Broun, Charles and Frederick Chambers, Eliot, and Hill, and the following conclusion is the result of their labours. We may assume that the Indo-Malayan region has for the mean of the year a barometric pressure probably below the general average of the earth. We might therefore imagine that during years of powerful solar influence this peculiarity would be increased. Now these observers have found that in this Indo-Malayan region the barometer is abnormally low during times of maximum sun-spots Again, western Siberia is a district which in the winter season has a pressure decidedly above the average, and we should therefore imagine that during years of powerful solar influence this winter pressure should be peculiarly high. But this is what Blanford has found in his discussion of the Russian stations to correspond with years of maximum sun-spots.[1]

Again, Frederick Chambers has enunciated the following laws as resulting from his discussion of various meteorological records:—

(1) Variations of the sun-spot area are succeeded some months afterwards in the Indo-Malayan region by corresponding abnormal barometric variations, a high barometer corresponding to a minimum of sun-spots.[2]

(2) This lagging behind is greater for easterly than for westerly stations. In other words, this, like other meteorological phenomena, appears to travel from west to east

We may therefore conclude that the barometric evidence as far as it goes is in favour of the hypothesis that the sun is most powerful at times of maximum sun-spots.

105. *Rainfall—Heights of Rivers and Lakes.*—In 1872 Meldrum of the Mauritius Observatory brought forward evidence showing that the rainfalls at Mauritius, Adelaide, and Brisbane were on the whole greater in years of maximum than in years of minimum sun-spots. Shortly afterwards it was shown by Lockyer (*Nature*, December 12, 1872) that the same law was observable in the rainfalls at the Cape of Good Hope and Madras.

Meldrum has since found that the law holds for a great number of stations, including eighteen out of twenty-two European observatories, with an average of thirty years' observations for each. The results are exhibited in the following table (XXXIV.):—

Name of Observatory.	Number of Years of Observation.	Excess (+) or defect (−) in maximum Sun-Spot years.
		Inches.
1. St Petersburg	41	+13·08
2. Christiania	31	+19·65
3. Edinburgh	51	+66·85
4. Königsberg	19	−22·79
5. Berlin	11	+10·95
6. Utrecht	11	+ 0·44
7 Munster	19	+22·02
8. Greenwich	52	+11·73
9. Breslau	53	+49·90
10. Bonn	11	+ 8·90
11. Brussels	41	+13·84
12. Prague	39	+23·57
13. Paris	64	+28·76
14. Vienna	11	+ 9·94
15. Kremsmünster	41	−18·35
16. Nicolaieff	11	+ 6·44
17. Geneva	41	− 6·16
18. Milan	39	−11·81
19. Rome	18	+18·30
20. Lisbon	11	+ 5·05
21. Palermo	41	+ 4·55
22. Athens	11	+10·86

It would, however, appear from the observations of Governor Rawson that the rainfall in Barbados forms an exception to this rule, being greatest about the times of minimum sun-spots.

106. Gustav Wex in 1873[3] showed that the recorded depth of water in the rivers Elbe, Rhine, Oder, Danube, and Vistula for the six sun-spot periods from 1800 to 1867 was greater at times of maximum than at times of minimum sun-spot frequency. These conclusions have since been confirmed by Professor Fritz.[4]

Quite recently Stewart (*Proc. Lit. and Phil. Soc. of Manchester*, 1882) has treated the evidence given by Fritz as regards the Elbe and Seine in the following manner. He divides each sun period, without regard to its exact length, into twelve portions, and puts together the recorded river heights corresponding in time to similar portions of consecutive sun periods. He finds by this means residual differences from the average representing the same law whether we take the whole or either half of all the recorded observations, and whether we take the Elbe or the Seine. The law is that there is a maximum of river height about the time of maximum sun-spots and another subsidiary maximum about the time of minimum sun-spots. There is some reason too to think that the Nile and Thames agree with those rivers in exhibiting a maximum about the time of maximum sun-spots and a subsidiary maximum about the time of minimum sun-spots, only their subsidiary maximum is greater than it is for the Elbe and Seine.

107. In 1874 G. M. Dawson came to the conclusion that the levels of the great American lakes were highest about times of maximum sun-spots. In this investigation the value of the evidence derived from rivers and lakes is no doubt greater than that derived from any single rainfall station, inasmuch as in the former case the rainfall of a large district is integrated and irregularities due to local influence thus greatly avoided.

108. Dr Hunter, director-general of statistics in India, has recently shown (*Nineteenth Century*, November 1877) that the recorded famines have been most frequent at Madras about the years of minimum sun-spots—years likewise associated with a diminished rainfall.

109. *Winds and Storms.*—Meldrum of the Mauritius Observatory found in 1872, as the result of about thirty years' observations, that there are more cyclones in the Indian Ocean during years of maximum than during years of minimum sun-spots.[5] The connexion between the two is exhibited in the following table:—

TABLE XXXV.—*Comparison of the Yearly Number of Cyclones occurring in the Indian Ocean with the Yearly Number of Spots on the Sun.*

Character as regards Sun-Spots.		Number of Hurricanes.	Number of Storms.	Number of Whole Gales.	Number of Strong Gales.	Total Number of Cyclones.	Number of Cyclones in Max. and Min. Periods.
	1847	5	0	0	0	5	
Max.	1848	6	2	0	0	8	23
	1849	3	2	3	2	10	
	1850	4	3	1	0	8	
	1851	4	2	1	0	7	
	1852	5	0	3	0	8	
	1853	1	1	5	1	8	
	1854	3	1	0	0	4	
Min.	1855	3	2	0	0	5	13
	1856	1	0	2	1	4	
	1857	2	1	1	0	4	
	1858	3	1	3	2	9	
	1859	3	2	6	4	15	
Max.	1860	7	4	2	0	13	39
	1861	5	2	2	2	11	
	1862	4	2	2	2	10	
	1863	5	2	1	1	9	
	1864	2	2	1	0	5	
	1865	2	2	3	0	7	
Min.	1866	1	4	2	1	8	21
	1867	0	4	2	0	6	
	1868	3	2	2	0	7	
	1869	3	1	3	2	9	
Max.	1870	2	1	5	3	11	31
	1871	3	2	3	3	11	
	1872	6	5	1	1	13	
	1873*	4	5	3	0	12	

* Up to May 31.

In 1873 M. Poëy[6] found a similar connexion between the hurricanes of the West Indies and the years of maximum sun-spots. He enumerated three hundred and fifty-seven hurricanes between 1750 and 1873, and stated that out of twelve maxima ten agreed.

110. In 1877 Mr Henry Jeula, of Lloyd's, and Dr Hunter found that the casualties on the registered vessels of the United Kingdom were 17½ per cent. greater during the two years about maximum than during the two years about minimum in the solar cycle.

111. *Temperature.*—Baxendell, in a memoir already quoted, was the first to conclude that the distribution of temperature under different winds, like that of barometric pressure, is sensibly influenced by the changes which take place in solar activity. In 1870 Piazzi Smyth published the results of an important series of observations made from 1837 to 1869 with thermometers sunk in the rock at the Royal Observatory, Edinburgh. He concluded from these that a heat wave occurs about every eleven years, its maximum being not far from the minimum of the sun-spot cycle. Sir G. B. Airy has obtained similar results from the Greenwich observations. In 1871 E. J. Stone examined the temperature observations recorded during thirty years at the Cape of Good Hope, and came to the conclusion that the same cause which leads to an excess of mean annual temperature at the Cape leads equally to a dissipation of sun-spots. Dr W. Köppen in 1873 discussed at great length the connexion between sun-spots and terrestrial temperature, and found that in the tropics the maximum temperature occurs fully a year before the minimum of sun-spots, while in the zones beyond the tropics it occurs two years after the minimum. The regularity and magnitude of the temperature wave are most strongly marked in the tropics.

112. The evidence now given appears at first sight to be antagonistic to that derived from the other elements both of magnetism and meteorology, and to lead us to conclude that the sun heats us most when there are fewest spots on its surface. This conclusion will not, however, be strengthened if we examine the subject with greater minuteness.

[1] *Nature*, November 25 and December 2, 1880.
[2] *Nature*, March 18, 1880.
[3] *Ingenieur Zeitschrift*, 1873.
[4] *Ueber die Beziehungen der Sonnenflecken Periode zu den Magnetischen und Meteorologischen Erscheinungen der Erde*, Haarlem, 1878.
[5] *Br. Assoc. Reports*, 1872.
[6] A. Poëy, *Sur les Rapports entre les Taches Solaires et les Ouragans des Antilles de l'Atlantique-Nord. et de l'Ocean Indien Sud.*

Scientifically we may regard the earth as an engine, of which the sun is the furnace, the equatorial regions the boiler, and the polar regions the condenser. Now this engine may be supposed to work in the following manner. Hot air and vapour are carried along the upper regions of the atmosphere from the equator to the poles by means of the anti-trade winds, while in return the cold polar air is carried along the surface of the earth from the poles to the equator, forming what is known as the trade-winds. Whenever the sun's heat is most powerful both trades and anti-trades should be most powerful likewise. But we live in the trades rather than in the anti-trades—in the surface currents, and not in the upper currents of the earth's atmosphere. When, therefore, the sun is most powerful is it not possible that we might have a particularly strong and cold polar current blowing about us? The same thing would happen in the case of a furnace-fire; the stronger the fire the more powerful the hot draught up the chimney, the more powerful also the cold draught from without along the floor of the room. It might thus follow that a man standing in the furnace-room near the door might be chilled rather than heated when the furnace itself was roaring loudest. In fact temperature is a phenomenon due to many causes. Thus a low temperature may be due (1) to a deficiency in solar power, (2) to a clouded sky, (3) to cold rain, (4) to cold winds, (5) to cold water and ice, (6) to cold produced by evaporation, (7) to cold produced by radiation into space.

Blanford has recently shown that at certain Indian stations a low mean temperature occurs when there is an unusually large rainfall and a great amount of clouds, a result in accordance with the conclusions previously enunciated by Professor Piazzi Smyth. Records of maximum and minimum temperature must not therefore be too closely associated with a maximum and minimum of solar power.

113. Considerations of this nature have induced Stewart to imagine (*Nature*, June 16, 1881) that the true connexion between sun-spots and terrestrial temperature is more likely to be discovered by a study of short-period inequalities of sun-spots than by that of the eleven-year period in which there is time enough to change the hygrometric state of the atmosphere and the whole convection system of the earth. He has accordingly discussed at some length two prominent sun-spot inequalities of short period (about twenty-four days), and endeavoured to see in what way they affect terrestrial temperature. From this it appears that a rapid increase of sun-spots is followed in a day or two by an increase of the diurnal temperature range at Toronto. Now an increase of diurnal temperature range most probably denotes an increase of solar energy, and we are thus led to associate an increase of solar heat with a large development of spots. This, however, is a point which requires further investigation.

114. *General Conclusion.*—On the whole we may conclude that the meteorological motions and processes of the earth are probably most active at times of maximum sun-spots, and that they agree with magnetical phenomena in representing the sun as most powerful on such occasions, although the evidence derived from meteorology is not so conclusive as that derived from magnetism.

HYPOTHETICAL VIEWS REGARDING THE CONNEXION BETWEEN THE STATE OF THE SUN AND TERRESTRIAL MAGNETISM.

115. *Principles of Discussion.*—In the following discussion we claim only to advance a working hypothesis, with the view of suggesting further inquiries into the subject of terrestrial magnetism. It seems therefore desirable that we should limit ourselves to such probable or possible causes as are known to exist and to operate on the earth. These various agents or causes will be described, and we shall endeavour to show that converging lines of evidence point in several cases to certain of these as being most likely to produce that particular type of effect which is exhibited in terrestrial magnetism. This course will in our view most readily suggest further inquiries with the view of confirming or disproving the various points of this working hypothesis. Believing that the introduction of any unknown cause can only be justified when known causes have been found insufficient to account for the phenomena in question, we have not advocated any direct magnetic action of the sun upon the earth. We have refrained from this for two reasons,—first, because from what we now know of the sun it appears to us unlikely that it should exercise an influence of this nature upon the earth, since a body at a high temperature possessing very strong magnetic properties is unknown to us; and, secondly, we shall see further on that such an influence will not explain the best-understood magnetic changes, nor is there in our opinion any magnetic phenomenon for the explanation of which it appears absolutely necessary to resort to this hypothesis. In fine, without presuming to deny the possibility of unknown influences of this nature, it does not appear to us that the time has yet arrived when we are called upon to resort to such as necessary aids to the discovery of further truth.

116. *Nature of Solar Variations.*—It is quite certain that there is a variability in the visible appearance of the sun's disk, which exhibits sometimes a comparatively large amount of spotted area while on other occasions it is entirely free from spots. Now it has been remarked by Thomson that were the sun an incandescent solid its surface would become cool in a few minutes. The astonishing property which our luminary possesses of pouring out continuously a vast amount of radiant energy must unquestionably depend upon machinery of great power by means of which fresh hot particles are rapidly brought from the interior to the surface, while those particles which have given out their light and heat are rapidly hurried downwards to be recruited from the great storehouse of heat in the sun's centre. In fine, a gigantic system of convection currents of this nature forms the essential condition without which the sun would not be able to continue shining as it does.

The mottled appearance of the sun's disk as seen through a telescope denotes no doubt the existence of a vast system of ascending and descending currents, the hot matter rising from beneath being denoted by the brighter portions and the cold matter descending from above by the darker portions of the structure. On certain occasions and in certain regions of the sun the scale of these phenomena is greatly increased, and we have a huge up-rush of bright and a corresponding down-rush of black matter—in fine, the well-known sun-spot with its bright faculous appendages. Whenever sun-spots are very frequent we should therefore expect the convection system of the sun to be particularly powerful, and the great velocity and size of the red flames or the higher portions of the convection system observed around the sun's limb on such occasions confirm us in this supposition. And if the convection system of the sun be particularly powerful when there are most spots on its surface, it would seem to follow that the radiation from our luminary should on such occasions be particularly powerful also. The spectroscope leads us to the same conclusion. It would appear from the observations of Lockyer and others that at times of maximum sun-spots certain definite regions of the sun when examined spectroscopically present all the appearances of a very high temperature.

We are unable to confirm these conclusions by direct observations of the sun's heating power. Actinometrical determinations have not yet been made with sufficient accuracy and persistence to decide this point experimentally. We have, however, evidence of an indirect nature derived from terrestrial magnetism and meteorology all tending to make us think that the sun is most powerful during times of maximum sun-spots. We have seen that on such occasions the solar influence upon the magnetism of the earth is peculiarly powerful in more than one way, and that its influence on meteorology is then peculiarly powerful also. although we are not so certain of this latter fact as of the former.

We may therefore take it to be most probable that the sun is most powerful at times of maximum sun-spots, and proceed from this basis to propound the two following questions:—in the first place, what is the nature of the solar influence upon terrestrial magnetism? and, secondly, why is this influence so much more easily perceived than certain forms of solar influence upon meteorology?

117. *Diurnal Magnetic Variations—Hypotheses regarding them.*—The various speculators on the cause of these phenomena have ranged over the whole field of likely explanations. (1) It has been supposed that the sun acts directly as a magnet upon the magnetism of the earth. (2) It has been imagined that convection currents established by the sun's heating influence in the upper regions of the atmosphere are to be regarded as conductors moving across lines of magnetic force, and are thus the vehicle of electric currents which act upon the magnet. (3) Faraday, reasoning from his discovery that oxygen is paramagnetic, and becomes weaker in its power when heated, and stronger when cooled, supposed that the sun by heating certain portions of the atmosphere renders them less magnetic while others, not subjected to any heating influence are rendered more magnetic. The action is equivalent to a displacement by means of the sun of the magnetic matter of the earth, and involves a displacement of the lines of force. Here too the solar action is associated with the great mass of the atmosphere. (4) It has been supposed by Christie and by De la Rive that the heat of the sun produces in the atmosphere and in the earth thermo-electric currents which produce the daily magnetic variations. It is not easy to perceive how we could have thermo-electric currents in the upper regions of the atmosphere, but there is no obvious objection to the generation of such currents in the crust of the earth. Thus the first hypothesis has no reference to the atmosphere whatever; the second deals with the upper atmospheric regions, the third with the great body of the atmosphere, while the fourth, as we have ventured to modify it, has reference to the crust of the earth.

118. *Discussion of these Hypotheses.*—Dr Lloyd and Mr C. Chambers[1] have both shown that direct solar magnetic action will not account for the peculiarities of the diurnal magnetic variation. Again (§ 48) we have strong evidence that changes in the range of the daily magnetic variation lag behind corresponding solar changes in point of time. Now this kind of behaviour is inconsistent with direct magnetic action, and points rather to an indirect magnetic effect caused by the radiant energy of the sun.

[1] *Proc. Royal Irish Academy*, February 22, 1858; *Phil. Trans.*, 1863.

119. Let us therefore dismiss the hypothesis of direct action and consider that of Faraday. 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. It is hardly credible that there should be such a great difference on these occasions in the sun's heating effect upon the great bulk of the atmosphere. Meteorologists have never observed such a difference, nor is there any marked corresponding inequality of diurnal temperature range. Meteorological evidence is thus against the diurnal magnetic changes being due to the heating up by the sun of the great mass of oxygen which constitutes the magnetic portion of the earth's atmosphere. Again, as there is a preponderance of hot oxygen in the northern hemisphere during the June and in the southern hemisphere during the December solstice, there ought according to this theory to be a well-marked annual variation of the magnetism of the earth, the northern hemisphere being at the same time differently affected from the southern. But there are no traces of such a phenomenon, the annual and semi-annual variations which we have already described (§§ 64–67) being of quite a different nature, and none of them very large.

120. Precisely the same objections apply with even greater force to the fourth hypothesis. It seems impossible to allow that any heating effect of the crust of the earth caused by the sun can be one and a half times as great at epochs of maximum as it is at epochs of minimum sun-spot frequency.

121. We are thus driven by the method of exhaustions to look to the upper regions of the earth's atmosphere as the most probable seat of the solar influence in producing diurnal magnetic changes, and it need hardly be said that the only conceivable magnetic cause capable of operating in such regions must be an electric current. Now we know from our study of the aurora that there are such currents in these regions—continuous near the pole and occasional in lower latitudes. A good deal has been said about the difficulty of imagining a daily set of currents to be generated in regions of such imperfect conductivity, but we shall see by and by (§ 134) that there seems ground for imagining that their conductivity may be much greater than has hitherto been supposed.

122. *Analogies between the Meteorological and Magnetical Systems of the Earth.*—We have in the first place a zone of maximum terrestrial temperature, the middle line of which is nearly coincident not only with the geographical but likewise with the magnetical equator. Again, there are possibly in the northern hemisphere two poles of greatest cold, which possibly do not greatly differ in position from those spots which we have called magnetic poles or foci. About the southern hemisphere we have no information.

Furthermore we believe that the hot air is carried from the zone of greatest heat to the place or places of greatest cold by means, no doubt, of the return trades which blow in the upper atmospheric regions. The hot air divides at this zone, one part blowing northwards in the northern and another southwards in the southern hemisphere. Now this zone, from which the anti-trades divide, has an annual motion of its own, being found farthest north at the June solstice and farthest south at that of December. Probably too the northern system is strongest in June and the southern system in December. If we now turn to the solar-diurnal variation of magnetic declination, we find here also a northern and a southern system (§ 41), the type of the one being antagonistic to that of the other. We find also that the northern system is strongest in June and the southern system in December.

Again, it seems probable from what we have now said that the anti-trades, strictly speaking, have reference not to the geographical equator and poles but to the zone of maximum and the poles of minimum temperature. Now, turning once more to the diurnal oscillations of the declination needle, it seems probable that the directions east and west must be interpreted as having a reference not to the geographical but to the magnetical pole (§ 45).

These analogies must be taken for what they are worth. Our object in introducing them has reference to the previous discussion, from which we concluded that the magnetic influence of the sun is probably due to currents in the upper region of the atmosphere—the cause of which we were content to leave in abeyance. Now these analogies would lead us to suggest that this cause, whatever it is, may perhaps be found to be related to the convection system of the earth on the one hand and to the magnetic system on the other.

123. *Analogies between Meteorological and Magnetical Weather.*—These remarks are borne out by the further analogy which appears to subsist betwixt what we have termed meteorological and magnetical weather. Let us take the solar-diurnal variation of declination. Not only is this variation similar in form to the diurnal variation of atmospheric temperature (§ 37), but the ranges of the two have a similar annual variation. And, as the element of meteorological weather affects the orderly march of the temperature range, just so the element of magnetical weather affects the orderly march of the declination range.

Furthermore, just as temperature-range weather progresses from west to east (§ 52), so declination-range weather would seem to progress in the same direction as the other (§ 52) although at a greater rate. It will doubtless require a more extended investigation to make us quite sure of this latter point; nevertheless we do not perceive the validity of the objection that is sometimes made to the hypothesis of progress in magnetic weather on the ground that magnetic influences are known to affect all portions of the globe simultaneously. It will, we think, be perceived that in the above statement no supposition whatever is made with respect to the rate of propagation of a magnetic influence through the earth; this may be instantaneous or it may not. It is supposed that we have here a travelling cause of excitement, say a travelling cause of currents in the upper regions of the atmosphere which progresses from west to east and always produces its most marked effect above those regions where it passes—just as the sun itself in passing from east to west produces a magnetic effect the various phases of which travel from east to west with the sun which causes them. We think too that this hypothesis of travelling causes of magnetic change is strengthened by the facts observed by Capello and described in § 97.

124. If, however, the objection made to this hypothesis refers to the fact disclosed by Broun (§ 85) that changes of horizontal force appear to take place simultaneously at distant parts of the earth's surface, then we think that analogy should lead us not to deny the possibility of a travelling magnetic excitement, but rather to suggest the possibility of there being some meteorological influence which, like the magnetical one above mentioned, may be found to take place simultaneously at different parts of the earth's surface. Now Broun (*Proc. Roy. Soc.*, May 11, 1876) has given us preliminary evidence for supposing that there are simultaneous barometric variations. For instance, there was a barometric maximum at Hobart Town, Peking, the Cape, St Helena, Makerstoun, Singapore, Madras, Simla, Ekaterinburg, and Bogoslovsk about the end of March or first day of April 1845. There appears to have been a simultaneous increase of the horizontal force of the earth at various stations much about the same time, and there also appears to have been a short-period maximum of spots on the solar surface. Broun has likewise registered simultaneous barometric variations at Singapore, Madras, and Simla, for the first three months of 1845. From these it would seem that simultaneous barometric maxima are possibly coincident with rapidly increasing sun-spot areas.

Again is it not absolutely certain that if there is a sudden increase of solar power this must mean an increase of heat communicated to the earth, although it may be difficult or even impossible to obtain experimental evidence of such a fact? All these are subjects which require further investigation.

125. *Further Remarks on the Solar-Diurnal Variation of Declination.*—In § 24 we have asked how far the action of the solar-diurnal force upon a freely-suspended magnet is due to currents acting directly upon the magnet and how far to a change produced in the magnetism of the earth. Some light appears to be thrown on this point by the behaviour of the needle at places near the magnetic pole where the dipping needle is nearly vertical. On opposite sides of this locality the declination needle points in opposite directions. Now suppose that we have a set of such needles placed all round this region. It seems a legitimate generalization from the observations described by Sabine (§ 45) to conclude that if we place ourselves above the centre of any of these needles at 8 A.M., and look towards its marked pole, we shall find it in every case deflected towards the right, while if we look towards the same pole at 2 P.M. we shall find it deflected to the left. Now if we imagine that at 8 A.M. there are above these magnets (in the upper atmospheric regions) electrical currents of which the horizontal components form a set of positive currents flowing from the pole on all sides, then by the known laws of such currents the marked pole of all these needles will be deflected towards the right. And if at 2 P.M. the resolved portions of such currents should be flowing towards the pole, then the marked poles of all these needles will be deflected towards the left. It thus appears that this peculiar magnetic behaviour might easily be explained by a hypothetical distribution of currents. And in fact in such regions we have indubitable evidence of the existence of currents in the upper regions of the atmosphere. On the other hand this behaviour could not easily be explained by the hypothesis of some definite temporary magnetic system set up by the solar influence in the earth, for in such a case we should imagine that similar poles of all the needles ought to be deflected towards the pole of this temporary system, which is not the case.

126. Another point for consideration is the possible complexity of the solar-diurnal variation. For we may imagine (1) that the sun acts in such a manner as to produce a diurnal variation; (2) it may also act like the moon (§ 94) and produce a semidiurnal variation; (3) these possible actions may be accompanied by induced currents in the upper regions of the atmosphere and in the crust of the earth; (4) it is possible that the sun's rays may affect these variations or some of them in the way in which Broun found that the lunar variation at Trevandrum was affected by the sun. It

was found by him that the lunar action was considerably increased when the sun was above the horizon of the place.

127. We have pointed out (§ 119) that, while there is a marked likeness in many respects between the diurnal variation of declination and that of atmospheric temperature, we have yet no long-period fluctuation of the diurnal range of temperature at all comparable in magnitude to the magnetic fluctuations. It does not, however, seem difficult to account for this difference if we imagine that the magnetic fluctuations take their origin in the upper atmospheric regions, while the temperature fluctuations are due to the lower regions of the earth's atmosphere. For, as the sun increases in power from times of minimum to times of maximum sun-spot frequency, we may imagine that a continuously increasing amount of aqueous vapour will be taken into the earth's atmosphere.

Now the experiments of Tyndall and others induce us to think that the air would under such circumstances become more and more opaque for certain rays of the sun, and thus a continuously decreasing proportion of the sun's heat would be able to penetrate into the lower atmospheric regions. This latter influence would therefore operate to cloak, perhaps to a considerable extent, the effect of the sun's increasing power; and this may very well be the reason why the temperature range at the earth's surface does not exhibit the same eleven-yearly inequality as the declination range.

128. There seems, however, reason to believe that if we go from long to short period inequalities there is a much greater similarity in the range of the magnetical and the meteorological changes (§ 113). The explanation seems to be that in the short-period changes the sun has not time to alter sensibly the constitution of the atmosphere, and hence the proportional increase of effect experienced in the upper atmospheric regions is more nearly the same as that experienced near the surface of the earth.

129. *Magnetic Disturbances.*—There is strong evidence that the most important disturbances break out very nearly simultaneously at widely different parts of the earth, and that they even affect both hemispheres at the same time. Very little, however, is known about the *modus operandi* of the forces concerned in producing such disturbances. For instance, it is not known whether a disturbance permanently affects the magnetic state of the earth, *e.g.*, whether one of the magnetic elements before a disturbance begins is sensibly different in value from what it is after the disturbance has ceased to exist. On the other hand we know (1) that disturbances break out on the very day when there are rapid changes taking place on the sun's surface (§ 83); (2) that they generally begin by momentarily increasing the horizontal force, but that the type quickly changes, so that during most disturbances the horizontal force is diminished (§ 86); (3) that large disturbances take place more particularly about the equinoxes, when, we have reason to believe, the horizontal force of the earth is at a minimum (§ 77). May we not possibly conclude from these habits of action that at times of disturbance the earth is magnetically in a delicate state of equilibrium, perhaps having more magnetism than its surroundings would strictly warrant, and being therefore inclined to part with some, and that a sudden increase of solar activity, tending, as such changes probably do, at first to exalt the magnetism of the earth, nevertheless destroys its magnetic balance and gives it ultimately the opportunity of parting with some of its magnetism? This can only be regarded as a speculation, inasmuch as we do not know whether or not a disturbance produces any permanent influence upon the magnetism of the earth.

130. *Auroras and Earth Currents.*—There is no doubt that these phenomena denote electric currents in the upper regions of the atmosphere and in the moist conducting crust of the earth. The point in dispute is with respect to the origin of such currents. Some are inclined to regard auroras as peculiar manifestations of atmospherical electricity in high latitudes, while others imagine that such displays are rather of the nature of induced currents generated by small but abrupt changes taking place in the magnetism of the earth. The advocates of the first view do not deny that currents taking place somehow in the upper atmospheric regions will have their conditions modified, to some extent at least, by the inducing influence of magnetic changes. Nor will the advocates of the induction hypothesis be disposed to deny the possibility or even the certainty that displays due to atmospherical electricity and not dissimilar to some kind of aurora take place in some region of the atmosphere. But the first party regard auroras rather as the cause than as the effect of magnetic changes, whereas the advocates of induction regard such displays rather as the effect than as the cause of changes somehow produced in the magnetism of the earth. And here it is desirable to remark that the advocates of the induction hypothesis take for granted the magnetism of the earth and the changes thereof as phenomena for which they do not profess to account, whereas unless we go to some absolutely unknown cause (and this is against our present programme) we must look to atmospherical electricity as likely to throw light upon the origin of terrestrial magnetism. We cannot therefore dispense with regarding atmospherical electricity as an agent which may have played an important part in the development of the present magnetical condition of the earth, but we are yet of opinion that, under the present state of things, the theory which holds by atmospheric electricity must largely be supplemented by the induction hypothesis if it is to explain the peculiarities in type or form of the phenomena which observation brings before us.

131. Professor Tait in his essay on thunderstorms attributes one kind of aurora to atmospherical electricity. Such an aurora is, he believes, the manifestation of almost continuous discharges, like those given by a Holtz machine in a vacuum tube. The cause is condensation of vapour going on very slowly in very large spaces of air. The electricity is due to previous contact of particles of air and vapour. The result is that the air-particles in the mixture in time acquire a definite difference of potential from those of vapour,—so that, when the latter aggregate, a misty region well charged is the result, and this discharges to the oppositely electrified air all round.

132. Again, Professor Stokes, without attempting to account for the origin of atmospherical electricity, has produced an hypothesis with the view of explaining the intimate connexion subsisting between auroral displays, earth currents, and magnetic changes on the one hand and outbursts of sun-spot activity on the other. His idea is that two somewhat distant atmospheric regions A and B are charged, let us say, with positive and negative electricity respectively; A induces in the ground below it a charge of negative, B a charge of positive electricity. At first things are held in this state: A cannot discharge either through the upper atmospheric regions to B or through the lower regions to the ground beneath it, while B is in a position precisely similar. Presently, however, an increase of the radiative power of the sun is produced. Such an increase would probably imply not merely an increase in general radiation but a particular increase in such actinic rays as are absorbed in the upper regions of the earth's atmosphere. The layer of atmosphere between A and B will therefore greedily absorb such rays, its temperature will rise, and, as is known to be the case for gases, the electrical conductivity of the stratum will be increased. A discharge will therefore ultimately take place in the upper regions between A and B; this will relieve the charges of negative and positive in the ground immediately beneath A and B, and these charges will therefore rush together through the ground, producing an earth current. This earth current will be in the opposite direction from the atmospheric current, and the two will combine to represent, virtually at least, if not absolutely, a closed circuit. This will of course affect the earth's magnetism and produce a disturbance.

133. This hypothesis certainly affords a good explanation of the promptness with which disturbances follow increased solar activity (§ 83). Unless we are to resort to some unknown cause it is difficult to think of any other possible explanation of this fact. Such an explanation appears too to receive corroboration from the fact (§ 97) that the lunar influence on the earth's magnetism as observed at Trevandrum is greater during the day than during the night,—greater possibly too at times of maximum than at times of minimum sun-spots. We are therefore disposed to accept this explanation of the way in which increased solar activity produces magnetic disturbance as the best that has been brought forward.

134. This does not, however, decide the disputed point how far these elevated currents are due to atmospherical electricity and how far to induction. The argument against the possibility of induced currents in these regions is derived from experiments with vacuum tubes, such as those recorded by Messrs De la Rue and Müller, which would seem to indicate that enormous differences of potential would be required to produce electrical currents in elevated regions, where the atmosphere is very rare.

Indeed, on account of these experiments, the measurements of the old observers, who sometimes assigned a height of more than 100 miles to the aurora, have been called in question, and it has been supposed against direct observation that these phenomena must always occur in regions much less elevated. It would appear too that such reasons were influential in determining Professor Stokes to regard the aurora as produced by atmospherical electricity, which, as we know from ordinary lightning, presents us with enormous differences of potential; but it is to be remarked that he has carefully guarded himself against the possibility of laboratory experiments with vacuum tubes not being strictly analogous to that which takes place in the upper atmospheric regions. Now it would appear that recent experiments by Hittorf throw some doubt upon the strictness of this analogy. The high difference of potential required to force the current through vacuum tubes is, according to this observer, due in great part if not entirely to the passage of the fluid from the terminal to the residual air of the tube, so that the potential requisite to pass a current through a tube of double length is not sensibly greater than that required for a tube of single length. The whole subject is one which demands further investigation; meanwhile we are not disposed to assert the impossibility of induction currents taking place in the upper atmospheric regions.

135. Let us now consider whether the form or type of the earth currents observed during disturbances favours the presence of induction to any sensible extent. The remarks of Dr Lloyd already quoted (§ 93), which are confirmed by the Greenwich observations, seem to be decisive in this respect. These may be interpreted in

the following manner. In a magnetic disturbance we have frequently a general displacement of the various elements—the horizontal force, for instance; now on the curve which represents this slow but considerable displacement a large number of comparatively small but very abrupt changes are superimposed. These latter appearances are invariably accompanied by quick and strong alternations from positive to negative of the earth currents, while the former slow motion, although it may be of large range, hardly appears to have any galvanic equivalent at all. This would appear to favour the induction hypothesis, according to which small but abrupt magnetic changes should give rise to strong earth currents alternately positive and negative without reference to the position of the magnet above or below its normal at the time.

136. Another fact bearing upon this hypothesis is that mentioned in § 88. From this it would appear that on ordinary occasions the curves recording the progress of the declination needle at Kew and Stonyhurst are as nearly as possible identical, but on occasions of disturbance the range at Stonyhurst is greater than that at Kew by an amount not apparently depending so much on the magnitude of the disturbance as on its abruptness. The introduction of the element of abruptness would appear to be in favour of the mixing up to some extent of induced currents with the phenomena in question.

137. 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. It seems, however, possible that the peaks and hollows alluded to in § 73 may form an important and integral part of the daily magnetic movement, and there even appears to be some evidence that the diurnal progress of the earth currents bears a nearer resemblance to that of the peaks and hollows than it does to the progress of the smoother curve which is usually held to represent the diurnal variation. But this is a question which can only be decided by more prolonged investigations.

138. To conclude, there can be no doubt that at times of great magnetic disturbance we have currents in the upper atmospheric regions and in the crust of the earth which, so far as we can see, must either be due to atmospherical electricity or to induction, or to a mixture of both. The proportions of this mixture can only be decided by further inquiry and by the multiplication of stations where atmospherical electricity and earth currents may be observed. It ought to be mentioned that the experience of the Kew observers, as far as this extends, seems unfavourable to the hypothesis of a connexion between auroras and atmospheric electricity.

139. *Lunar-Semidiurnal Variation.*—From the fact observed by Broun (§ 98) that the moon's magnetic influence is as nearly as possible inversely proportional to the cube of the moon's distance from the earth, it is impossible to refrain from associating it either directly or indirectly with something having the type of tidal action, but in what way this influence operates we cannot tell. Is it possible that the earth currents observed by A. Adams (§ 101) are induction currents generated in the conducting crust of the earth by the magnetic change caused by the moon,—inasmuch as these currents were found by him to be strongest in one direction about the lunar hours 3 and 15, when the lunar-diurnal magnetic effect is changing most rapidly in one direction (§ 95), while they were found to be strongest in an opposite direction about the lunar hours 9 and 21, when the lunar-diurnal magnetic effect is changing most rapidly in an opposite direction?

140. We might perhaps expect from the analogy of the tides that the sun should possess a semidiurnal magnetic effect similar in type to that of the moon. Now Sir George Airy in his analysis of the earth currents observed at Greenwich (*Phil. Trans.*, 1870) during days of tranquil magnetism has detected in such currents a semidiurnal inequality having maxima in one direction at solar hours 3 and 15, while it has maxima in the opposite direction at solar hours 9 and 21. The reference to solar hours in this inequality is thus precisely similar to that which the inequality observed by Adams bears to lunar hours.

141. If there are induced currents of this nature in the crust of the earth, we might naturally suppose that there will be corresponding currents in the upper regions of the earth's atmosphere, and in accordance with the suggestion made by Professor Stokes (§ 132) we might perhaps suppose that these currents will be strongest when the upper atmospheric regions are heated by the sun and thereby rendered better conductors. Is it not possible to suppose that the influence of daylight upon the lunar magnetic effect discovered by Broun (§ 97) may be due to this cause, and may it not also induce us to recognize the possibility of a maximum lunar influence (§ 99) at times of maximum sun-spots, when there is reason to believe that solar radiation is most powerful?

142. *Secular Variation.*—Sabine and Walker are agreed in regarding this variation as cosmical in its origin, and they are apparently of opinion that it is caused by some change in the condition of the sun. It seems difficult if not impossible to attribute it to anything else, since the terella of Halley cannot be now regarded as having a physical existence. Again it is more than possible—it is probable—that there are solar variations of much longer period than eleven years. On the other hand the evidence given in § 81, tending to show that an access of sun-spots produces a change in the magnetic state of the earth consistent with the hypothesis that the magnetizing power of the sun has then been augmented, requires to be confirmed by more observations; and even then it is certain that this magnetic change produced by a considerable change in spotted area is extremely small. We cannot therefore regard the very large secular magnetic change as due to a non-cumulative magnetic influence of some long-continued solar variation; nor does it seem possible to attribute the change to solar influence at all unless we regard this influence as producing results of a cumulative nature.

It is possible, however, to regard solar influence as producing a cumulative effect in one of two ways, or by a combination of both. For (1) time is necessarily an element in any influence acting upon the hard-iron system of the earth—presuming the earth to possess such a system. There are in fact indications in the results of § 82 that a system of this kind is perhaps connected with the American pole; yet, even allowing the influence of time, it seems difficult to account for the peculiarities of the secular variation by an hypothesis of this nature. But (2) any long-continued variation of solar power would no doubt act cumulatively in producing an increase or diminution of the large ice-fields round the poles of the earth. In the course of time this cumulative change in the extent and disposition of these might perceptibly alter the distribution of the convection currents of the earth—and these, according to the views herein indicated, might in their turn perceptibly alter the earth's magnetic system.

143. *Concluding Remarks.*—If we agree to look for an explanation of terrestrial magnetism and its changes to strictly terrestrial processes, we may derive some assistance in our search from such considerations connected with symmetry as enable us, for example, at once to perceive that when two perfectly similar things are rubbed together we cannot have electrical separation, because there is no reason why the one should be positively and the other negatively electrified. Suppose then that an observer stands at the equator and looks towards the north, and then turns his back upon the north and looks towards the south. In the first position let him regard the northern system of meteorological processes and motions, and in the second the southern. Now if symmetry obtained absolutely in these systems—that is to say, if the observer, whether he regarded the northern or the southern system of things, had in either case precisely similar phenomena at his right hand and at his left—then we should see no reason why the earth should be a magnet, or why one hemisphere should be the seat of magnetism of the one kind rather than of the other. If then we regard meteorological processes and motions as being in some way the cause of terrestrial magnetism, we must direct our attention to that peculiar element which causes a want of perfect symmetry such as we have described in meteorological phenomena. This element can hardly be anything else than the rotation of the earth, which is from left to right to an observer facing the north, but from right to left to an observer facing the south.

144. Now if we look upon the terrestrial meteorological system modified by the earth's rotation as having produced somehow in the past the magnetic state of the earth, it seems most natural to regard the system which formerly produced this magnetic state as being likewise that which at present maintains it in its efficiency, and which also accounts for the various magnetic changes which take place. It would seem therefore that terrestrial meteorology and terrestrial magnetism are probably cognate subjects, and that they ought to be studied together in the well-founded hope that the phenomena of the one will help us to explain those of the other.

Furthermore, if these meteorological processes—deriving their one-sided character from the earth's rotation—are to be regarded as accounting not only for the origin but for the maintenance of the earth's magnetic system, we can hardly fail to imagine that these processes must derive part of the energy which they exhibit from that of the earth's rotation. Tidal energy we know is derived from this source; but we must likewise regard part of the energy displayed in convection currents whether in the air or in the ocean as derived no doubt from the same source. And we may perhaps allow that in the phenomena of tidal action, as well as in those of convection currents of the air and ocean, there may be, not merely a transmutation of actual energy directly through friction into heat, but likewise a transmutation of it, ultimately perhaps into heat, but first through the intermediate agency of electrical currents which serve to maintain the magnetic state of the earth and to produce magnetic changes.

Now if this be the case, if there be a large and complicated system of tidal and convection currents all tending to change the rotative energy of the earth ultimately into heat, whether directly through friction or indirectly through the medium of electricity, it is surely impossible with the present state of our knowledge to calculate with the smallest pretensions to accuracy at what rate this transmutation is taking place, and hence at what rate the velocity of the earth's rotation is being slowly diminished. (B. S.)

METHODISM

I. Wesleyan Methodism.

The history of Wesleyan Methodism embraces—(1) the Methodism of Oxford, which was strictly Anglican and rigidly rubrical, though it was also more than rubrical; (2) the evangelical Methodism of the Wesleys after their conversion (in 1738), of which the Wesleyan doctrines of conversion and sanctification were the manifesto and inspiration, while preaching and the class-meeting were the great motive and organizing forces,—a movement which before Wesley's death had developed into a form containing, at least in embryo, all the elements of a distinct church organization, although in its general designation and deliberate claims it purported to be only an unattached spiritual society; and (3) Wesleyan Methodism since the death of Wesley, which, by steps at first rapid and afterwards, though leisurely, distinct and consecutive, assumed an independent position, and has grown into complete development as a church.

1. *Oxford Methodism.*—This began in November 1729, when John Wesley, returning to Oxford from Lincolnshire, where he had been serving his father as curate, found that his brother Charles, then at Christ Church, had induced a few other students to join him in observing weekly communion. John Wesley's accession lent weight and character to the infant association. Their first bond of association, besides the weekly communion, was the common study of the Greek Testament, with which they joined regular fasting, the observance of stated hours for private devotion, the visitation of the sick, of the poor, and of prisoners, and the instruction of neglected children. They never themselves adopted any common designation, but of the variety of derisive names they received from outsiders that of "Methodists" prevailed,—a sobriquet the fitness of which, indeed, as descriptive of one unchanging and inseparable feature of Wesley's character (which he impressed also on his followers), was undeniable.

This first Oxford Methodism was very churchly. Between 1733 and 1735, however, a new phase was developed. Its adherents became increasingly patristic in their sympathies and tendencies, and Wesley came much under the influence of William Law. In regard to this period of his history, Wesley himself says that he

> "Bent the bow too far, by making antiquity a coordinate, rather than a subordinate, rule with Scripture, by admitting several doubtful writings, by extending antiquity too far, by believing more practices to have been universal in the ancient church than ever were so, by not considering that the decrees of a provincial synod could bind only that province, and the decrees of a general synod only those provinces, whose representatives met therein, that most of those decrees were adapted to particular times and occasions, and, consequently, when those occasions ceased, must cease to bind even those provinces."

It was in 1736, during his residence in Georgia, whither he had gone as a missionary of the Propagation Society, that he learnt those lessons. Notwithstanding his ascetic severity and his rubrical punctilios, the foundations of his High-Churchmanship were gradually giving way. When he returned to England he had already accepted the doctrine of "salvation by faith," although he had not as yet learned that view of the nature of faith which he was afterwards to teach for half a century. He had, however, as in the journal of his homeward voyage he tells us, learned, "in the ends of the earth," that he "who went to America to convert others was never himself converted to God." In this result his Oxford Methodism came to an end.

The original Methodism of Oxford never at any one time seems to have numbered as many as thirty adherents. There was a set called "Methodists," but there was no organization, no common bond of special doctrine or of discipline; there were habits and usages mutually agreed upon, but there was no official authority, only personal influence. The general features of the fraternity, if fraternity it may be called, seem to suggest closer analogies with the "Tractarian" school in its earlier stages than with anything else in modern history, and the personal ascendency of John Wesley may remind us in some measure of the influence exercised a century later by J. H. Newman. There was no more any germ of permanent organization in the Oxford Methodism of 1735 than in the patristic and "Tractarian" school of Oxford of 1833.[1]

2. *Methodism after Wesley's Conversion.*—John Wesley landed at Deal, on his return from Georgia, on February 1, 1738. His journals on the homeward voyage, says Miss Wedgwood,[2] "chronicle for us that deep dissatisfaction which is felt whenever an earnest nature wakes up to the incompleteness of a traditional religion; and his after life, compared with his two years in Georgia, makes it evident that he passed at this time into a new spiritual region." . . . . . "By Peter Böhler,[3] in the hands of the great God," he writes in his journal, "I was, on March 5, fully convinced of the want of that faith whereby we are saved." This "conviction" was followed on March 24 of the same year (1738) by his "conversion."

Like most good men of that age in England, Wesley, before he came under the influence of his Moravian teacher, had regarded faith as a union of intellectual belief and of voluntary self-submission—the belief of the creeds and submission to the laws of Christ and to the rules and services of the church, acted out day by day and hour by hour, in all the prescribed means and services of the church and in the general duties of life. From this conception of faith the element of the supernatural was wanting, and equally that of personal trust for salvation on the atonement of Christ. The work of Böhler was to convince Wesley that such faith as this, even though there might be more or less of divine influence unconsciously mingling with its attainment and exercise, was essentially nothing else than an intellectual and moral act or habit, a natural operation and result altogether different from the true spiritual faith of a Christian. This conviction led him a few days afterwards to stand up at the house of the Rev. Mr Hutton, College Street, Westminster, and declare that five days before he had not been a Christian. When warned not thus to despise the benefits of sacramental grace, he rejoined, "When we renounce everything but faith and get into Christ, then, and not till then, have we reason to believe that we are Christians." It is true that for several years after this he remained High-Church in

[1] One evidence of this is to be found in the early and wide divergence of the various members of the Oxford Methodist company, after their brief association at the university came to an end. We know which way the Wesleys went; we know also the separate path that their friend Whitefield made for himself. John Clayton, the Jacobite churchman, settled at Manchester, renounced the Wesleys after they began their evangelical movement, and remained an unbending High-Churchman to the end. Benjamin Ingham became a great evangelist in Yorkshire, founded societies, and, with his societies or churches, took the decisive step of leaving the Church of England and embracing the position of avowed Dissent. The saintly Gambold, a poet as well as a theologian and preacher, became a Moravian bishop. James Hervey was in after life a famous evangelical clergyman, holding "Low" and Calvinistic views. These were the chief of the Methodists of Oxford.

[2] *John Wesley and the Evangelical Reaction of the* 18*th Century.*

[3] A disciple of Zinzendorf, then in England on his way to America.