures of the corona. But inasmuch as the photographs actually obtained only showed the brightest part of the corona, all depended on direct observation; and in this, as in many former instances, discrepancies appeared in the various accounts, while the sketches differed also considerably *inter se.* Observers agreed in describing the corona as four-cornered in figure; but as to its colour, its extension, and the exact position of the radiations, they were not by any means in satisfactory agreement. The question remained in abeyance; and many were disposed to believe that the eclipse of December 1870 would leave this particular problem still unsolved.

Now as respects direct observations last December, though there was much that seemed to indicate that *certain* radiations were seen from different stations, and that *these* radiations remained unchanged in position during totality, there was still an element of uncertainty. Mr. Langley, in the account from which I have already quoted, fairly sums up the results of observation: 'In some well-marked features all agree, in other minor ones such differences exist that one might almost say each saw a different corona.'

But at this time the photographs taken in Spain and Sicily had not been compared with each other, or with the drawings of different observers. Already, indeed, peculiarities had been recognised in the drawings taken in Spain, which promised to give decisive evidence on the point at issue. At three stations, forming a triangle with sides five or six miles long, near Cadiz, a well-marked V-shaped gap with clearly defined bounding radiations, opposite the moon's south-eastern quadrant, had been noticed as the most prominent feature of the corona. It had remained unchanged in position during the whole continuance of totality, although the play of light and shade over the eclipsed sun had been considerable. It was pictured in a large drawing exhibited by Lieut. Brown at the January (1871) meeting of the Royal Astronomical Society. At the same meeting Mr. Hudson, a fellow of St. John's College, who had seen it from another station, remarked that in the picture, marked as this feature was, it was not so marked as it had appeared to himself; and Lieut. Brown admitted that his picture did not present this striking feature to his own satisfaction.

If only all the evidence here stated could be admitted as certain, the question of the existence of real solar radiations to a distance nearly equal to the moon's apparent diameter was demonstrated. But doubts were still expressed whether the accounts and drawings disposed finally of the question.

At the same meeting photographs by Lord Lindsay were handed round, and these seemed scarcely to confirm the view that this great V-shaped gap really existed in some vast solar appendage. In these photographs no very considerable extension of the corona could be traced, and it seemed open to question whether, in taking so many as nine, Lord Lindsay had not unduly shortened the exposure for each.[1] Again, there were obvious signs in the best of the photographs that at Lord Lindsay's station a haze or some other atmospheric cause had tended to mar the distinctness of the corona; for the disc of the moon, especially on the side where the corona was brightest, was illuminated with a light far too strong to be otherwise explained (assuming always that all the photographic operations had been satisfactorily performed).

But after this meeting attention was directed to a photograph taken by the American observers at a station close by. In this photograph only a portion of the corona was shown;[2] but the extension of the corona was considerably greater than in any photographs which had hitherto been taken; and *there*, in the south-eastern quadrant, was that very V-shaped gap of which the observers had spoken, and which Lieut. Brown and others had depicted. It was not a mere faintly-seen or perhaps half-suspected feature, but *the most striking feature in the photograph.*

One thing only was required to remove all shadow of doubt. News had reached England that Mr. Brothers had been most successful in photographing the corona at his station near Syracuse. In the fifth

[1] I wrote this under correction; the complete series of photographs not being available for examination at the time. Certainly, though complete success may not have rewarded Lord Lindsay's exertions, there can be no question of the degree of credit due to him. At one time it seemed probable that his expedition, set forth at his own charge, would be the only one to uphold the scientific credit of our country.

[2] I do not mean that the outer part had failed to appear on the glass, but that the glass only included the inner half.

plate of six he had taken, 'the corona is shown,' said the account, 'as it was never seen on glass before.'

Here a crucial test seemed available. If the great gap opposite the south-eastern quadrant was not seen in this photograph, negative evidence, about as strong as negative evidence could be in this case, would be supplied against the theory that the radiations are true solar appendages. On the other hand, if the great gap appeared in the photograph, then positive evidence of the most convincing kind would be afforded on this interesting question.

I was so fortunate as to be the first to receive intelligence on this point. Mr. Brothers forwarded, through me, to Dr. Huggins, a rough drawing of his best photograph, and in that picture the V-shaped gap appears *as the most striking feature of the corona.* It is more plainly shown than in the American photograph, and its borders can be traced very much farther from the sun. The photograph, indeed, fairly bears out the statement that the corona is shown as it was never seen on glass before; it is *facile princeps* among photographs of the corona: but, except in this greater clearness and extension, the figure of the great gap and of the bounding radiations agrees perfectly with the American photograph.

At length, then, we have evidence which cannot be questioned on this long-mooted point. The corona itself has left us an unmistakable record, has written down in the plainest possible characters a statement of its true nature. By a piece of good fortune such as few were so sanguine as to anticipate, a feature strongly marked enough to be recognisable beyond the possibility of question has been depicted in two exceptionally successful photographs, taken at widely separated stations. This one feature proves all that we require. Granted that two radiations (for the gap implies necessarily the existence of two bounding rays) exist in some real solar appendage, it will no longer be doubted that radiations of the same nature exist all round the sun. Nor will it now be questioned that the faint prolongations of such radial beams, seen when eclipses are viewed under very favourable circumstances, belong also to this solar appendage. Those expansions of the four-cornered corona in 1869, which General Myer, stationed 5,000 feet above the sea-level, was able to trace to a distance of 'two or three diameters of the moon's disc,' must now be regarded as indubitably appertaining to some solar appendage. For the faint shadow of doubt which hung over the concurrent accounts of the figure of the corona during the American eclipse has been fairly dissipated by the testimony now obtained; and once admitting the coronal projections seen at lower stations as belonging to a solar appendage, the extensions of those projections seen by observers above the denser atmospheric strata must of course equally be associated with that appendage. The fixity of those four far-reaching extensions during the four minutes of totality, as also the fixity of the far-reaching extensions seen during the Swedish eclipse of 1736, not only during totality, but for several seconds afterwards, can now be understood. Astronomers have not had to deal, in these and other instances, with beams shining through our own atmosphere, but with illuminated regions of space exceeding the sun's own orb many times in volume.

As to the physical meaning of the coronal phenomena, I refrain at present from speaking. The subject is one of wide extent, and could not fitly be treated at the close of such a paper as the present. The interpretation of the coronal radiations is connected, I believe, with the subject of meteoric astronomy already dealt with in these pages, with the phenomena of our own auroras, with the zodiacal light, with cometary systems, and finally with those strange laws according to which magnetic and auroral phenomena are associated with the disturbance of the solar photosphere. The task of duly presenting these interwoven relations must be left to another occasion.

Fraser's Magazine, March 1871.

WHAT, THEN, IS *THE CORONA?*

It is not easy for a thoughtful mind to study the evidence bearing on any scientific subject without being led to theorise. Even though the evidence be imperfect, even though—however carefully sifted and analysed—it still leave the problem indeterminate, the mind will yet weigh fact against fact, and probability against probability, adopting then, though but provisionally, the theory which seems best to accord with such facts as have been revealed. As fresh facts are ascertained, the theory may have to be modified or even abandoned; and often one theory after another may thus be adopted for a while and presently rejected; yet it is only by thus theorising—boldly, but with due deference to facts—that the truth can finally be established. There is no recorded instance—so far as I know—of any difficult problem in science which has been mastered otherwise than by resolute and industrious theorising based on the careful study of all the observed facts bearing upon the subject matter. So Copernicus was enabled to place the sun at the centre of the planetary scheme; so Kepler assigned to the planets the laws according to which they move; so Newton was able to discover the mainspring of the universe. No otherwise, again, did Römer learn how to measure the velocity of light, or Bradley find a meaning in the aberration-ovals traced out by all the stars upon the heavens. These men, and a hundred others whose names stand highest in the records of science, were theorists; some of them *mere* theorists; and Newton, the greatest of them all, was (so far as astronomy is concerned) so completely the theorist that he never made a single astronomical observation of any real importance.[1]

Therefore I do not think that the fear of being called theorisers by the unthinking should deter us from an attempt to found upon the evidence already obtained respecting the corona such conclusions as that evidence may seem fairly to support. So far as I am myself concerned I am the readier to do this, because I think I shall have to modify very importantly certain opinions in which I had but lately some confidence. I do not indeed find that any theories I had urged as in effect demonstrated are otherwise than strengthened by the evidence lately obtained; but some opinions which had appeared probable to me some time since seem open now to grave objection.

[1] There is a sentence in the introductory pages of the Astronomer Royal's admirable 'Lectures on Astronomy' which reads strangely in connection with the known facts of Newton's life: I mean that sentence in which he divides those who merely take interest in the science of astronomy from 'persons who are officially attached to observatories, or in other ways professionally cognisant of the technicalities of practical astronomy.' How shall Newton, thus judged, retain his place as an astronomer, or rather the greatest of astronomers? Where are the transits he took? the star-catalogues he formed? the physical features he detected in sun, or moon, or planet? the double stars he divided or measured? In all that some in our day call astronomical *work* he did absolutely nothing. Where others worked he only *thought*; and thus all that he could do was—*to create modern astronomy.*

Much of the evidence on the corona is presented in the preceding paper on that subject; but some facts which only reached me after that paper was written, require to be briefly noticed. For a full account of the scientific details, together with pictures of the corona as photographed, &c., I would refer the student to the second edition of my 'Treatise on the Sun.'

The reader of the preceding paper on the corona will gather that I look on the evidence recently obtained, which proves the coronal radiations to belong to a real solar appendage, as in effect but 'a demonstration of the demonstrated.' No one who had studied the immense mass of evidence acquired during the last two centuries on this point could feel any doubt as to the real existence of these radiations in some amazing solar appendage.

I was prepared therefore to learn that the corona as seen and photographed in Sicily corresponded in all essential respects with the corona as seen and photographed in Spain. This correspondence exists beyond all possibility of question; but when the best records are studied, and when the photographs are carefully examined, something more is revealed which, whatever its interpretation, is undoubtedly full of meaning. *Where any great gap or rift appears in the outer or radiated part of the corona, there a depression is seen in the inner and much brighter portion; and yet again, where this inner portion is thus depressed, there the coloured prominences are wanting, and the sierra is shallow.* As to the former point I shall merely remark that the peculiarity is very markedly shown in Lieut. Brown's drawing of the corona as seen in Spain; that he referred to it as a fact he had specially noticed; and that both in the Spanish and Sicilian photographs it is most strikingly manifested. As to the latter, I shall quote Professor Roscoe's words respecting Mr. Seabroke's maps of the prominences and Professor Watson's drawing of the corona: 'On comparing the two drawings thus independently made, a most interesting series of coincidences presented themselves. Wherever on the solar disc a large group of prominences was seen in Mr. Seabroke's map, there a corresponding bulging out of the corona was chronicled on Professor Watson's drawing; and at the positions where no prominences presented themselves, there the bright portions of the corona extended to the smallest distances from the sun's limb.'[1] But I must add one piece of evidence directly associating the most distant portion of the corona with the region richest in solar prominences. Mr. Brothers's photographs *all* show the corona extending much farther towards the west than towards the east. 'There can be no question,' he writes, 'that there was more coronal light on the west side of the moon than at the other points;' and then he calls attention to the fact 'that the prominences are more numerous on the side where the corona is brightest.'

[1] It is perhaps necessary to point out that Mr. Seabroke's drawing was not made in the hurry of the eclipse, but (by the spectroscopic method) before the eclipse began.

Now here is a fact of the utmost significance—so significant, indeed, that it will be well to inquire whether it is in any way supported by the evidence obtained during former total eclipses of the sun.

Fortunately, it is not difficult to find corroborative evidence of the most satisfactory kind. We have only to turn to the account of the corona as seen during the American eclipse of 1869, and to compare the drawings with the photographs, to see that then also this feature was presented. The peculiar trapezoidal figure of the corona as seen on that occasion is most clearly indicated in the much smaller corona shown in the best photographs then taken. And indeed, one piece of evidence then obtained goes somewhat beyond any that can be deduced from the observations made last December. For at a station where the observers were raised more than 5,500 feet above the sea-level, the quadrangular figure of the corona was seen to be extended into four radial streamers, reaching to a distance equal to three times the moon's apparent diameter.[1] It

[1] I feel compelled to set aside the evidence of Dr. Gould. He saw moving streamers not agreeing with the inner quadrangular radiance: but he alone gives such an account; and surely it would be absurd to reject the numerous accounts pointing to identity and fixedness on the score of one easily explained account of a different kind. Nothing is more natural than that at some station or other atmospheric effects should be mistaken for the real coronal radiations. But to reject on this account the narratives of witnesses describing close resemblance is surely unwarranted. It is as though, after twenty witnesses had stated that a person dressed in a particular way had passed along a certain road, their evidence should be regarded as not relating to one and the same person because one witness had seen a differently-dressed person traverse the same road.

may be added that this four-cornered aspect of the corona has been very commonly noticed in former eclipses, and the greatest extensions have always been opposite the points midway between the solar poles and equator; in other words, opposite the solar spot-zones, where also Professor Respighi has demonstrated that the prominences are largest and most numerous.

Now let us carefully study the observed facts, and see in what direction they seem to point. I premise that in this place I pay no attention to the atmospheric glare theory. It was permissible in my former paper to discuss all the theories, profound or shallow, which had been urged in explanation of the corona; for, in fact, unless that had been done, a popular essay on the subject would have been incomplete. But we are now engaged on a more arduous task; we are proposing to analyse evidence of interest and importance; and therefore we must no longer afford room for the consideration of ideas, which in the presence of all the evidence now available can be regarded only as puerilities.

The associations we have to explain are somewhat numerous, and at first view most perplexing. First, there is the demonstrated association between the solar spot-zone and the larger prominences; secondly, that between the larger prominences and the bright inner portion of the corona; thirdly, that between the inner corona and the outer, fainter, and more strikingly radiated portion of the corona; while, lastly, those long streamers or projections into which the radiations are seen under favourable circumstances to extend themselves, will require to be examined. We must also endeavour not to lose sight of a single recorded fact respecting either the corona, or the prominences, or the solar spot-zone.

The difficulty in explaining the connection between the spot-zone and the prominences consists in this, that prominences are seen outside the spot-zone and even at the solar poles and equator, though prominences so situated are smaller, less numerous, and last for a shorter time, than the majority of those which appear over the spot-zone. In one sense, indeed, it may be said that the whole circumference of the sun's disc at all times shows prominences, for the edge of the sierra is always marked by serrations which may be taken to be small prominences or the remains of larger ones.[1] We see, then, that notwithstanding the association between the prominences and the spot-zone, the prominences have a greater range than the spots. Furthermore, Respighi has noted—and Secchi is of the same opinion—that it is rather over the *faculæ* or bright streaks which surround the spots than over the spots themselves that the prominences are most strikingly developed. We find ourselves thus brought into the presence of those most perplexing problems which are

[1] This we know from eclipse observations since 1842. When the supposed discovery of the sierra (in 1868) was first announced, it was imagined that the evidence showed the sierra to have a smooth outline. Professor Respighi soon after announced the jagged character of the sierra's edge, speaking also (I believe) in ignorance of the prior recognition of the sierra. It is now beyond question that the sierra nearly always presents a rough outline.

suggested by the behaviour of sun-spots and faculæ; by the curdled or mottled aspect which the sun's whole surface presents, but which is most marked in the zone of spots; by those much finer granulations which have been compared to willow leaves, rice grains, straw-thatching, and the rest: in fine, we have to deal with problems which have been attacked over and over again without as yet any seeming approach to a solution. Assuredly there is little promise of our obtaining an answer to the questions suggested by the corona, if we associate them just at present with other problems of such exceeding difficulty.

Let us then pass on to the next point, viz., the observed association between the coloured prominences and the inner and brighter part of the corona.

In order to see the real importance of this association it is well to remember what has been learned about the prominences. The researches of Zöllner and Respighi leave no room to question the fact that the prominences are phenomena of eruption. We seem clearly to have to do with masses of glowing vapour flung violently forth from some considerable depth beneath the visible surface of the sun. And quite apart from any theories as to the cause of these eruptions, we can make little question that before each eruption the gas eventually erupted had been prevented from escaping by some temporary barrier of considerable resisting power. No otherwise can we explain the violence of the eruption or the signs of an energy acquired and concentrated by compression. In fact, even though an explosion of some previously quiescent substances —such an explosion, I mean, as takes place when gunpowder is ignited—were here in question, yet unless there were some restraining or imprisoning matter the explosion could never have that definite propulsive character which we see in Zöllner's and Respighi's eruption-prominences. The *Trennungschicht* of Zöllner seems, in any case, a necessary part of any theory by which these prominences are to be adequately explained.

Now, this assumed, can we see any reason why the bright parts of the corona should seem to extend farther from the sun over the large prominences, or rather over the regions of large prominences, than elsewhere? If there is an atmosphere extending far above the prominences, and quite distinct both from the prominences and the sierra—as seems all but certain—yet why should the projection of prominence-matter into that atmosphere cause any perceptible expansion? Either much more matter than is contained within the visible prominences is flung into this part of the atmosphere, or some effect is produced by the erupted prominence-matter in expanding, illuminating, or heating the surrounding portions of the solar atmosphere. Yet beyond question neither of these interpretations is acceptable as it stands; nor does spectroscopic analysis of the inner part of the corona during total eclipse afford a particle of evidence that these expanded portions are in any different condition from the shallower portions of the inner corona; whereas, on either of the above suppositions—whether a quantity of fresh atmospheric matter had been added where the corona is deeper, or whether there had been a great and striking extension of a certain condition of its substance-matter—it would seem inevitable that the spectroscope would show some signs of the change.

Yet we seem no nearer a solution if we suppose the inner corona to indicate the presence of non-atmospheric matter in the sun's neighbourhood, that is, of matter not constituting a veritable solar envelope. Supposing for example that a theory to which I had been for many months inclining were the true one—that meteoric and cometic systems circling around the sun, many of them passing very close to him, and these much more intensely illuminated than the more distant, were the real source of the coronal light; yet what explanation can we find of the observed extension of this inner part of the corona over the regions of great prominences? How can the occurrence of great solar eruptions affect meteoric or cometic matter travelling myriads of miles above those regions? We cannot at any rate suppose that a sufficient superiority exists in the light-giving or heat-giving powers of the prominence-regions to explain so marked a difference; for we can test the spot-zones in both respects, and we find on so doing that no such superiority exists. Nor again is it easy to suppose that some special form of electrical or magnetical action exerted above the spot-zones and prominence-regions is the true explanation of the peculiarity. That in some way or other electricity is at work in the production of the coronal light, may well be believed; and further, that electrical action is at work in some special manner above the prominence-regions is far from improbable; yet to explain a coronal extension by the assumed extension of this electrical action is to explain our problem by another of far greater difficulty.

But if we find a source of perplexity in the extension of the bright inner portion of the corona where the prominences are most markedly developed, our perplexity is greatly increased when we see that the outer radiations reach their greatest extension over the extensions of the inner corona. Remembering that the radiations seen even under relatively unfavourable circumstances often extend to a distance from the sun nearly equal to his own apparent diameter—that is, at the very least (assigning no effect to foreshortening for example), to about three-quarters of a million of miles, or three times the moon's distance from the earth—it is difficult indeed to understand how matter so distant from the sun should seem to be associated with the inner corona (seemingly forming an irregular envelope so much nearer to him), and finally with the prominence-regions so far again below the limits even of the inner corona. Yet more difficult does the problem become when we recall the fact that the corona is not a glory of light, painted, so to speak, upon the surface of the vault of heaven, but belongs to what may roughly be regarded as a globe of matter, a globe exceeding at least twenty times in volume the volume of the solar globe which it encloses. What is that mysterious association existing between the sun's surface-regions and the matter included within these gigantic outer regions which causes disturbances taking place in the former to be reflected, as it were, to the outermost limits of the latter?

But finally, when we consider the long radial streamers which have been observed during total eclipses as prolongations of the inner radiations, how shall we face the yet more serious difficulties which suggest themselves? If these outer streamers had belonged, as I was once inclined to suppose, to meteoric systems circling around the sun, there would be no reason whatever why they should be directed as if towards his centre. A streamer might, by a coincidence, be so seen, but the coincidence would be uncommon. And even were it common, there would still be no reason why these meteoric streamers should be directed always along those radial lines which the corona affects. We have something here to explain which the meteoric theory is insufficient to deal with. Let me be clearly understood. The meteoric theory *per se* is demonstrably correct. No one who is acquainted with the evidence bearing on the subject can for a moment doubt that the sun's neighbourhood is traversed by countless millions of meteoric families; nor again can anyone acquainted with the evidence feel any doubt that a large quantity of light must needs be reflected during total eclipse from those meteoric bodies which lie towards the sun's place; but the meteoric theory *per se* cannot account for *all* the phenomena presented by the corona, nor (especially) for the particular series of phenomena dealt with above.

And before proceeding farther, I would note that the difficulty now considered had not escaped my notice at an earlier stage of the inquiry. I have considered it —as a difficulty (that is, without endeavouring to explain it)—in the chapter on the Corona in my book on the Sun, and it caused me to write thus hesitatingly respecting the constitution of the corona: '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; 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.' I was able at that time to express confidence only in one general fact, viz. 'that the corona and zodiacal light form a solar appendage of amazing extent and importance, not being mere terrestrial phenomena, but worthy of all the attention astronomers and physicists can direct to them.'

But somewhat later, while preparing a paper on the Eclipse Expeditions for the January number of the 'Popular Science Review,' a renewed examination of the same difficulty led me to approach somewhat closely to that theory which I shall presently advocate, not indeed as certainly established or even nearly so, but as the one which seems to accord best with the evidence thus far adduced. As often happens, the explanation of one great difficulty was suggested by associating with it another scarcely less important. I quote the passage *in extenso*, as conveniently leading up to the theory to be enunciated farther on. 'If I were willing to hazard a speculation as to the structure and physical cause of the coronal beams,' I then wrote, 'I should associate them, I think, with the tails of comets, and regard them as phenomena indicating the action of some repulsive force exerted by the sun. Sir John Herschel has pointed out that we have demonstrative evidence of the real existence of repulsive forces exerted by the sun with great energy under certain conditions and upon certain forms of matter. A source of perplexity exists, however, in the relative narrowness of these beams, whose apparent cross-section, as delineated by most observers, is far less than the apparent diameter of the sun. One would thus be led to infer that the real seat of these repulsive energies lies far beneath the solar photosphere. It is worthy of notice, too, that the beams usually appear to extend from the zone of spots; and one might almost infer that the repulsive action is exerted with peculiar energy in lines extending from the sun's centre towards the so-called spot-zones.'

The relatively narrow beams here spoken of are those which make up the great radiations. Under favourable circumstances the structure of the corona is found to be by no means so simple as it is commonly supposed to be; and it seems established by evidence too striking to be overlooked that close by the sun this structure assumes the form of radial jets issuing nearly at right angles to the sun's edge. This is a peculiarity which seems likely, if carefully studied, to throw some noteworthy light on the problems we are dealing with. Let us first examine the evidence on which it rests.

The corona has seldom been examined under favourable atmospheric conditions with a telescope of considerable power. Nearly always, when such telescopes have been employed during eclipses, the observer's attention has been specially directed to the coloured prominences. It happened, however, that during the important American eclipse of 1869 a party of observers, headed by Mr. W. S. Gilman, jun., of New York (a well-known telescopist), very carefully studied the intimate constitution of the corona. Mr. Gilman himself employed an excellent telescope four inches in aperture, and others of the observing party were armed with good though smaller telescopes. The weather also was all that could be desired. So that perhaps there is no other instance in the history of eclipse-observations where the study of the corona's structure was prosecuted under more favourable conditions.

Here then is Mr. Gilman's report of the aspect of that part of the corona which lay close by the moon: 'The corona was composed of an infinitude of fine violet, mauve-coloured, white, and yellowish-white rays, issuing from behind the moon.' Mr. Farrell, who observed in company with Mr. Gilman, spoke thus on the subject of the corona: 'It was a silvery gray crown of light, and looked as if it was the product of countless fine jets of steam issuing from behind a dark globe.' In the 'Quarterly Journal of Science' for October 1870 Mr. Gilman gives a more detailed account of the fine radial lines. He says: 'The absorption-bands of the solar spectrum occurred to me at the time as an illustration of the delicate striations in these portions of the corona. In the case of one gap a multitude of fine violet lines were compressed into a space about ten degrees' (of arc round the sun's disc) 'in width, forming to my mind one of the most beautiful features of the eclipse. The same striated appearance was noted in other regions of the corona, though in a less striking degree.'

The solution *obviously* suggested is that these seeming jets were real jets—that they indicate the existence of an eruptive action exerted beneath certain portions of the solar surface. But though this is the obvious answer to the questions suggested by observed appearances, it is one which seems at a first view altogether too startling to be accepted. For although there is nothing absolutely incredible in the conception that masses of gaseous, liquid, or solid matter should be flung to a height exceeding manifold that of the loftiest of the coloured prominences, yet the idea (first enunciated by Mr. Mattieu Williams in his 'Fuel of the Sun') seems too much opposed to formerly-received conception to be readily admitted. Before dismissing the supposition, however incredible, it will be well to remember that on the one hand it is not opposed to any known laws, physical or dynamical, while on the other it is supported by much evidence of a kind that it would be unsafe to overlook. Combining these considerations with the circumstance that the supposition seems to promise a solution of some of the difficulties which have been considered above, while evidence of the most striking character has been obtained respecting the sun's eruptive energies, it seems desirable that we should examine it very carefully.

It will be obvious even at a first view that *if* the sun has power to propel streams of gaseous, liquid, or solid matter to distances corresponding to the observed extension of the coronal radiations, all those difficulties which depend on the association of the radiations with the bulging portions of the inner corona, and the regions of most active prominence-formation, vanish at once. The observed relations are in fact what the theory would require: if others were noticed, the theory would have to be rejected.

But is it conceivable that the required velocity of projection or eruption can be given to such streams or masses? Or rather have we any evidence which could render the conception credible? For I take it that as regards the sun's potential energies in respect of eruptive action, it would be very difficult to fix any definite limit. The forces at work within the sun are so enormous that there would be nothing absolutely incredible in the idea that a globe as vast as our earth, or even as one of the giant planets which travel outside the zone of asteroids, might be propelled from him with a velocity sufficing to carry it within the sphere of attraction ruled over by some other sun.

We have in the eruption prominences the means of gauging in some sort the sun's propulsive energies. It seems to have been proved beyond possibility of question that some of these prominences have been projected from the sun with a velocity of about one hundred and twenty miles per second.[1] Zöllner gives this as the result of direct observation, and the inference has been confirmed by observations made with the spectroscope. Motions within the sierra at such a rate, as well as up-and-down motions in the hydrogen around the photosphere at a similar rate, seem to leave no question that glowing hydrogen is at times urged with this inconceivable velocity through the solar atmospheric envelope.

But now, assuming with Zöllner that the phenomena of eruption are due to a force of compression having its birthplace far below the photosphere, and in regions where the atmospheric resistance must enormously exceed the resistance occurring above the photosphere, we may conclude that in the beginning the velocity is far greater than that of which we can alone become cognisant. The eruption-prominences when first discernible by us (I speak of their first appearance, it will be understood), are rushing upwards with a velocity

[1] The two white spots seen by Carrington in September 1859 travelled at a rate of at least 120 miles per second. If their paths were foreshortened, as is most probable, their rate must have been much greater.

which is but what is left of the far larger velocity originally imparted to them. This at least seems no unreasonable inference, but on the contrary highly probable. And again, although the larger proportion of the erupted matter may be glowing hydrogen, yet it seems far from unlikely that other and denser matter, even perhaps liquid matter, may be propelled along with the hydrogen—much as water, sand, and mud are propelled along with steam in the explosions of an Iceland geyser. And precisely as the water, &c. thus propelled from a geyser reaches to a far greater height than the steam, as is shown by the place of the cloud into which the latter resolves itself, so it seems reasonable to infer that any denser matter, and especially any liquid or solid matter projected during a solar eruption, would reach to a far greater height than the erupted hydrogen, or, which is more to my present purpose, would rush from the sun's surface with a far greater proportion of the velocity originally imparted. When we remember that observations made during the recent eclipse serve to show that immediately above the solar photosphere there exists an atmosphere (which, to be sensible at all, can scarcely be less than two hundred miles deep) compounded of the vapours of nearly all the elements known to exist in the sun,[1]

[1] I refer to the observations made both by Professor Young and Mr. Pye. At the moment of totality a spectroscope having its slit placed tangentially to the sun's edge at the place of contact with the moon's shows for a second or two all the Fraunhofer lines reversed; that is, bright instead of dark. I had ventured some time before to express my belief that the compound atmosphere thus indicated really exists *at all*

and therefore necessarily (under the enormous influence of his attractive energy) exceedingly dense, it will not, I think, be unreasonable to assign as the initial velocity imparted far below even the photospheric level, a rate exceeding twice or thrice that observed within the sierra. This would give us for the velocity of such denser or even liquid matter as we have supposed to be erupted along with the hydrogen, a rate of between two hundred and forty and three hundred and sixty miles per second. The least of these velocities would suffice to carry a projectile as far from the sun as the outer limits of those coronal radiations observed under ordinarily favourable conditions. A velocity of three hundred and sixty miles per second would suffice to send a projectile as far from the sun as our earth's orbit. It would need but an addition of twenty miles per second to this velocity to send a projectile clean away from the sun; while if there were any further increase, the erupted matter would not only be propelled with force enough to carry it beyond the domain of the sun, but when it eventually entered the domain of another sun, then, over and above the velocity imparted to it by the attractive energies of that sun, it

times above the photosphere; though I felt by no means confident that observation could thus have revealed its existence. Observers, who had once seen hundreds of the Fraunhofer lines reversed in this manner, had assumed the phenomenon to be altogether exceptional, and that it is *below* the photosphere that the reversal of the Fraunhofer lines really takes place. This had always seemed to me an untenable proposition for reasons I have indicated elsewhere. But assuredly, even if there were more evidence in its favour, this opinion would not suffice to negative such observations as those made by Professor Young and Mr. Pye.

would possess a proportion of the velocity imparted to it by its parent sun.[1]

It would almost seem, then, that we are proving too much; that velocities not so much greater than those necessary to explain the coronal radiations as phenomena of eruption would cause our earth to be saluted with solar missiles; and also—for we must push this argument to its limit—our sun sending within the domains of other stars masses still possessing a portion of their initial velocity, we should be led to expect that from out *those* domains masses would reach *our* solar system with extra-solar velocities. In the former case it is clear that the fall of large meteoric masses (assuming solar and stellar eruptions to vomit *such* bodies) should be a phenomenon occurring oftener in the day-time than at night—that is, over the hemisphere turned towards the sun. Strangely enough, this preponderance of day-falls actually exists in a very marked degree, and has never, so far as I know, received any adequate explanation. Yet again if meteoric masses reached us with extra-solar velocities—that is, with velocities greater than the sun could impart to matter drawn by him from an indefinitely great distance up to our earth's place—then the observations which have been made upon the motions of meteors and falling stars ought to have revealed this excessive velocity. Here again it happens that velocities far

[1] For a complete investigation of the subject here considered, so far at least as the sun's propulsive energies are concerned, the reader is referred to the author's 'Essays on Astronomy,' pp. 210, *et seq.*

greater than could be explained by the sun's attractive energies [1] had been assigned by very careful observers to certain meteoric bodies; and it had been pointed out (I think first by Mayer) that these velocities presented a difficulty very hard to interpret; nor, so far as I am aware, had any satisfactory explanation been afforded. Quite apart too from the perhaps questionable observations of meteoric velocities, many comets had been proved beyond all cavil to travel on hyperbolic orbits, the velocity in such orbits being (in part) necessarily extra-solar. The observed association between meteor-systems and certain comets renders this evidence as satisfactory as the direct measurement of meteoric velocities.

Have meteors, then, been expelled from the stars? Surely it might be supposed that such an idea is too wild to be entertained for an instant. Let us pause, however, to learn what the meteors themselves may have to tell us on this point. We are travelling rather far, it may seem, from the solar corona; but we shall

[1] The greatest possible velocity with which a meteor could enter the earth's atmosphere under solar influence alone would amount to 44·4 miles per second—obtained by adding the earth's perihelion velocity of 18·5 miles per second to the maximum velocity of 25·9 miles per second, which the sun could give to a body at the earth's distance. But the observed velocities of meteors range from 17 to 80 miles per second. In the case of a meteor rushing at the rate of 80 miles per second through our atmosphere, we know certainly that 35·6 miles per second of that velocity is extra-solar, or has been imparted to the meteor by other suns than ours. Neither the earth's rotation nor her power of attraction need be considered in this inquiry; since neither cause can give meteors any velocity comparable with that due to their own and the earth's motion.

return to it presently; and in the mean time these are questions which *must* be answered before we can safely adopt such a theory as the one we are dealing with.

Now there is one fact respecting meteors which had long been a source of great perplexity to me. Soon after I had published my treatise on 'Saturn and its System,' Mr. Sorby, F.R.S., sent me notes of his researches into the structure of meteoric bodies, as bearing on the questions which I had discussed in Chapter V. (on the nature of the Saturnian Rings). He found evidence that the substance composing meteors had been vaporised while existing under enormous pressure—'in mountain masses,' as he expressed it. Whence and how had come the enormous temperature and the enormous pressure thus indicated? On any of the ordinary theories respecting meteors I can see my way to no explanation. Collision, friction in passing through resisting media, and other like processes, are altogether inadequate to account for the observed facts. But if meteors had their birth in far-off suns, in the very midst of that intense heat and light which causes those suns to shine clearly in our skies from beyond depths wholly inconceivable by us, *then* the explanation of what Sorby's microscope has revealed is found at once. We should *expect* to find the substance of meteors in precisely that condition which he describes.

But then, if meteors were thus propelled from beneath the surface of sun or star, one might expect that chemical as well as microscopic traces of their origin might be recognised. Supposing, for instance, that the hydrogen of an eruption-prominence is to be compared with the smoke which rushes from the mouth of a cannon, volleys of meteors being the missiles, is it not likely that these meteors would show some signs of having been originally surrounded by intensely hot hydrogen existing at an inconceivably enormous pressure? It has been established by the researches of chemists, and especially by those of the late Professor Graham, that iron solidified under such conditions would condense within its substance a considerable proportion of hydrogen. Since, then, meteorites contain always a large quantity of iron, the question suggests itself whether that iron shows any signs of having been surrounded by hydrogen when as yet the metal had not solidified. The answer comes in no doubtful terms. To Professor Graham himself we owe the examination of meteoric iron and the definite enunciation of the conclusions warranted by his prior researches. He examined a piece of the meteoric iron of Lenarto, which, says Mr. Mattieu Williams, has been shown by the analysis of Werle to consist of 90·883 parts of iron, to 8·450 parts of nickel, 0·665 of cobalt, and 0·002 of copper. When a volume of 5·78 cubic centimètres of this iron was heated to redness, 'gas came off rather freely—namely, in thirty-five minutes 5·38 cubic centimètres, in the next one hundred minutes 9·52, and in the next twenty minutes 1·63 cubic centimètres'—in all, in rather more than two hours and a half, no less than 16·53 cubic centimètres, or about three times the volume of the iron itself. 'The first portion of the gas collected,' says Professor Graham,[1] 'had a slight odour, but much less than the natural gases occluded by ordinary hydrogen. It did not contain a trace of carbonic acid.' The second portion of gas collected (consisting of 9·52 cubic centimètres) gave of hydrogen 85·68 parts per cent., the rest consisting of nitrogen and carbonic oxide. 'The Lenarto iron appears, therefore, to yield 2·85 times its volume of gas,' says Graham, 'of which 86 per cent. nearly is hydrogen, the proportion of carbonic oxide being so low as 4½ per cent.' But the gas occluded by iron from a carbonaceous fire is very different, the prevailing gas then being carbonic oxide. For comparison a quantity of clean horse-shoe nails was submitted to a similar distillation.' This metal gave 2·66 times its volume of gas; the first portion collected contained only 35 per cent. of hydrogen, 50·3 per cent. being carbonic oxide, 7·7 per cent. carbonic acid, and 7 per cent. nitrogen; the second portion gave no carbonic acid, but 58 per cent. of carbonic oxide, and only 21 per cent. of hydrogen. The contrast between these results and the former is very marked. But

[1] I quote from Mr. Williams's work, *The Fuel of the Sun*, not having Graham's original paper by me. This work is well deserving of careful study, especially by the astronomer, too often apt to forget the teachings of other sciences than his own. Doubtless there is much in the book which is not in accordance with known dynamical laws, much therefore respecting which Mr. Williams has something to learn from astronomers; but on the other hand he has much—every well-informed student of chemistry has much to teach astronomers.

let us hear Professor Graham's own reasoning on the subject.

'It has been found difficult,' he says, 'to impregnate malleable iron with more than an equal volume of hydrogen under the pressure of our atmosphere. Now the meteoric iron (this Lenarto iron is remarkably pure and malleable) gave up about three times that amount without being fully exhausted. The inference is that *the meteorite has been extruded from a dense atmosphere of hydrogen gas,* for which we must look beyond the light cometary matter floating about within the limits of our solar system.' . . . 'Hydrogen has been recognised in the spectrum analysis of the light of the fixed stars by Messrs. Huggins and Miller. The same gas constitutes, according to the wide researches of Father Secchi, the principal elements of a numerous class of stars, of which *a* Lyræ is the type. *The iron of Lenarto has no doubt come from such an atmosphere, in which hydrogen greatly prevailed. This meteorite may be looked upon as holding imprisoned within it, and bearing to us, the hydrogen of the stars.*'

So far, then, is the theory we are dealing with from being negatived by the startling conclusions to which it seems to point, that on the contrary it seems to afford an account of startling facts which appear in no other way explicable. But let us consider whether the corona itself supplies any further evidence on the subject.

The coronal spectrum is compound, consisting of certain bright lines superposed on a continuous spectrum. Now the bright lines correspond to those seen in the auroral spectrum, and had been held to indicate that the corona is a perpetual aurora. But a great difficulty was suggested by the question how the electrical action thus indicated could be excited. If the theory we are now upon be correct, the rush of the erupted matter through even the exceedingly tenuous medium at great distances from the sun should produce precisely such an effect. The fact that one of the lines of the coronal spectrum belongs to the spectrum of iron may be regarded as supplying subsidiary evidence of some weight. As regards the continuous spectrum, indicative of liquid or solid matter in a state of incandescence, the theory obviously requires that such a spectrum should be discernible.[1]

It will be seen that according to the theory we are dealing with, the prominences, large and small, come to be regarded as the signs of the shooting forth of liquid or solid masses or streams of matter; and it might be asked whether any direct evidence has ever been obtained of the outpouring of such substances. It might well be that no such evidence was available; for it must be remembered that such masses of matter must needs be very far less in actual volume than the prominences; that owing to their rapid motion their detection would be very difficult; and that as their

[1] It seems not unlikely that far the largest portion of the corona's light corresponds to the continuous portion of the spectrum; although, being spread over a considerable range, this light may seem relatively far less brilliant in the spectrum itself than that constituting the bright lines.

light would give a continuous spectrum, their detection by means of the spectroscope would be almost out of the question. Still, swiftly as they are supposed to move, a velocity even of three hundred miles per second would not cause them at the vast distance of the sun to pass very quickly across the field of view of a telescope; on the contrary, if viewed during total eclipses (the only occasions on which they could be expected to become telescopically visible), they could be watched quite readily, their motion, even during so long an eclipse as the Indian one, sufficing only to carry them over about such a distance as would correspond to the height of one of the tall prominences. It would be their minuteness rather than their swift motion which might be expected to defeat scrutiny. Single masses indeed could hardly be seen, but only streams or clusters formed of many masses. I think the exceedingly brilliant flakes seen by Mr. Gilman in the largest prominence visible during the eclipse of 1869 may be regarded as presenting the appearance to be expected. 'They stood out,' he says, 'as if totally disconnected from the rest of the prominence.' Again, there is an observation of Zöllner's which seems to bear on this matter. I have said that only with the telescope could the liquid or solid matter be actually seen. In the spectroscope they would give a continuous streak; and unless the light of the matter were very much brighter than that of the prominences, such a spectrum could not be recognised. For it is the concentration of the prominence-light into a few definite parts of the spectrum's length which enables us to see the prominences when the sun is not eclipsed. The light from the liquid matter being spread out into a continuous streak would be matched on equal terms with the light from our own illuminated atmosphere, which we know blots out the prominence-light altogether. Yet Zöllner, on June 27, 1869, made the following remarkable observation: 'As soon,' he writes, '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 protuberances also appeared*.' Zöllner proceeds to remark that 'the phenomenon can be explained 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.'

It may be added that the researches of De la Rue, Stewart, and Loewy seem to prove that 'the faculæ of a spot have been uplifted from the very area occupied by the spot, and have fallen behind from being thrown up into a region where the velocity of rotation is greater.' This, of course, would correspond with the results which the theory we are dealing with would suggest. And it may be noticed that regarding spots as phenomena of eruption—that is, *beginning* with eruption—we can find a reason for their occurrence being associated, as De la Rue and his fellow-workers believe, with the relative proximity of the planets. For eruptions and earthquakes on our own earth, stable as its substance undoubtedly is by comparison with the sun's, have been observed to occur more frequently when the moon is in perigee; and again Herschel has explained the predominance of active volcanoes and earthquake regions along shore-lines, as depending on the seemingly insignificant changes of controlling pressure due to tidal action. How much more, therefore, might we expect that the solar equilibrium would be disturbed by planetary action, when all that has been revealed respecting the sun tends to show that the mightiest conceivable forces are always contending beneath his photosphere, one or other needing only (it may well be) the minutest assistance from without to gain a temporary mastery over its rivals! And if, as recent observations tend to show, the mightiest of the planets sympathises with solar action—if, when the sun is most disturbed, the belts of Jupiter are also subject (as of late and in 1860) to strange phenomena of change, how readily do we find an explanation of what otherwise would seem so mysterious, when we remember that as Jupiter disturbs the mighty mass of the sun, so the sun would reciprocally disturb the mass of the largest of his attendant orbs.

In conclusion, let me remark that I by no means urge the somewhat startling theory here put forward as definitely to be adopted. It does seem, however, to afford an explanation, or at least some account, of many striking facts which at first sight seem in no way associated; and therefore it is not to be safely dismissed without further and very careful examination. I have made no reference hitherto to the circumstance which first directed my attention in a special manner to this particular theory—the fact, namely, that with the photographs of the late eclipse before me, the conception of *eruptive action* seemed forced upon me as the true explanation of the corona's peculiarities. But the direct evidence thus afforded by the aspect of the corona is not to be neglected, simple though it is in character. It is in this respect that the photographs of the late eclipse are, as I think, calculated to be most useful. We can study at our leisure appearances which during an eclipse must be hastily examined, under circumstances not favourable to the calm exercise of the reasoning faculties. As time progresses, and other photographic records of the corona are placed at our disposal, I believe the definite solution of the problem it presents may be confidently anticipated.

Fraser's Magazine for April 1871.

THE CORONA AS A PHENOMENON OF ERUPTION.

Before adducing fresh evidence in favour of the eruption theory, I think it desirable to make a few preliminary remarks on the subject of the accumulation of evidence in favour of theoretical views. It has been urged once or twice, and that by persons to whose opinion I feel bound to pay respectful attention, that in advocating a theory I have shown myself somewhat too apt to adopt a tone resembling special pleading—that I have endeavoured to show not merely that certain arguments favour the theory, but that all the known circumstances of the case point in the same direction. This has been said, in particular, of those reasonings by which I endeavoured to convince others as I was convinced myself, long since, that the corona is a solar appendage, and not, as had been urged, a mere phenomenon of our atmosphere or due to the illumination of matter lying between the earth and the moon. I dwelt very earnestly on my views about the corona, because I felt that when the evidence was duly weighed, the atmospheric theory could not but be regarded as *disposed of*, and that the labours of eclipse observers could be directed to determine the nature of the solar surroundings, instead of being wasted on the inquiry whether in the corona solar surroundings were in question at all. It was urged (and not untruly) that a variety of circumstances seemed to favour the atmospheric theory, and that the consideration of these circumstances had led others to entertain an opinion different from that which I was advocating.

The circumstance that the observations made during the late eclipse have at length definitely convinced even those who had been most doubtful, that the corona is a solar appendage, and that the able observer, Janssen, has, in the most uncompromising terms, enunciated the opinion which I advocated so earnestly two years ago,[1] places me in a position to defend with some confidence that method of advocacy which has been depreciated.

It seems to me then, that those who oppose the accumulation of evidence in favour of a theory—that is, the attempt to show that *all* the known circum-

[1] He writes, 'Le résultat de mes observations à Sholoor indique, sans aucun doute, l'origine solaire de la couronne et l'existence de matières au-delà de la chromosphère;' and again, 'Rien de plus beau, de plus lumineux, avec des formes spéciales qui excluent toute possibilité d'une origine atmosphérique terrestre. . . . Je crois *tranchée* la question de savoir si la couronne est due à l'atmosphère terrestre, et nous avons devant nous la perspective de l'étude des régions extra-solaires, qui sera bien intéressante et féconde.' The expressions here used are singularly emphatic, and they are the more effective that Janssen himself had adopted no definite theory of the corona, while M. Faye, to whom they were addressed, had once been the chief advocate of a non-solar interpretation—though (as appears from some expressions in Janssen's letter) M. Faye had of late adopted the theory now shown to be the just one.

stances favour the theory—have not sufficiently kept the fact in mind that the only circumstance which can justify the advocacy of a theory at all, is the conviction on the part of the theorist that the theory is the true one. If he merely conceives that the theory is more probable than some other theory or theories which have been discussed, he should only present those facts which appear to favour the theory, making careful mention of the circumstance that other facts seem less favourable or even contradictory to the theory. Until he can show that not one known fact opposes the theory, or seems difficult of explanation in connection with it, he has no right to advocate the theory at all (in the proper sense of these words). But the case is altogether different when he is convinced that a theory is the correct one. For this conviction can only arise from the fact that every known fact has been carefully compared with the requirements of the theory, and found to be altogether in accordance with it. It is only to false but plausible theories that Voltaire's description applies: 'A theory is like a mouse which passes through nineteen holes, and is stopped at the twentieth.' The very first duty of a theorist who conceives that he has lighted upon the true explanation of any physical phenomenon, is to test his theory by bringing it into the presence of every single known fact respecting that phenomenon, and to forsake it at once if a single fact is opposed to it. Supposing it stands such a test, how can the theorist otherwise present it to the world than as in accordance with all known facts, showing carefully that this is so in the case of each particular fact? His doing this may have an air of special pleading to those who conceive that he is bringing forward facts thought of *after* the theory had been adopted; but when it is understood that those facts have been the tests to which the theory was exposed *before* being adopted, it will appear not only natural, but necessary that they should be adduced.

It seems to me, indeed, that the course reprehended as resembling special pleading is so far from being objectionable that it is to be regarded as the only proper course for the theorist; and I am sure that if this method were applied rigidly to every theory suggested to the minds of scientific inquirers, we should have fewer ill-digested theories under discussion than at the present time. Speaking for myself, I can safely assert that if the method has on a few occasions caused me to be somewhat confident respecting theories which I have advocated and still hold, it has again and again caused me to abandon theories which I had begun to regard with favour and should undoubtedly have still entertained but that on some one or other point they would not bear the test thus applied to them.

Now it will be noticed that the eruption theory of the solar corona was not *advocated* in my former paper. It was described, and the evidence, which seemed favourable to it, was discussed at considerable length; but although no evidence suggested itself as unfavourable to the theory, there was still not that overwhelming array of facts necessary in the case of a theory which is to be deliberately advocated.

In particular, I recognised certain difficulties which no evidence available when I wrote enabled me to remove. For instance, although it seemed reasonable to infer that matter propelled from the sun must be enormously retarded by his atmosphere, and that the rarer matter so erupted would be more retarded than the denser, yet there was no direct evidence of such retardation. All the prominences which had been regarded by Zöllner, Respighi, and others, as phenomena of eruption, had been observed when already formed, and the estimate of the velocity of ejection had been simply based on the height attained by the erupted matter. In no single instance had the actual upward motion of prominence matter been watched, measured, and timed. Again, the theory seemed to require that the eruption-prominences should be somewhat more markedly limited to the solar spot-zones than at the time appeared to be shown. Respighi had found the prominences on the spot-zone larger and better developed than those in the sun's equatorial or polar regions; yet prominences were not wanting in the last-named parts of the sun, and some of those seen there have been, as Respighi's pictures show, of no inconsiderable magnitude. Yet again, it seemed that if other matter, as the metallic elements, were propelled along with the glowing hydrogen of the prominences, the spectroscopic analysis of the prominences ought to reveal traces of the presence of such matter more markedly in eruption-prominences than in others, even if traces of such matter ought not to be wholly wanting in prominences not apparently eruptive.

Such were some of the doubts suggested by the analysis of the evidence available a year ago. But all these doubts have been removed by evidence which has since been obtained.

Father Secchi has conducted a long series of careful researches into the appearance and behaviour of the coloured prominences; and the results of his researches, recently communicated to the Paris Academy of Sciences (and enthusiastically received), bear importantly on the subject of solar eruptions.

Passing over other matter not directly related to our subject, it is noteworthy that Secchi divides the prominences into two classes, presenting very marked contrasts. He calls the prominences of the two classes *jets* and *plumes.* The jets alone he regards as phenomena of eruption, while the plumes appear to be not uncommonly formed in the upper regions of the solar atmosphere, and as though by the motion of solar air-currents.

Now if we carefully study the characteristics of the jet prominences, we shall find that they almost force upon us the eruption theory of their origin. They are in the first place much more luminous than the plume prominences, their great luminosity implying that they have been propelled from the intensely hot regions below the solar photosphere. 'The luminosity of the jets is always very great,' Secchi says, '*their roots* *being more luminous than the rest of the solar surface.* Their appearance is extremely beautiful, and the most splendid display of fireworks would fall far short of realising to the imagination the magnificent glory of the sublime spectacle they present. Their light is so bright that they can be seen through the light clouds into which the chromatosphere[1] breaks up.' Again, as phenomena of eruption we should expect them to form rapidly and soon to disappear. On this point Secchi says, 'A characteristic of these prominences is their short duration; they rarely last an hour, frequently only a few minutes.' On the other hand, the plume prominences frequently last for two or three days in succession. Again, if the jets are phenomena of eruption they should exhibit under spectroscopic analysis the traces of the presence of other matter than the hydrogen common to all the prominences; and they should also exhibit the spectroscopic indications of rapid motion (in other words, displacement of their bright lines either towards the red or towards the violet end of the spectrum). Here again Secchi supplies the required evidence. 'The spectrum of the jet prominences indicates,' he says, 'besides hydrogen, the presence of many other substances; and I frequently observe in them a great variability in the refrangibility of the rays.' Lastly, Secchi tells us that the jets are

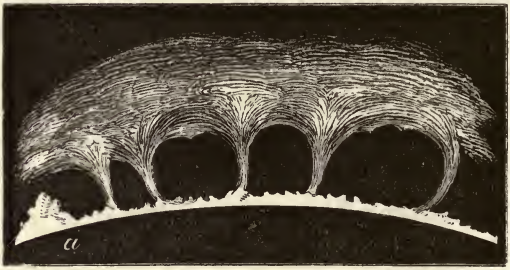
[1] I prefer this form of the word to that more commonly employed—viz. *chromosphere.* It can scarcely be believed indeed that the word chromosphere can long remain unchanged. It is, of course, as incorrect as *phosphere* would be for photosphere, *phography* for photography, *chromic* for chromatic, and so on.

distinguished from the plumes in being limited to the neighbourhood of the spots.

In the following passage Secchi sums up his results respecting the distinctive characteristics of the two orders of prominences: 'In distinguishing between jets and plumes,' he says, 'I have no intention to decide whether plumes may not also be jets. The real distinction appears to be that in jets a part of the photosphere is lifted up (let this be compared with De la Rue's explanation of the phenomena of solar spots.—See preceding essay, p. 296), whereas in the case of plumes it is only the chromatosphere which is disturbed. It does not appear to be established as a fact that all prominences require an orifice of projection; and still less that the height of a prominence can be taken as the measure of the pressure which has projected the gaseous stream, since plumes have been seen to form themselves in the masses suspended in the free solar atmosphere, far above the possibility of liquid origin. The persistence of plumes is very remarkable as compared with the continuance of the jets. In spite of the mobility of the former, they may be found for two or three days in the same place, and towards the poles their existence lasts still longer; whereas the most beautiful jets generally last but a few minutes, in very rare cases a few hours. This confirms me in the opinion that sheaves are due to a veritable eruption, taking place at a great depth, the matter composing them having an exceedingly high temperature, and being propelled with immense velocity. The presence of jets and sheaves is the most certain sign that a spot is imminent. As to the connection between protuberances and the solar *faculæ,* it may be stated that jets, whatever may be their shape, are invariably accompanied by *faculæ*; but that plumes, more particularly if they are small, are often seen where there are no *faculæ.* A peculiarity worthy of notice is the feebleness of the light from prominences near the poles —an indication, as I have before stated, of less activity and a less powerful propelling force. The protuberances, both as to number and size, are in accordance with the solar activity as manifested by the spots; the fewer the spots the less numerous and the less extensive are the protuberances likewise.'

But although the evidence here cited removes several of the sources of doubt referred to above, since it clearly shows that eruptions take place in those parts of the sun which the theory points to, and that these eruptions present the features which we should expect if the theory be true, yet the main difficulty remains. So far we have had no absolute evidence in favour of the existence of velocities of outrush corresponding to the requirements of the eruption theory. Nay, to a certain extent, the evidence just presented tends to throw doubt on what had appeared to be satisfactory evidence of enormous velocities of ejection. For it may be remembered that I referred to the heights of certain prominences as indicative of such and such initial velocities; those velocities, namely, which would suffice to carry a projectile from the sun's surface to the observed altitudes, supposing there were no appreciable atmospheric resistance. But Secchi has proved that some of the very loftiest of the prominences are not phenomena of eruption at all; and although other tall prominences are demonstrably eruptive, yet a certain degree of doubt is thrown over the inference (which had before seemed trustworthy)

Fig. 1.



SOLAR CLOUD PROMINENCE, SEEN BY PROFESSOR YOUNG, SEPT. 7, 1871.

that the highest range of these prominences is solely due to the velocity of projection.

But it is precisely respecting this chief source of doubt that we have recently obtained the most satisfactory information. We no longer have to draw questionable inferences from the observed height of any eruptive prominence; but we have a case to deal with (and a single case is all we require in this instance) in which the actual upward motion of prominence matter was measured and timed.

On September 7, 1871, Professor Young, the eminent American spectroscopist, was observing a large hydrogen cloud by the sun's edge (fig. 1). This cloud was about 100,000 miles long; and its upper surface was some 50,000 miles, the lower surface about 15,000 miles, above the sun's surface. The whole had the appearance of being supported on pillars of fire;

FIG. 2.

THE SAME HALF AN HOUR LATER.

these seeming pillars being in reality hydrogen jets, brighter and more active than the substance of the cloud. At half-past twelve, when Professor Young chanced to be called away from his observatory, there were no indications of any approaching change, except that one of 'the connecting stems of the southern extremity of the cloud had grown considerably brighter, and was curiously bent to one side; and near the base of another, at the northern end, a little brilliant lump had developed itself, shaped much like a summer thunderhead.'

But when Professor Young returned, about half an hour later, he found that a very remarkable change had taken place, and that a very remarkable process was actually in progress. 'The whole thing had been literally blown to shreds,' he says, 'by some inconceivable uprush from beneath (fig. 2). In place of the quiet cloud I had left, the air—if I may use the expression—was filled with flying *débris*, a mass of detached vertical fusiform fragments, each from ten to thirty seconds (i.e. from 4,500 to 13,500 miles) long by two or three seconds (900 or 1,350 miles) wide, brighter and closer together, where the pillars had formerly stood, and rapidly ascending. When I looked some of them had already reached a height of nearly four minutes (100,000 miles); and while I watched them, they rose with a motion almost perceptible to the eye, until in ten minutes, the uppermost were more than 200,000 miles above the solar surface. This was ascertained by careful measurements, the mean of three closely accordant determinations giving 210,000 miles as the extreme altitude attained. I am particular in the statement, because, so far as I know, chromatospheric matter (red hydrogen in this case) has never before been observed at an altitude exceeding five minutes, or

135,000 miles.[1] The velocity of ascent also—167 miles per second—is considerably greater than anything hitherto recorded. . . . . As the filaments rose, they gradually faded away like a dissolving cloud, and at a quarter past one, only a few filmy wisps, with some brighter streamers low down, near the chromatosphere, remained to mark the place. But in the meanwhile the little " thunderhead " before alluded to had grown

Fig. 3.

CHANGED APPEARANCE OF THE SOLAR 'THUNDER CLOUD.'

and developed wonderfully into a mass of rolling and ever-changing flame, to speak according to appearances. First it was crowded down, as it were, along the solar surface; later (fig. 3) it rose almost pyramidally fifty thousand miles in height; then its summit was drawn down into long filaments and threads (fig. 4), which were most curiously rolled backwards and forwards like the volutes of an Ionic capital; and finally it faded away, and by half-past two had vanished like the other. The whole phenomenon,' adds Professor Young, 'sug-

[1] This, however, is a mistake, since Respighi had already seen prominences reaching to a height of 160,000 miles.

gested most forcibly the idea of an explosion under the great prominence, acting mainly upwards, but also in all directions outwards, and then, after an interval, followed by a corresponding inrush; and it seems far from impossible that the mysterious coronal streamers, if they turn out to be truly solar, as now seems likely, may find their origin and explanation in such events.'

Fig. 4.

THE SAME A FEW MINUTES LATER.

Here the complete narrative of this stupendous solar outburst has been given because the event requires to be thoroughly understood in its general relations, in order that inferences from details may have their full weight. But the point which mainly concerns us in the above account is the actual observed velocity with which the hydrogen wisps travelled upwards. This velocity is estimated by Professor Young at 167 miles per second; but his estimate relates only to the average velocity with which a certain range was traversed, not to the maximum velocity within that range, and still less to the initial velocity of ejection, which (even according to the ordinary law of projected bodies) must have been considerably greater. It will be remembered that such a velocity as 300 miles per second would be required for the ejection of matter to distances corresponding to the observed extension of the corona. It can now be shown that matter *must* have moved with such a velocity—nay, with a much greater velocity, during the eruption witnessed by Professor Young.

I may premise that it is by no means to be regarded as certain—perhaps it is scarcely probable—that the hydrogen seen travelling upwards was itself erupted matter. It may have been merely following the track of the matter actually erupted, being carried along that track by the currents generated during the inconceivably rapid outrush of the erupted matter. In this case we can safely infer an even greater velocity of outrush than would be deduced if we regarded the hydrogen as itself erupted.

Again, it is not certain that the eruption, whatever may have been its nature, took place actually at the edge or very close to the edge of the sun's visible hemisphere. If the scene of the eruption were at any considerable distance from the edge, the motion of the hydrogen wisps must have been, to some extent, foreshortened. In this case a greater range was in reality traversed, and the average velocity, as well as the inferred initial velocity, would be so much the greater.

Yet, again, Professor Young saw hydrogen wisps which had not yet reached a height of 100,000 miles carried to a height of 210,000 miles in ten minutes. His estimate of the average rate of motion is based on the assumption that such matter only travelled from a height of 100,000 miles to a height of 200,000 miles in ten minutes. We should obtain an appreciably larger estimate of the initial velocity by taking the actually observed limits of the range than by following Professor Young in regarding 200,000 miles as the true range.

But even if we neglect all these considerations, we shall yet be enabled, or rather we shall be obliged, to infer an initial velocity amply sufficient for the requirements of the eruption theory.

We should, of course, be justified in regarding the observed height of 210,000 miles as the limit of the flight of matter projected from the sun, and in thence inferring the velocity of outrush. For the objection which was valid as against the former employment of this method of calculation, is invalid against Professor Young's observation, in which matter was actually seen to be transported to the elevation just mentioned. On this supposition we should infer an initial velocity of 210 miles per second, and the general considerations advanced in my former paper would suffice to indicate a real initial velocity greatly in excess even of this enormous amount.

But, as a matter of fact, a careful analysis of Professor Young's observation shows that it supplies much more satisfactory evidence.

We can readily calculate the time which a projectile from the sun's surface ejected to an extreme height of 200,000 miles *in vacuo* would occupy in traversing the upper half of its upward course. Now the time required is found to be as nearly as possible twenty-six minutes. The corresponding space in the case of Professor Young's hydrogen wisps was traversed in ten minutes. Here is a discrepancy which must be explained in some way before we can safely proceed in our inquiry.

We may ask first under what circumstances a projectile from the sun, *in vacuo,* would traverse the second space of 100,000 miles of its upward course in ten minutes. It is found that, for this, the actual vertical range must be 360,000 miles; corresponding to an initial velocity of 260 miles per second. But is it conceivable that the actual upward range of the glowing hydrogen was so great as this, and that the wisps only appeared to be checked in their upward course at a height of 200,000 miles, because at this height they lost their great brightness, and so passed out of view? I cannot but think that Professor Young's narrative will bear no such interpretation. He says, indeed, that the hydrogen films faded gradually as they rose; but he actually measured their distance from the sun when they had reached their greatest elevation, so that they must still have been distinctly visible at that time. Had they continued to rise after that (at the enormous rate proper to them if they were making for an extreme elevation of 360,000 miles), so acute an observer as Professor Young could scarcely have failed to recognise the circumstance.

Only one explanation of the rapidity with which the observed range was traversed remains available. We must assume that the hydrogen wisps had originally had imparted to them a much greater velocity than would have sufficed to carry them to the observed height; but that the retardation caused by the solar atmosphere deprived the uprushing hydrogen of a large part of this velocity—the sun's attraction destroying *only the remainder and not the whole,* as the hypothesis of motion in a vacuum would require.

Now let the results of this supposition be carefully attended to, *this* being remembered, that they are not hypothetical inferences, but follow demonstrably when the retarding action of the solar atmosphere has once been admitted. Moreover, let it be remembered that the existence of this retarding action must be regarded as in the first place a certainty in itself, and in the second as observationally demonstrated by Professor Young.

We have seen that to pass from a height of 100,000 miles to a height of 200,000 miles in ten minutes, *in vacuo,* a projectile must travel so as to reach an extreme upward range of 360,000 miles; and the velocity with which it would commence this part of its range would correspond to an initial velocity of 260 miles per second at the sun's surface. But *such* a velocity, though adequate to carry the projectile from a height of 100,000 to a height of 200,000 miles in ten minutes *in vacuo,* must obviously be quite inadequate in a medium exercising so remarkable a retardative effect that the extreme upward range is reduced from 360,000 to 200,000 miles. In the case of motion in a vacuum, under the supposed circumstances, the velocity at the height of 200,000 miles would still be very great; but in the actual circumstances the velocity was reduced to nothing at this height, and must have been correspondingly less at all the intermediate levels in the range of from 100,000 to 200,000 miles. So that to traverse the observed range in the observed time a very much greater velocity was necessary at the height of 100,000 miles than that which would carry the projected matter *in vacuo* to a height of 360,000 miles. Let us take, as a very moderate estimate, a velocity, at the height of 100,000 miles, such as, if not affected by the retarding atmosphere, would carry the projected matter to a height of 500,000 miles.

But now let this be noticed:—The atmosphere *above* the height of 100,000 miles sufficed so to reduce the velocity of our projected matter that instead of travelling 400,000 miles beyond that level it travelled only 100,000 miles, losing thus three-fourths of its range above 100,000 miles. But the atmosphere must be very much denser—I had almost said *infinitely* denser—below the level of 100,000 miles than above that level; so that its retardative effect, while the projected matter was passing from the sun's surface to a height of 100,000 miles, must have been very much greater than that exerted during the remaining portion of the projectile's flight. Suppose—and again the supposition is most moderate—that the retardative action was but twice as great in the lower half of the actual range of 200,000 miles. Then, as a very rough method of calculation—a method falling far short of the truth—we may take the total vertical range of 500,000 miles (which would have resulted if no retardative action were exerted after the matter had passed the height of 100,000 miles) as *one-eighth* of the total range if there had been no retardation at all.[1] It follows from this that the actual range *in vacuo* would have been no less than four millions of miles, or much greater than the observed extension of the solar corona.

This, however, is but a rough and a wholly inadequate method of estimating the effects of retardation. If it be remembered that the retarding action of the solar atmosphere caused the projected matter to come to rest at a height of 200,000 miles (where, if unretarded by atmospheric resistance, it would still have had a velocity of more than 100 miles per second, we see that we must add a velocity of 200 or 300 miles per second to the initial velocity as estimated for a vacuum. Thus we arrive at some such velocity as five or six hundred miles per second, and a velocity of 380 miles per second is all that is needed to carry matter clean away from the sun to the domain of some other star.

It is abundantly clear, therefore, that in solar erup-

[1] The reader will see why this has been done. We are now conceiving twice as great a retarding effect to be exerted as in the upper part, where, as we have seen, the remaining range was reduced to *one-fourth* the range *in vacuo*.

tions matter is ejected with a velocity fully equal to the requirements of the eruption theory of the corona.

The only question, then, which remains is whether along with, or in front of, the uprushing hydrogen, some solid, liquid, or at least very dense matter, is expelled in these stupendous eruptions—such matter retaining a much larger proportion of its velocity, because much less effectually resisted by the solar atmosphere than hydrogen, the rarest of all known gases. On this point we have no direct evidence; but we have indirect evidence of such a character as to leave little room for any doubt. Much evidence of this sort has already been given in the earlier part of the present essay and in the preceding paper. There is yet other evidence; but this evidence is not such as can be suitably presented at the close of a paper already drawn beyond the limits proposed when it was commenced. For the present, then, I must remain content with having presented the evidence recently obtained respecting the nature of solar eruptions—as respects the regions where they chiefly occur, the features which distinguish the prominences to which they give birth, and the enormous velocities with which the erupted matter travels when close by the solar surface. Viewed in connection with the eruption theory of the corona, the facts here adduced amount very nearly to a demonstration; but apart from all reference to theoretical considerations they possess an exceeding interest, in so far as they bear on our ideas respecting the physical habitudes of the great luminary which rules the planetary system.

COLOURS OF THE DOUBLE STARS.

OLD ZAHN, in the strange work called the 'Syntagma,' says of the stars that they shine 'more like torches burning with eternal flame before the altar of the Most High, than the lamps of the ethereal vault, or the funeral lights of the setting sun.' And he proceeds to discuss the various colours seen among the stars, arguing that the stars show by their tint to which planetary party they belong. There are the partisans of Saturn, with a dull and leaden aspect; the Jovial stars brilliantly white; and the Martial party with fiery, ruddy rays. Those stars which have an orange-coloured light are the adherents, he thought, of our sun; while those which are pale and faint belong to the moon. Lastly, the stars which obey the planet of Love, shine with a box-coloured light!

One cannot wonder that even before its true significance was understood, a phenomenon so beautiful as the coloured splendours of the stars should have attracted attention. In our latitudes, indeed, the colours of the stars are not very striking, though even here they may be very easily recognised when the air is clear and dry. But in southern climes, and especially in the land where astronomy had its birth, the colours of the stars form a very beautiful feature of the nocturnal heavens. 'The whole sky,' remarks a modern traveller, 'seems set with thousands of varied gems.' Nay, even the shooting-star, as it flashes across the heavens, exhibits colours which are never seen in our latitudes. Sir Alexander Burnes remarks on the magnificent spectacle presented by the coloured shooting-stars seen from the elevated table-land of Bokhara, and Humboldt was deeply impressed by the same beautiful phenomenon.

The colours, then, which we notice in the stars are to be looked upon as giving but the faintest notion of the real splendour of the hues with which those distant suns are shining. If the mere change from our latitudes to tropical climes can add so much to the brilliancy of the stellar colours, how gorgeous would be the scene if we could behold the galaxy of suns from above the limits of our own obscuring atmosphere! We should see Arcturus and Aldebaran, Pollux, Antares, and Betelgeux, blazing like sun-lit rubies among their fainter neighbours; the glorious yellow of Capella and Procyon would surpass the most splendid golden or topaz colours known to our artists; while the brilliant white hues of Vega and Altair and the blazing Sirius would be no less beautiful and striking.

But even such a scene as this, wonderful as it would appear, would be as nothing when compared with the splendours which would come into view if the powers of the observer's vision could be gradually increased until the stars, which are now only detected by the piercing eye of the telescope, were seen in all the richness and variety of their colours. It is among the stars which are invisible to the unaided eye that the real splendours of celestial colouring are to be found. No words can adequately describe the beauty of the scene which our observer would behold; but if he sought to convey some imperfect notion of the glories revealed to him, he could find perhaps no apter account than the well-known lines of Thomson:—

> First the flaming red
> Sprang vivid forth; the tawny orange next,
> And next delicious yellow; by whose side
> Fell the kind beams of all-refreshing green.
> Then the pure blue that swells autumnal skies,
> Ethereal played; and then, of sadder hue
> Emerged the deeper indigo (as when
> The heavy-skirted evening droops with frost),
> While the last gleamings of refracted light
> Died in the fainting violet away.

In this order would the colours of the stars come into view. We see in the nocturnal skies no traces of those green and violet and blue and purple suns which are really pouring forth their richly-tinted rays on other worlds and other scenes. Only the ruddier tints of the prismatic colour-scale are visible to the unaided eye, and even these not with that fulness or depth of tone which may be recognised in the telescopic stars.

But even among the stars which the telescope reveals to us, the full range of colour is only to be seen among the members of a peculiar order. There is a little difference among astronomers on this point; but most of them agree that no isolated stars of a blue, or green, or purple colour can be seen even with powerful telescopes. So commonly has this been asserted, that the late Admiral Smyth, who thought he could recognise very decided blue tints among the minuter stars, expressed a doubt whether this might not be due to some idiosyncrasy of his eyesight. And certainly there is no instance, among the thousands on thousands of stars whose places have been recorded, of one isolated star of a well-marked blue colour.

But when we turn to those interesting objects, the double stars, the scene is wholly changed. Every variety of colour is seen among these singular systems. We not only find all the tints of the rainbow, but a number of other colours, such as fawn, buff, ash-colour, silvery white, coppery, and grey. The range of colour seems, in fact, wholly unlimited; and astronomers need the aid of a practical artist before they can even tabulate the long list of colours which the double stars exhibit to them.

There are few subjects which seem better calculated to attract and interest even the least thoughtful than the presence of these singularly beautiful colours among the orbs of heaven. Regarding the fixed stars as suns, the centres of schemes of dependent worlds resembling in many respects the worlds which circle around our sun, we cannot but look with wonder upon the strange scene which must be presented amid those distant systems. It would be interesting enough to consider merely the case of a number of worlds circling around a red, or orange, or yellow sun. But when we imagine the condition of those worlds which travel round a pair of differently-coloured suns, we are lost amid the perplexing considerations which suggest themselves. 'Imagination fails to conceive,' says Sir John Herschel, 'the charming contrasts and grateful vicissitudes of a red and green day, alternating with white light or with darkness, in the planetary systems belonging to these suns.'

Perhaps, however, we do not see in this description the true result of the presence of two suns as the companion rulers of a planetary scheme. Until we know something of the distance at which the members of such a system circle round their double primary, we can hardly assert with confidence that those planets have days of different colours. It may well be that they are so far from both the orbs which sway their motions that their two suns are always seen close together, as they appear to us, who are so much farther off.

But even when we take this view, we are struck with the thought of the strange scene which the sky of one of those distant planets must present. Conceive two coloured suns above our horizon. Now one, now the other, is the leading light of the firmament. Their distance from each other is constantly varying as the planet circles round them. Often one must pass before the other, and then the colour of the day changes, passing through many gradations, as the strange transit of sun over sun is in progress. Then every object on such a planet must cast two different shadows. If the suns are red and green, for instance, the shadows are green and red. When we remember how large a part shadows play in the appearance of a landscape, we see at once how strange a scene the hills and dales and valleys and woods in those distant worlds must present to those who inhabit them. Living creatures must exhibit a yet stranger aspect.

But our object is not to deal with fanciful speculations such as these. There is a real physical meaning in the colours of the double stars which is well worth searching out.

Let us first notice certain facts about the colours of the double stars which are at once interesting and instructive.

In the first place, it has long been noticed that among many double stars complementary colours may be recognised. Red and green companions are commonly met with; in some instances the beautiful contrast between yellow and purple is exhibited; while not unfrequently blue and orange stars are seen in company.

It was suggested that this peculiarity might in reality be optical rather than real. It is well known that where the brighter of two neighbouring objects presents a well-marked colour, the fainter very commonly presents the complementary colour, though not in reality tinted with that hue. Artists are familiar with this peculiarity, insomuch that some of the most striking effects of colour in well-known paintings, have been produced, not by a real intensity in the colours made use of, but by the judicious contrast of suitable complementary colours. Many of our readers have doubtless heard the story of the French painter who tried in vain to obtain a certain brilliant yellow tint which he was desirous of introducing into a picture, and was about to set out for the Louvre, to see how other painters had mastered the difficulty, when a passing cabriolet, the yellow wheels of which were picked out with purple, showed him how he could give brilliancy to the yellows he had been so little satisfied with. Thus astronomers thought the green companions of brilliant red stars, or the blue companions of brilliant orange stars, might in reality simply be white stars whose purity of tint was overmastered by the effect of contrast.

But this idea had to be abandoned. It was found possible in several instances to hide the brighter of the two stars from view while the smaller still continued visible. When this was done there remained, of course, no effect of contrast. Yet in nearly every instance the colour of the smaller star continued as well marked—though not perhaps as pleasing—as when both stars were visible together. Usually this plan of hiding one star while the other continued visible was effected by artificial means, a small cross-bar of brass or copper being introduced into the telescopic eye-piece for the purpose. But there is one instance in which the moon was made to aid the astronomer; and the story seems to us so interesting that we venture to give it in full:—

The star Antares, or the Scorpion's Heart, had long been a source of perplexity to astronomers. It is a brilliantly-red star, and has indeed been called the Sirius of red stars. But when the star is watched intently, especially with an instrument of adequate power, a singular scintillation of green light is found to obtrude itself most persistently into notice. It was suspected, at length, that this star must have a green companion; but for a long time none could be found. At length the late General Mitchell, with the fine telescope of the Cincinnati Observatory, detected a companion to the brilliant Antares; and as had been suspected, this companion proved to be green. This, the first noteworthy achievement of the Cincinnati telescope, was a source of considerable gratification to Mitchell, until he heard that at another observatory two green companions could be seen. He searched again and again for the second green star, but could find no trace of it; and at last the welcome news came that the telescope of the other observatory was in fault. It possessed the undesirable faculty of dividing small stars on its own account—that is to say, it divided stars which really were single. Reassured of the fidelity of his telescope, General Mitchell re-examined the star. But he, and others who joined in the work, found it difficult to satisfy themselves as to the real greenness of the companion. The latter also was too minute an object, and too close to its primary, to be separated by the artificial device mentioned above. It happens, however, that Antares is one of those stars which the moon occasionally passes over as she travels along the zodiac; and the late Mr. Dawes, perhaps the most sharp-sighted observer that ever used a telescope, availed himself of one of these passages to settle the question of the tiny star's colour. When the moon had hidden Antares, the small companion was shining alone for a few seconds, in the telescope's field of view. Its colour was then seen to be unmistakably green.

Another peculiarity of the coloured stars is even more surprising. Some of them appear to possess the extraordinary power of changing colour. They are, as it were, chameleon stars. Startling as this circumstance appears, yet the evidence on which it rests is too strong to be resisted.

We may remark, in the first place, that even among the brighter stars a similar peculiarity appears to exist. Sirius, which outshines nearly fourfold all the other stars visible in our northern skies, is now brilliantly white. Yet the ancients recognised Sirius as a red star. Both Ptolemy and Seneca expressly mention his ruddiness of hue; indeed, it is doubtless to this tint that the star owed its bad reputation among the ancients. Another star, called by astronomers Gamma Leonis, was white in Sir William Herschel's time, but is now golden yellow; and it happens that we are more certain than we could otherwise be about the reality of this change because Sir William Herschel was rather apt to over-estimate the yellowness or ruddiness of stars, so that a star described by him as pure white might be suspected of having been even somewhat bluish.

But some of the changes among the double stars are more striking even than these. I shall confine myself to one very noteworthy instance:—

In the year 1856, Admiral Smyth, who took particular interest in the question of star-colours, called the attention of his son, the present Astronomer-Royal for Scotland, to the good results which might be secured if the latter observer examined the colours of the stars from the summit of Teneriffe, whither he then proposed to betake himself to carry out his now celebrated 'astronomical experiments.' An observatory was not set up on the summit of Teneriffe, owing to insuperable difficulties, but the Pattinson telescope, of $7\frac{1}{4}$ inches aperture, was hauled up to the Alta Vista, and there mounted at an elevation of 11,000 feet above the sea-level. In pursuance of his father's wishes, Prof. C. Piazzi Smyth observed carefully the colours of several well-known doubles. Often he had the assistance of visitors in this work; and among the stars which he examined in company with others was one known to astronomers as 95 Herculis. He entered the colours of this double in his note-book as 'both yellow, with tinge of bluish green.' Admiral Smyth adds that 'the tints of the two stars—though not quite the same at each examination—were judged to be common to both, and the impression was ratified by the evidence of some Spanish visitor at the astronomical eerie.'

Now this particular double had been very carefully studied by the elder Smyth, and he had described the companion stars as 'apple green' and 'cherry red.' He therefore re-examined the double, and *there* were the colours apple green and cherry red as before. He next applied to other well-known observers of double stars. Mr. Dawes wrote to him as follows—'On referring to my colour-estimations I find that they agree very nearly with your own.' Lord Wrottesley pronounced the brighter star to be greenish, the other reddish. Mr. Fletcher said that 'to his eye the brighter appeared light green, the other pink.' Mr. H. A. Fletcher thought the stars bluish green and orange. Mr. Carr thought them light green and dull red.

These results, it will be seen, agree closely enough together, but are altogether opposed to the Teneriffe observations. And to add to the difficulty, it was found that in 1844 the Italian astronomer, Sestini, had seen both the stars golden yellow, while his colleague De Vico, 'in the same place and with the same instrument dubs them "rossa e verdi."'

'Assuredly,' as Admiral Smyth remarks, 'all this is passing strange.' It was quite impossible to refer the difference to peculiarities in the visual powers of the observers, because no known instances of colour-blindness correspond to this particular case. And besides, the list sent by Piazzi Smyth to his father showed in all other respects the most satisfactory accordance with the observations made by the latter. 'A general agreement existed throughout the list,' remarks Admiral Smyth, 'even in some of the most delicate hues.'

It may, however, be interesting to inquire what effect can be ascribed to different qualities of eyesight in this peculiarly delicate work of estimating star-colours. Admiral Smyth fortunately was at the pains to try the experiment, and the result will be found at once amusing and instructive:—

He placed a fine Gregorian telescope of $5\frac{1}{4}$ inches aperture[1] in front of the south portico of his house, and invited a party of six ladies and five gentlemen to gaze upon the fine double star Cor Caroli. 'They were each to tell me,' he says, 'but *sotto voce*, to prevent bias, what they deemed the respective colours of the components to be.' First to step forth was the late Rev. Mr. Pawsey, 'more addicted,' says the Admiral, 'to heraldry than to astronomy.' 'After a momentary watch, he flatly declared that "he could make out nothing particular."' The other spectators were more patient; and their respective impressions are thus noted down in the large album of the Hartwell Observatory. It is to be noticed that A. means the chief star of the pair, B. the companion:—

[1] It is worth noticing that small reflectors are the best telescopes for showing star-colours.

Mrs. Tyndale . .	A. Pale white. B. Violet tint.
Mrs. Rush . . .	A. Yellowish cast. B. Deadish purple.
Miss Honour . .	A. Yellowish. B. Lilac.
Miss Charlotte . .	A. Light dingy yellow. B. Lilac.
Miss Emily . . .	A. White. B. Plum colour.
Miss Mary Anne . .	A. Palish yellow. B. Blue.
Mr. Rose . . .	A. Cream colour. B. Violet cream (!).
Mr. B. Smith . .	A. Pale blue. B. Darker blue.
Dr. Lee . . .	A. Whitish. B. Light purple.
Captain Smyth . .	A. White. B. Plum-colour purple.

One would hardly imagine that so great a difference would be found in the estimates made by different persons of the colours of the same pair of stars. As Admiral Smyth remarks, 'Whatever may be said about instrumental means, weather influence, atmosphere, or the position of the object, it is clear that in this instance all these properties were common to the whole party, and we doubtless all meant the same hues. It must be admitted, however, that the star was new to most of the spectators; and although,' adds the gallant seaman, 'some of the eyes were surpassingly bright, they had never been drilled among the celestials.'

The experiment is one which might be repeated with advantage. The regular observer of the stars is not apt to look with particular complacency on the advent of visitors, but the most cross-grained of star-gazers might sometimes usefully apply the sight-seeing energies of his visitors in the way suggested.

It will be noticed that there is nothing in the above list of colour-estimates to explain the discrepancies in the case of the star 95 Herculis. All the observers recognised a difference of tint between the two stars, and only one, Mr. B. Smith, failed to recognise the difference in the colours. It may be accepted, therefore, as certain that the components of this remarkable pair change in colour to a very noteworthy extent.

Among the various explanations which were put forward to account for the enormous variety observed in the colours of the double stars, and also for the fact that these objects sometimes seem to change in colour, there is one which, though incorrect, is too interesting to pass unnoticed.

The reader is aware that light is merely a form of motion; that it travels in a series of undulations, not by a transmission of material particles; and that the colour of the light depends on the length of these undulations. In ordinary cases light-waves of many different lengths travel together, just as we often see the face of the ocean traversed, not by a series of uniform waves, but by a number of waves of many different sizes.

Now the idea occurred to the French astronomer Doppler, that if we are rapidly approaching a star or receding from it, either by our own motion or the star's, its colour ought to be changed. To a swimmer swiftly crossing a wave-tossed sea, the waves will clearly seem narrower or broader according as he swims against or with them; for in the first case he will pass them more rapidly. So, too, of the waves which produce sound. It has been shown that if an instrument which is giving forth a particular note is moved rapidly towards or from the hearer, the tone of the note perceptibly varies. When the instrument is approaching the hearer, all the sound-waves are apparently shortened so that the tone appears more acute; and when the source of sound is moving away, the tone appears more grave. Professor Tyndall remarks that when the whistle of the steam-engine is sounded as an express train rushes rapidly through a station, persons on the platform can detect a well-marked lowering in the tone of the whistle as the train, after rapidly nearing them, as rapidly passes away.

If we apply this principle to the case of light, we see that there might conceivably be a star which, while seemingly blue, or red, or green, was in reality sending forth light of another colour. If a star were emitting those light-waves, for example, which produce a red colour, and we were very rapidly approaching the star, the light-waves might be apparently so much shortened that they would produce the effect of blue light: in other words, the star would seem to be blue, though in reality it would be red. And so a blue star rapidly receding from us might appear red. And if a green star were sweeping rapidly round and round in a long oval path, first coming swiftly towards us and then moving as swiftly from us, it might change in colour, apparently, through all the hues of the rainbow.

This was a very ingenious theory, but, unfortunately, like many other very ingenious theories, it was surrounded with great difficulties.

In the first place, it seemed inconceivable that any of the stars could be moving with the enormous velocity which the theory required. It must be remembered that to produce any apparent change of colour a velocity was required which should bear an appreciable proportion to the velocity with which light travels. To return to the case of our swimmer: unless he were urging his way through the water with considerable speed, he would not seem to cross the waves much more rapidly when he was facing them than when he was swimming with them. Nor, again, in the case of sound, can we notice any appreciable change of tone unless the motion with which the source of sound is approaching or receding is very great—in fact, unless it bears an appreciable proportion to the velocity with which sound travels. But the velocity of sound may be looked upon as absolute rest compared with the tremendous velocity of light. We know that when a cannon is fired at some distance from us, an appreciable interval elapses after our seeing the flash before the sound is heard. But light travels so swiftly, that while the sound of Big Ben is travelling from Westminster to Constitution Hill, light would travel a distance exceeding that which separates us from the moon. Eight times would light circle this earth on which we live in the course of a single second. It was reasonably doubted, therefore, whether the stars can be assumed to travel with a velocity which can be compared with the inconceivable velocity of light.

But this was not all. It was pointed out that even if the double stars were circling around each other with a velocity so enormous as M. Doppler's theory required, yet there would be no apparent change in their colour. We have been supposing that the light-waves proceeding from a star were all of one definite length. But this is not the case. The light of a star, like the light of our sun, is composed of waves corresponding to many different colours. This is as true of the coloured stars as of the white ones. Their light, when subjected to prismatic analysis, is changed into the rainbow-tinted streak which is called the prismatic spectrum.

Now this changes the nature of the case altogether. So long as it was supposed that only light-waves of a certain length came from a star, then we might fairly compare those waves to a series of rollers crossing a sea over which a stout swimmer was urging his way, or to the uniform sound-waves which proceed from a railway-whistle. But now we must alter the analogy altogether. We must suppose our swimmer to be in the midst of a sea across which waves of many different forms are travelling. We must imagine that the approaching or receding railway-whistle gives out, not one sound, but a number of different tones. We see that this alters the result altogether. Our swimmer would no longer be able to recognise the effects of his own motion; nor would the nicest ear be able to appreciate the change produced in the tones which reached it. And in the case of the star we see that, while there could be a change, it would be one far more difficult to detect—even if nothing more remained to be said—than the change we considered before. As a matter of fact, however, it would be absolutely impossible to detect it, for a reason which remains to be noticed.

At each end of the rainbow-tinted streak called the spectrum there are waves which produce in us no sense of light. Beyond the red end there are waves longer than those which produce red light, and these waves, while they produce the sensation we call heat, exert no effect on the visual organs. Beyond the blue end of the spectrum there are waves shorter than those which produce violet light, and these, though they produce certain chemical effects, are also not recognisable by the eye. Now, if all the light-waves were lengthened through the rapid recession of a star, some of the waves at the red end of the spectrum would be rendered invisible, being changed into heat rays. Beyond the blue end of the spectrum a number of chemical rays would be lengthened, and become visible as violet light. We see, then, that the rainbow-streak would remain absolutely unaltered. It would begin with the deepest visible red, and would pass through all the seven gradations of colour down to the deepest visible violet just as it did before. And clearly the rapid approach of a star would be similarly ineffective in changing its apparent colour.

Doppler's theory, therefore, though it had had a singular fascination for many thoughtful minds, had to be given up.

But the time was approaching when the powers of the most searching instrument which the astronomer has yet been able to devise were to be directed to the solution of this difficulty. It had long ago occurred to Sir David Brewster that if the light of the coloured stars could be analysed by means of the spectroscope, something might be learned respecting the cause of those beautiful and varied tints which they exhibit to the telescopist. This, be it remembered, was before the invention of what is now termed spectroscopic analysis. He could not have argued more justly than he did, however, had he known all that Kirchhoff afterwards discovered. 'There can be no doubt,' he remarks, 'that the spectrum of every coloured star wants certain of the rays which exist in the solar spectrum.' Nay, he made an observation with a rock-salt prism, which may be looked upon as absolutely the first application of the spectroscopic analysis to the stars. He says, 'In the orange-coloured star of the double ζ Herculis I have observed that there are several defective bands. By applying a fine rock-salt prism to this orange star, as seen in Sir James South's great achromatic refractor, its spectrum clearly showed that there was one defective band in the red space, and two more in the blue space. Hence the colour of the star is orange, because there is greater defect of blue than of red rays.'

Here is, in fact, the optical explanation of the whole matter. Subsequent observations, by the experienced spectroscopists who now apply the power of the new analysis to the stars, have confirmed Sir David Brewster's observations in the fullest possible manner; and so far as the mere optical peculiarity is concerned, nothing further remains to be said. But it must be remembered that the new analysis deals with more than mere optical peculiarities. It is its distinguishing characteristic that it gives a physical interpretation of those peculiarities. A certain dark line, or group of lines, is seen across the rainbow-tinted spectrum, and the physicist at once announces that the vapour of a certain element surrounds the body whose light he is analysing. A certain set of bright lines appears as the spectrum itself of a given source of light, and he pronounces with equal confidence that that source is a certain incandescent vapour.

Now, how does the spectroscopist interpret the fact which Sir David Brewster discovered?

It is, of course, not always possible to say of a set of bands crossing the spectrum of the light from a star that it is due to the presence of this or that element, because as yet spectroscopic analysis is in its infancy, and we do not know the spectra of many of the elements so exactly as we hope to do; but the physicist knows very certainly that the presence of a set of bands indicates the existence of some absorbing vapours around the source of light. And this is precisely what Sir David Brewster did not know. In fact, Admiral Smyth, after quoting Sir David Brewster's observations, added, 'We have no reason to believe that these defective rays are absorbed by any atmosphere through which they pass.' At present we have not only reason to believe this, but we feel absolutely certain about it.

What we know, then, about the colours of the double stars is this, that they are due to the existence of certain vapours around the stars. Why the two stars should be in many cases differently constituted, so that around one a set of vapours should be suspended different from those around the other, we do not know. But we can readily understand that such differences should exist. Again, we cannot tell at present why these vapours should sometimes subside, as they must do when a star changes colour. But this also is not difficult to understand, since we know that even our own terrestrial atmosphere is more heavily loaded sometimes with aqueous vapour than it is at others.

What we do know is, however, sufficiently interesting, without hazarding speculations about that which is unknown. We see that those beautiful objects which have been so long the delight of our telescopists can teach us much respecting the constitution of the universe. Out yonder, amid the unfathomable depths which the telescope only can explore, vapours are forming and dissipating according to laws not dissimilar to those which regulate the vapours of our own atmosphere. There is no quiescence in those far-off regions any more than in our own neighbourhood. Ceaseless change and endless variety characterise no less the universe of stars than the terrestrial scene with which we are so familiar.

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