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VI. *On the Cause of the Solar-Diurnal Variations of Terrestrial Magnetism.* By Professor BALFOUR STEWART, LL.D., F.R.S.*

IN attempting to account for the Solar-diurnal inequalities of Terrestrial Magnetism, scientific speculators have ranged over the whole field of likely explanations. Some of them think that they have now finally driven the truth into a corner, and are in hopes of ultimately securing the object of their pursuit.

I shall begin by enumerating the chief hypotheses which have been proposed:

(1) There is, first, that of the direct magnetic action of the Sun upon the Earth. There are, however, many serious objections to this hypothesis. One of these is the great difficulty attending the supposition that the Sun is capable of acting as a powerful magnet, since terrestrial analogies would lead us to infer that matter at the high temperature of the Sun is quite incapable of possessing magnetic properties. Again, we have strong evidence that changes in the range of the daily variation of magnetic declination lag behind corresponding solar changes in point of time. This kind of behaviour is apparently inconsistent with direct magnetic action, and points rather to some indirect influence probably caused by the radiant energy of the Sun, inasmuch as the changes and turning-points of such indirect influences due to radiation are well known to lag in respect of time behind the corresponding changes and turning-points in their cause. Finally, Dr. Lloyd and Mr. C. Chambers † have both shown that direct solar magnetic action will not account for the peculiarities of the diurnal magnetic variation. We may therefore perhaps dismiss this hypothesis.

(2) The next hypothesis which I shall introduce is that of Faraday. Reasoning from his discovery that oxygen is magnetic and becomes weaker in its power when heated, he concludes that when the Sun heats a certain portion of the Earth's atmosphere, this renders that portion less magnetic

* Read April 10, 1886.

† Proc. Royal Irish Academy, Feb. 22, 1858, and Phil. Trans. 1863.

than those other portions not subjected to solar influence. This kind of action is thus seen to be equivalent to a displacement by means of solar influence of the magnetic matter of the Earth, and hence implies a corresponding displacement of the lines of magnetic force. Now there is unquestionably truth in this hypothesis, but yet there is very great doubt whether it will account for the diurnal oscillation as we know it. It will form a *cause*, but yet it will not be *the chief cause*. It is obviously one prominent peculiarity of Faraday's hypothesis, that in it the action of the Sun must, to ensure its probability, be associated with the great mass of the Earth's atmosphere, that is to say with the lower strata as well as with the upper ; or rather with the lower strata in preference to the upper, bearing in mind the superior mass of the former and their greater nearness to the magnetic Earth. Now we know both from observations of the declination and horizontal force (Proc. Roy. Soc. March 22, 1877, and Phil. Trans. 1880, p. 541) that the action of the Sun in producing diurnal variations of these elements is one and a half times as powerful at epochs of maximum as it is at epochs of minimum sun-spot frequency.

But it is very difficult, if not impossible, to imagine that the great mass of the atmosphere can be heated by the Sun one and a half times as much at epochs of maximum sun-spot frequency as it is at epochs of minimum sun-spot frequency; and yet we must imagine this if we suppose that Faraday's hypothesis accounts for the diurnal variation. Certainly the facts of meteorology are strongly against any such belief. Again, during our summer the heating influence of the Sun is considerably greater in the Northern than in the Southern Hemisphere, while during our winter the reverse will be the case. Totally apart from the diurnal variation we ought therefore, if Faraday's hypothesis be true, to find a very perceptible difference in the mean magnetic condition of the Northern Hemisphere between summer and winter, and a like difference too in the Southern Hemisphere, the one being at the same moment differently affected from the other. But there are no traces of such a phenomenon, the annual and semi-annual variations being quite of a different nature and none of them very large.

(3) Let me now discuss the hypothesis that these variations are caused by earth-currents. These currents were first detected by W. H. Barlow, and were at a later period observed and studied by C. V. Walker. They are now continuously recorded by photography at the Royal Observatory, Greenwich, and at the Central Russian Observatory.

With respect to these currents, I have always maintained that in type they bear all the marks of being secondary currents generated in the moist conducting crust of the Earth, and caused by small but abrupt changes in the magnetism of the Earth. Whether this be the true explanation or not, it is at any rate, I venture to think, consistent with what we know of electrical action. But while I should thus feel disposed to regard these phenomena as effects rather than causes, in their primary sense at least, it is manifest that, when once generated, such currents will influence the needle; and the results of the Greenwich observers induce us to think that such influence is very great and forms perhaps the chief immediate cause of those oscillations of the needle called disturbances, which are quite different from the solar diurnal variation. In one sense therefore such currents may be effects, while in another they are causes, but causes of disturbances rather than of the daily variation.

It may, however, be replied that earth-currents may be complex phenomena, and that while their most important manifestations may be due to induction, thus being primarily effects rather than causes, yet another portion of them may operate as a true cause producing the solar-diurnal variation. Now there are two objections to this hypothesis; and one of these is, that we know of no physical agency that will produce in the Earth's crust currents of this nature, which, in order to account for the solar-diurnal variation, must be one and a half times more powerful at times of maximum than at times of minimum sun-spot frequency. Another objection is that Sir George Airy has not been able to detect any resemblance in form between the regular diurnal progress of the magnet and that of the earth-currents. We think therefore that we may dismiss this hypothesis.

Now, if it be unlikely that these magnetic variations are caused either by the direct magnetic action of the Sun,

or by earth-currents, or by the heating effect of the Sun on the chief mass of the Earth's atmosphere, we seem to be driven by the method of exhaustion to look for their cause in the upper atmospheric regions. We shall, however, have to show that there is no improbability in locating their cause in these elevated regions, otherwise our method of exhaustion will have done us no service. In the first place, I need hardly say that if the cause we are in search of be in these upper regions, it must either be in the shape of a set of electrical currents, or in some other shape with which we are quite unacquainted; but the nature of this discussion precludes us from entertaining the latter supposition; and we are therefore driven to regard electrical currents as being the only conceivable cause, if this cause is to be located in the upper atmospheric regions.

I shall now attempt to reply to two imaginary objections that may be raised as to the possibility of such currents. In the first place, it may be said that while undoubtedly rarified air is a conductor of electricity, yet it is not a good conductor; and where can we look for sufficient potential to drive currents through these upper atmospheric regions? To this I would reply that *as a matter of fact* we know that there are visible electric currents in the upper atmospheric regions which occur occasionally at ordinary latitudes, and which are very frequent, if not continuous, in certain regions of the Earth. I allude to the Aurora, which is unquestionably an electric current, and must therefore influence the magnetic needle. Furthermore, both with respect to its times of occurrence and to the disposition of its beams, the Aurora manifests a close connection with the phenomena of terrestrial magnetism, occurring at ordinary latitudes only when there are great magnetic disturbances, and the disposition of its beams having a distinct reference to lines of magnetic force. We are therefore justified in asserting that there is no impossibility in conceiving a set of electrical currents, intimately associated with certain phenomena of terrestrial magnetism, to exist in the upper region of the Earth's atmosphere.

In the next place, the objection may be raised, How is it possible to suppose any cause that will make such currents

one and a half times more powerful at times of maximum than at times of minimum sun-spot frequency? Can it be imagined that the radiant energy of our luminary varies in this proportion? Now it has been remarked by Professor Stokes that an increase in the radiative power of the Sun would most probably imply, not only an increase in general radiation, but a special and predominant increase in such actinic rays as are probably absorbed in the upper regions of the Earth's atmosphere. These regions will therefore greedily absorb the new rays, their temperature will rise, and, as is known to be the case for gases, the electrical conductivity of the strata will be increased*. Thus, even if we imagine the general atmospheric current to remain constant, a greater proportion of it would be thrown at such times into those heated regions which had become good conductors; but then it is likewise probable that the current itself would be increased as well as increasingly diverted into a particular channel.

Such an explanation appears to receive support from the curious and suggestive fact that the lunar influence on the Earth's magnetism, as observed at Trevandrum, is greater during the day than during the night, and greater perhaps, too, at times of maximum than at times of minimum sun-spot frequency. If the arguments now advanced be generally assented to, we may presume that, while it is unlikely that the cause of the daily variation exists anywhere else than in the upper regions of the Earth's atmosphere, there is no objection to our imagining that it resides there in the form of a set of electric currents. Such currents will of course affect the needle directly, but they will also affect the magnetic earth, and any affection of the magnetic earth will again influence the needle. The question thus arises, To what extent do these hypothetical currents act directly upon the needle, and to what extent do they act upon it indirectly by influencing the magnetism of the Earth? That this question is not unimportant will be manifest by considering what takes place

* It seems not impossible, too, that the constitution of these strata with respect to aqueous vapour, may be different at times of maximum and at times of minimum sun-spot frequency; hence their absorbing power may be different.

when a powerful electromagnet influences a needle. In this case probably only a small portion of the influence is due to the direct action of the current, while a large portion is due to the iron of the electromagnet that has been magnetized by the current. In order to study this point it will perhaps be desirable that we limit ourselves to the solar diurnal variations of declination, as these are of a simple definite type in each hemisphere, the type in the one hemisphere being the reverse of that in the other. Let us therefore consider the peculiarities of this variation, particularly in the regions on either side of the magnetic equator.

Here we find, speaking generally, that north of the equator the extreme westerly deflection of the marked pole of the needle is reached at 2 P.M., while south of the equator this pole reaches its extreme easterly deflection at the same hour. Now, if we attempt to picture to ourselves any general change in the magnetic system of the Earth that will produce these results, it will be one of a very artificial character. On the other hand, these motions of the needle, which cannot well be explained by any easily conceivable temporary change of the Earth's magnetic system, can very easily be explained by a system of atmospheric currents. In the northern hemisphere we should have a set of positive currents flowing from the equator northwards, while in the southern we should have in like manner a similar set of positive currents flowing southwards. And, therefore, since positive currents flow out from the equator on both sides of it, we should have to contemplate a vertical stream of positive electricity at the equator which there splits itself up into two portions—one for each hemisphere.

The argument as I have now advanced it is of a general nature and is twofold.

(1) In the first place it brings forward objections to the localization of the cause of the diurnal variations anywhere else than in the higher regions of the Earth's atmosphere; then points out that if this cause exist in these regions it must be in the shape of a system of electric currents; and finally endeavours to show that there are no valid objections to such a system of currents.

(2) It endeavours to show that, if we take the diurnal

variation of the declination-needle, which is of a remarkably simple type, we have great difficulty in imagining that it can be brought about by any temporarily induced displacement of the magnetic system of the earth, while we can with the greatest ease account for it by means of a system of currents in the upper regions of the Earth's atmosphere.

At this point Dr. Schuster has endeavoured to apply mathematical analysis to the subject, and he has obtained results which are very satisfactory and encouraging *. He had previously, in the Report of the Magnetic Committee of the British Association for the year 1885, pointed out the existence of a test † which will enable us to decide whether or not the diurnal variation can be caused by a series of electric currents which embrace both the upper atmospheric regions and the Earth, and complete themselves by electric discharges through the lower regions of the Earth's atmosphere into the ground ; and he now endeavoured to ascertain whether the supposed vertical currents at the equator, to which allusion has been made, could possibly be of this nature. If this were the case, he argues, and if a unit pole were carried from a position A at the Earth's surface in a path which should return again to A, and in the centre of which there should exist such a vertical current, then through all its course work would be done either by or against this pole, inasmuch as there would be a continued effort of the vertical current to turn the pole round it in one direction. In other words, the pole and the point A being immersed in the system of currents, there would be a potential having multiple values. On the other hand, if the system which produces the daily variation be outside of A, whether above or below it, there will be at A a true potential, and if the unit pole be carried as before, in any path from A and back to A again, as much work will be done against the pole as will be done by it. Let us consider, now, a little path embraced between two parallels of latitude near each other, and two meridians of longitude likewise near each other, and imagine our pole to be carried round this boundary. On the

* Proc. Lit. & Phil. Society of Manchester, January 12, 1886 ; Phil. Mag. April 1886, p. 349.

† The reader will find a complete discussion of this subject in Maxwell's 'Electricity and Magnetism.'

supposition that there is no vertical current in the centre of the path, the work done upon the pole will be equal to that done by it. This will at once give us an equation between X and Y ; that is to say, between the components of the solar-diurnal periodic force towards the geographical north and west respectively. From this equation, and by means of certain legitimate assumptions regarding the type of Y, which represents very nearly the force influencing the declination (and this we know to be of a simple uniform type), Dr. Schuster is able to obtain certain relations between the solar-diurnal variations of the declination and of the horizontal force which ought to hold, *if there be no vertical currents between the upper regions and the Earth*, or, rather, if such vertical currents are inoperative as an element in producing the solar-diurnal variation. One of these is that the northerly or horizontal-force component of the daily variation ought to have a maximum or a minimum at the time when the westerly or declination component vanishes, that is to say attains its mean position.

Now at Greenwich, X or the horizontal-force component has a maximum at 7 o'clock in the evening and a minimum at noon ; while Y or the declination component vanishes a little after 7 o'clock and between 12 and 1 in the afternoon. At Bombay the declination-needle is at its mean value on an average a little after 10 in the morning and about 10 in the evening. The horizontal force has its maximum a little after 11 in the morning and the minimum at a quarter past 9 in the evening.

The observations at Lisbon and Hobarton show an equally good agreement, those at St. Helena and the Cape of Good Hope less so ; but in these two latter places the observations taken at different months show a considerable difference of behaviour. On the whole, Dr. Schuster regards the agreement as satisfactory, and as so far tending to disprove the existence of vertical currents through the Earth's surface. Pursuing his analysis, he comes to the conclusion, from certain approximate expressions which he obtains, that the northerly force ought to be a maximum in the morning and a minimum in the afternoon in the equatorial regions ; while in latitudes above 45° the minimum ought to take place in the morning.

Now this is almost exactly what happens. At Bombay, for instance, the *maximum* of horizontal force takes place at 11 A.M., while at Greenwich the *minimum* takes place a little after that time.

Having thus obtained results which lead him to infer that the unit pole we are observing is not immersed in the attracting system, Dr. Schuster next proceeds to discuss the question whether this system is above us or beneath us. To do this it is necessary first of all to obtain an approximate value for the potential at the surface of the earth, and from it to derive the general potential. To aid in determining this we must bear in mind that, since the unit pole is now assumed not to be immersed in the attracting system, the potential V will be subject to the condition

$$\frac{d^2V}{dx^2} + \frac{d^2V}{dy^2} + \frac{d^2V}{dz^2} = 0$$

or to an equivalent condition adapted to the system of coordinates employed. From this Dr. Schuster deduces two possible expressions for the potential ; the one referring to a system above our heads and having powers of (r) the radius in its numerator, the other referring to a system beneath our feet and having powers of the radius in its denominator. Now the expression for the vertical force as derived from the potential will be $-\frac{dV}{dr}$, the sign of which will be manifestly different according as r appears in the numerator or in the denominator of the potential in question. Which, therefore, of these two possible (approximate) forms of V agrees best with observation? In the first place, both expressions found by Dr. Schuster have their turning-points coincident in epoch with those for X , or the northerly component of the disturbing force ; and this is a fact confirmed by actual observation. But while the expression for the vertical force derived from a system above us has its maxima and minima coincident with those of horizontal force at latitudes greater than 45° , in the equatorial regions the maximum of horizontal force ought to be coincident with the minimum of vertical force, and *vice versa*. Precisely the opposite arrangement should hold if the attracting system be beneath our feet.

Now at Greenwich the maximum of northerly force takes place at 7 P.M. and the minimum at noon; while the maximum of vertical force takes place at 7 P.M. and the minimum at 11 A.M.

At Bombay the maximum of northerly force takes place at 11 A.M. and the minimum at 9 P.M.; there is a very decided minimum of vertical force at 11 A.M., but there is no pronounced maximum; two minor maxima, however, occur, one at 6 A.M. and the other at midnight.

On the whole it must be said that the results of this first and praiseworthy attempt are very encouraging, and point to the supposition that the greater portion of the disturbing currents lies outside the Earth's surface.

I now come to consider another aspect of the subject. It seems quite possible to conceive a set of currents to exist in the Earth's atmosphere without exhibiting a considerable diurnal variation. Let us take, for instance, an ordinary electric circuit, say of a circular shape and horizontal, and heat it by causing some source of heat, such as a lamp, to travel slowly around it with a definite rate of progress. It will be evident that we shall have (assuming the current to be otherwise constant) no variation in flow due to this heating effect. In like manner, if there be electric currents in the atmosphere which circulate round the Earth in the direction of parallels of latitude, such currents will not be subject to a considerable solar diurnal variation. For while the conductivity of a given region would vary according to the position of the Sun with regard to it, yet the whole circuit round the Earth, which would always embrace a region affected by the Sun, would not have its total resistance altered, at least not greatly altered; and as there would be no cause for much alteration of the total electromotive force, there would be no great reason for inconstancy of current—in other words, no great solar diurnal variation.

It may, however, be asked if there are any grounds for imagining the existence of such a set of currents. To this I would reply that, assuming the existence of currents in the upper regions of the atmosphere, their disposition would certainly be to some extent subject to magneto-electric considerations. If we bear in mind the distinct connection between

auroral beams and lines of magnetic force, we must certainly, I think, be prepared to admit the possibility of perhaps even a marked reference to magneto-electric considerations in the disposition of the currents in question.

For the purpose of the following argument we may consider the Earth to be at rest (*i. e.* devoid of rotation), and imagine that the Sun circulates round the equator in twenty-four hours. As a consequence of solar influence, we shall have convection-currents in the upper regions of the atmosphere flowing from the equator northwards and southwards towards the poles. Whether these currents reach the poles or come down in some intermediate region may be left an open question. Now such currents will not only be conductors, but they will form a movable system of conductors, which we may suppose to be created at the equator when they rise into the upper regions, and destroyed at the poles or those intermediate regions where they descend.

Again, for the purpose of this argument we may, without sensible error, look upon the magnetic Earth as similar to the class model that is sometimes used to represent it; that is to say, as a globe wrapped round continuously with insulated wires in the same direction which convey an electric current, the circles of such wires being small near the poles, and of course large at the equator. Now if an external insulated circuit of wire a trifle larger than the diameter of this globe be supposed to travel from the equator to either of the poles, it will leave behind more convolutions of the primary globe-current than it approaches, and will therefore be traversed by an induced current in the same direction as that of the primary; and the continuous travelling of such an external insulated system might be supposed to increase the magnetic power of the globe. Applying the same sort of reasoning to the Earth and to the convection-currents under consideration, these may be imagined to be traversed by equatorial currents, the tendency of which in both hemispheres would be to increase the general magnetism of the globe. From the reason already given such currents would have no solar-diurnal variation, but yet they would be dependent upon the state of the Sun and would vary with it. For imagine a change to take place in the radiation of our luminary producing an excess of such rays as are greedily absorbed by the upper

atmospheric regions ; there would be (as I have before remarked) a sensible increase in the conductivity of these regions even if the electromotive force remained altered, and hence there would be an increase in the supposed equatorial current. In other words, such currents, while presenting no great diurnal variation, due to the carriage of a constant Sun round the Earth, would yet be eminently susceptible to any inconstancy in the sun itself. Now have we any such phenomenon connected with terrestrial magnetism? I think we have. The late John Allan Broun has, I believe, proved that we have changes in the mean daily value of the horizontal force which are simultaneous, and in the same direction at places on the Earth's surface very far removed from each other ; and I have endeavoured to show that the changes of this nature recorded by him depend, as far as we can judge from somewhat imperfect records, upon the state of the Sun's surface, an increased area of spotted surface coinciding apparently with increased values of the daily means of horizontal force all over the Earth. But my chief object in giving prominence to this part of the subject is with the view of advocating that the Gaussian method of analysis should not be applied merely to the solar diurnal variations of the three magnetic elements, but that they should likewise embrace a consideration of the simultaneous variations in the mean daily values of the elements at various stations. We must, in fine, consider the possibility at least of there being in the upper atmospheric regions, not merely currents which present a solar-diurnal variation, but others that have no such variation, while yet they may be highly susceptible to changes in the Sun. The double method of treating mathematically not merely the solar-diurnal variation, but likewise the simultaneous changes in the mean daily values of the elements, would thus appear to be necessary, and sufficient for giving us the required information. Already the Greenwich scheme of publication embraces not merely information connected with the solar-diurnal variation, but gives us likewise the mean daily values of the three elements ; and if a complete system of this nature be pursued by a sufficient number of well-equipped magnetical observatories, we may hope soon to obtain reasonably complete information regarding the existence and distribution of currents in the upper regions of the Earth's atmosphere.