

THURSDAY, JANUARY 21, 1875

## DR. LLOYD'S "TREATISE ON MAGNETISM"

*A Treatise on Magnetism, General and Terrestrial.* By Humphrey Lloyd, D.D., D.C.L., Provost of Trinity College, Dublin. (London: Longmans and Co., 1874.)

AN observational science like meteorology or terrestrial magnetism is placed in some respects at a disadvantage when compared with the more experimental branches of physical inquiry. It is often difficult to obtain a good and readable account of that which has been done. The reason of this is, that those who are personally engrossed with the science have to deal with such large masses of figures and precise measurements that they are frequently unable to spare the time necessary to give a good historical account of their favourite research. Those again who are the historians of science find it a very formidable task to bring themselves *en rapport* with all that has been done in such a subject as terrestrial magnetism—in fine, there is not sufficient inducement to undertake the task. No doubt, when such a science is more advanced and has attained a position like that of astronomy, it will find plenty of historians; but in its infancy, and when a good *résumé* of the progress already made is of peculiar value, it has comparatively few friends. Now these are precisely the circumstances when a Government or a University is able to interfere with very great effect, and with respect to terrestrial magnetism this opportunity has been admirably used by Trinity College, Dublin. The Rev. Dr. Lloyd tells us in his preface that the Dublin Magnetic Observatory was founded and placed under his superintendence by the governing body of Trinity College in 1838. This college has been peculiarly fortunate in having chosen as an observer the eminent physicist who is now its provost, and who, besides reaping much fame as a practical magnetician, has at length found leisure to present us with the much-required treatise on terrestrial magnetism.

The first part of this work refers to the general phenomena of magnetism, and contains one of the clearest accounts of the elementary laws of this subject which we have ever read. Some of the experiments recorded we do not remember to have seen anywhere else.

One of the most interesting preliminary chapters is that on the effects of temperature. It is well known that heat has a very peculiar effect upon all magnets. The following paragraph from Dr. Lloyd's work will explain the particulars of this action:—

"Heat is also found to weaken the coercitive force of iron, and therefore to facilitate its magnetisation and demagnetisation. When a bar of iron is heated and exposed to the inductive action of a strong magnet, the magnetism developed is augmented. This effect increases up to a *dull red* heat, at which it is a *maximum*. At a *bright red* heat the capability of induction ceases altogether. *Cast-iron* and *steel* present the same results. The maximum force imparted to soft iron has been found by M. Ed. Becquerel to be 1·04, that imparted at the ordinary temperature of the air being unity; and it is a remarkable circumstance that the maximum force induced in *cast-iron* and in *steel* is precisely the same as that of *soft iron*, although at ordinary temperatures their induced magnetisms are very different. It appears from these facts that the coercitive force of these bodies vanishes altogether at a *dull red* heat" (p. 23).

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Although it may be convenient to speak of the magnetic fluid, yet we think there can be little doubt that in a magnet we have directed molecular motion of some kind. This, we believe, is the hypothesis held by Sir W. Thomson and other physicists. The author of this notice has ventured to bring forward certain views as to the action of heat in destroying all directed motion. We know, for instance, that the conduction of electricity and of heat, two forms of directed molecular motion, is more resisted at high than at low temperatures. The analogy urged to explain this was that of a carriage or train in motion, on a road lined with passengers who were constantly entering at the one side while they were passing out of it at the other.

A stream of passengers of this nature would have the effect of bringing the directed motion of the train ultimately to rest. Now, heat may affect directed molecular motions in the same way, carrying into the train matter which does not partake of the motion of the train, and carrying out of the train matter which does partake of this motion, and so weakening the velocity of the train. Even visible directed motion may be influenced in this way, and it does not seem improbable that the etherial medium may act after this fashion in stopping the differential motions of the universe.

Now the question arises, is it likely that we have any action of this nature traceable in the effects of heat upon magnetism? Let us again quote from Dr. Lloyd as to certain peculiarities of the action of heat:—

"When the heat applied to a steel magnet is moderate—when, *e.g.*, it does not exceed that of boiling water—part of the magnetism which had disappeared on the increase of temperature reappears when the original temperature is restored. It follows from this that heat produces two effects, which (in the present state of our knowledge) must be considered as distinct.

"Like mechanical action, it permanently *destroys* a portion of the existing magnetism by enabling the two magnetisms which had been separated in each molecule to recombine. And, on the other hand, it *renders latent*, or neutralises, another portion of the same magnetism, which portion reappears again when the temperature is reduced to its original state.

"This two-fold operation of heat, although fully recognised as a fact, has not been sufficiently considered in reference to the cause. There seems reason to believe that the two effects, so dissimilar in their conditions, are, in fact, referable to distinct causes; and that while the permanent loss of magnetism is a *dynamical effect* due to the *molecular movement* in which heat is known to consist, the recoverable portion is probably to be ascribed to the *dilatation* of the body and to the diminution of the reciprocal action of the magnetic elements consequent upon their increased distance."

We quite agree with Dr. Lloyd in his remarks on these two effects of heat, and would venture to supplement them with a suggestion as to the possibility of regarding the *dynamical effect* of heat as due to the introduction of new matter—new passengers, as it were—into the directed train of magnetic motion. If this view be tenable, we may perhaps imagine that a permanent loss of magnetism will be occasioned by every change of temperature of the magnet, whether this be from a lower temperature to a higher or from a higher temperature to a lower; in fact, as far as we can see, all the experiments hitherto made are just as compatible with this supposition as with

that which attributes a permanent loss of magnetism only to an increase of temperature.

We now come to the most valuable part of the work, or those chapters which treat of terrestrial magnetism. Dr. Lloyd tells us in his preface that the course he has pursued has been "to present the results obtained at a single station, *i.e.* Dublin, at which all the general features of the phenomena belonging to the middle northern latitudes were fully developed, and to supplement the information by the results of observation at places widely removed from the former in geographical position, as well as in their relation to the sun's daily and yearly courses."

Dr. Lloyd adds that "he has not entered upon the interesting speculation connected with the physical causes of the phenomena, further than to reprint a paper, published by himself many years ago, in which the agency of the sun and moon are shown not to be due to their direct operation as magnetic bodies." And he concludes his preface by remarking that "the electrical earth-currents must have their effect upon the magnetical variations recorded in our observatories, whether they be the sole, or only a co-operating cause."

Are we justified in our inference, from the concluding observation, that Dr. Lloyd has to some extent modified his views with regard to the importance of these earth-currents? for, if we are not mistaken, he was at one time inclined to attribute the daily variations of terrestrial magnetism to their operation.

Speculations regarding the causes of the phenomena of terrestrial magnetism may be divided into two classes: firstly, those which attempt to account for the magnetisation of the earth; and secondly, those which only pretend to account for the changes taking place in this magnetisation. It is from the latter point of view that we would now venture to make a few remarks.

Let us assume, to begin with, that nothing is definitely known regarding the causes of these changes. Let us next endeavour to enumerate and discuss the various agencies we know of which may be conceived to take a part in producing these phenomena, in the hope that by a preliminary trial of its kind we may, perhaps, light upon the true cause, even although the evidence at our disposal be insufficient to give certainty to our suspicions.

In the first place, we may take it for granted that neither the sun nor the moon can cause the changes in terrestrial magnetism, which they are known to produce, by virtue of their direct magnetic influence.

This point has been sufficiently discussed both by Dr. Lloyd and by Mr. Chas. Chambers, and the conclusion to which both of these magneticians have arrived is, that the magnetic effects caused by the sun and moon are not due to their direct operation as magnetic bodies.

Let us take the sun and confine ourselves in the meantime to the daily variations which he causes. Now, first of all, it is clear that these are not due to any kind of tidal action of the sun, or to the indirect consequences of such an action, inasmuch as there is only one maximum and one minimum in the day.

The only other known way in which the sun can affect the earth is through his heat; and starting with the assumption that the earth *is* a magnet, no matter how or why, let us next enumerate the various ways in which the heat of the sun may possibly affect the earth.

In the *first* place, it might influence the magnetic properties of that medium (the air) which surrounds the earth and any suspended magnet.

Or, *secondly*, it might produce a temperature effect upon the earth itself considered as a magnet.

Or, *thirdly*, it might be conceived to generate thermo-electric currents in the earth.

Or, *fourthly*, it might cause the motion of conducting bodies across the earth's lines of magnetic force.

The first of these is the hypothesis of Faraday; and while the change produced by heat in the magnetic qualities of the atmosphere cannot be without its influence, yet it is, we believe, the universal opinion of magneticians that this change cannot account, either in magnitude or law, for the somewhat considerable daily variation. The diurnal change produced by the sun's heat in the magnetic condition of the crust of the earth must be still more insignificant, and may be at once dismissed.

Our attention is thus concentrated on the third and fourth of the above possible causes, one of which we may perhaps expect to account for the daily variation, unless this be due to some cause of the nature of which we are entirely ignorant.

It is now well known that what are called earth-currents are of very frequent, if not continuous occurrence, and we are indebted to the present Astronomer Royal for an experiment made with the view of ascertaining the nature of the relation between these currents and the changes of terrestrial magnetism. He set up certain wires on the Croydon and Dartford lines, which gave him, by means of a self-recording arrangement, a continuous record of the strength and duration of these earth-currents, and the following is the conclusion which he has derived from the discussion of these observations:—

"Neither in magnitude nor in law are these inequalities consequent on the galvanic currents competent to explain the ordinary diurnal inequalities of magnetism."

In fact, there is some reason to regard these currents rather as the *effects* than as the *causes* of magnetic changes, that is to say, to view them as secondary currents; and the author of this notice has shown in a paper, published in the Transactions of the Royal Society of Edinburgh, that these earth-currents are strongest at those periods of the day when the change in terrestrial magnetism is most rapid—a result which would follow if the earth-currents were secondary currents due to magnetic changes. Our attention is thus drawn to the fourth hypothesis as the only remaining conceivable cause of magnetic changes, unless these are caused by something of which we are entirely ignorant.

It is known that Faraday tried to detect induction currents in the Thames, supposing that these might be caused by the carriage of a conducting liquid across the earth's lines of magnetic force, but found no positive result. Sir W. Thomson afterwards made a proposal to test the idea by tides in the English Channel, but we do not think this has ever been carried out. He also discussed to some extent the part which may be played in the phenomena of terrestrial magnetism by moving conductors.

But to return to the fourth hypothesis. In the first place, let us ask ourselves the question, Under what circumstances can the convection currents generated by the

sun's heat become conductors? Now, this can only take place in the upper and rarer regions of the atmosphere, since dense air is manifestly a non-conductor. We have therefore in the upper regions of the air a conductor—rare air—conveyed across the earth's lines of force by the convection due to the sun's heat, probably with a very considerable velocity. Now, is it not possible that these moving conductors may have currents generated in them which will act upon the magnet both directly and through the earth? As far as we are aware no attempt has yet been made to treat the question mathematically; indeed, we are hardly prepared for that at present, since we know very little about the convection currents in the upper regions of the earth's atmosphere.

We may perhaps, however, deduce the laws of the upper convection currents from what we know of the lower currents. Now, there are several points of similarity between the convection currents as we know them and the daily magnetic variations. The *first* in order is that noticed by Mr. Baxendell, who observed a very strong likeness between the daily behaviour of the wind and that of the magnetic declination.

The *next* is a resemblance between what we know takes place near the equator as regards the magnetic declination and what we imagine must take place as regards the upper convection currents. Sir E. Sabine has shown that near the equator the diurnal magnetic change is of an opposite character during the two halves of the year reckoning from the equinoxes, so that it is only at or near the equinoxes that the diurnal inequality might be expected to vanish as it passes from the one phase to the other. Now, we should quite expect something of this kind if the diurnal changes were due to convection currents; and just as the change which we might expect in the convection currents of these regions on account of the motion of the sun in declination would probably not be gradual, but of a hesitating or oscillatory character, so Mr. J. Allan Broun has found from his magnetic observations at Trevandrum (page 180 of Dr. Lloyd's work) that the magnetic change is not a gradual or regular one. This is a very important remark, and if followed up by a thorough discussion of the various tropical magnetic observations, may be expected to throw much light on the cause of the daily variation.

The third point we would notice is a peculiarity in the behaviour of the daily magnetic variation near the magnetic pole.

"The observations of Sir Leopold M'Clintock in 1858—59, at Port Kennedy," says Dr. Lloyd, "have enabled Sir Edward Sabine to throw further light upon the laws of the diurnal variations. The declination at Port Kennedy is N. 136° W.; while that of Point Barrow is N. 41° E. The north poles of the needles at the two stations, which are at opposite sides of the earth's magnetic pole, thus point in opposite directions. Now, when disturbances are removed, the observations gave the greatest deflections at 8 A.M. and 2 P.M., as in other places. But they showed, further, that the positions were referred in both to the *magnetic meridian* of the place, and not to the *astronomical*; the deviations of the magnet at 2 P.M., for example, being in both places to the *left* of an observer looking towards the magnetic pole at each place, and therefore *geographically* in opposite directions."

Now, meteorologically, the north magnetic pole is not far from the pole of greatest cold, and we might, perhaps,

expect on opposite sides of the pole to find the upper convection currents going in opposite directions. If this be the case, and if the daily variation be due to those currents, then we might also expect a magnetic behaviour such as was deduced by Sir E. Sabine from the observations of Sir L. M'Clintock.

We think, in fine, that the behaviour of the daily variation at the tropics, at middle latitudes, and near the magnetic pole, is not inconsistent with the hypothesis that such variation is due to convection currents. But if this hypothesis be true, it cannot be limited to the daily variation. We know very well that the currents of the earth's atmosphere often present great irregularities, and that these irregularities are especially prevalent at the equinoxes. Now, we have a precisely similar peculiarity in magnetic changes. These are frequently irregular, and their irregularities are greatest at the equinoxes. In proof of this we extract the following table from Dr. Lloyd's work:—

*Annual variation of the mean disturbance at Dublin.*

Month.	Mean Disturbance.	Month.	Mean Disturbance.
January	0·48	July	0·57
February	0·57	August	0·56
March	0·58	September	0·67
April	0·57	October	0·66
May	0·52	November	0·59
June	0·48	December	0·45

The next point to which we would allude is a similarity between the secular variation of the meteorology and magnetism of the earth. Mr. Baxendell, we think, was the first to point out that there is a change in the convection currents of the earth, depending on the state of the sun's surface with regard to spots; and Mr. Charles Meldrum has followed with the very interesting and important announcement that we have most frequent cyclones in the Indian Ocean during years of maximum sun-spots; and finally, M. Poey has shown that there is a similar correspondence between sun-spots and the hurricanes of the West Indies. In fine, we have here an intimate connection between solar and terrestrial meteorology. But we have also a connection between sun-spots and magnetic disturbances; and Sir E. Sabine was the first to point out that during the years of greatest sun-spots we have the greatest disturbance of terrestrial magnetism. Now, may not the increase of magnetic disturbance be due to the increase of meteorological disturbance which the sun somehow produces, the upper convection currents influencing the magnet in the manner above stated?

It is probable, however, that some will raise the following objection to this hypothesis. When there is a great magnetic storm or disturbance, this takes place simultaneously and abruptly throughout the whole earth; now, how can this be the result of a meteorological commotion?

We would reply to this objection that magneticians have begun to recognise two sets of disturbances.

When the writer of this notice was at the Kew Observatory, this was forcibly brought before him. There are disturbances of a rounded character, and there are others which are exceedingly abrupt; and we think that Senhor Capello has shown that these rounded disturbances are

certainly not simultaneous between Kew and Lisbon. The abrupt disturbances constituting magnetic storms are, however, probably simultaneous all over the world. It is thus possible to imagine the former or rounded disturbances to be caused by convection currents, but it is quite impossible to regard the latter as so caused. How, then, can these be accounted for consistently with this hypothesis? We reply, that when there is a considerable disturbance in the convection currents of the earth, these currents, as we have explained, conveying electricity, we may then expect such currents to influence and alter the magnetism of the earth. The earth gets out of relation as a magnet to these currents, and rights itself abruptly; and this abrupt change of the earth occurring simultaneously all over it, may form the second kind of magnetic storm.

Corresponding to these two varieties of magnetic disturbances, we have, in all probability, two kinds of auroras.

The upper convection currents of the earth, if they convey electric currents, may probably be self-luminous, and this may account for auroras of a local nature, and perhaps also for the nearly perennial displays of auroras near the magnetic pole.

On the other hand, whenever we have an abrupt magnetic storm we have the production of secondary currents due to the small but abrupt changes taking place in the magnetism of the earth, and these secondary currents will manifest themselves both in the upper strata of the earth's crust, which are conductors, and in the upper strata of the earth's atmosphere, which are also conductors. In the former case they will produce violent earth-currents; in the latter they will produce a magnificent auroral display, cosmical rather than local in its characteristics.

We have already alluded to the Greenwich self-recording instruments for registering earth-currents, and the author of this notice has inspected several of the curves given by the Greenwich instruments during violent magnetic storms. The characteristic of these traces is an abrupt and violent change from positive to negative and from negative to positive. Now, this is a behaviour quite in accordance with the hypothesis that these are secondary currents due to magnetic changes, but quite inconsistent with the hypothesis that they are themselves the causes of such changes.

Altogether, we would venture to conclude, *firstly*, that if the changes of terrestrial magnetism are not due to some such cause as that which we have stated, then they must be due to some cause of which we are entirely ignorant; and, *secondly*, that the laws of the magnetic changes are, in all the points we have examined, consistent with the idea that they are due to the carriage of conductors across the earth's lines of force.

B. STEWART

#### SIMON'S "SPIDERS OF FRANCE"

*Les Arachnides de France.* Par Eugène Simon, Vice-Président de la Société Entomologique de France. Tome premier. (Paris, 1874.)

EXCEPTING two or three, either partial or abortive, attempts at the early part of the present century, by Baron Walckenaër, no effort has, until now, been

made to supply a history of the spiders indigenous to France. This is the more remarkable, inasmuch as, though Arachnology has but few votaries in any country, yet England, Sweden, Prussia, and even Italy, have furnished more or less complete works on their respective spider-faunas. Looking again at the geographical position of France, perhaps few other equal areas would give such a promise of rich results to the araneologist; with all the advantages of an insular position, France combines those of the general Continent of Europe; and her climate ranges from the sub-arctic, in her mountain regions, to the semi-tropical on the Mediterranean shores. We may confidently, therefore, expect a vast addition to our knowledge of European spiders from the labours of the industrious author who has stepped into the breach, and whose first volume on the Spiders of France stands at the head of this notice.

As its title implies, the work is intended to embrace more than the one order (Araneidea) of Arachnids; certainly (it is understood) the orders Scorpionidea and Phalangidea; but whether it will extend also to the other orders, is yet undecided. The present volume, pp. 1-269, Pl. i. ii. iii., embraces five families of the order Araneidea (or Araneæ). It is a matter of regret that it had not been practicable to retain a systematic sequence in regard to the details of the order; the reason given for this is that the author has taken first those families of which he was in possession of the amplest materials; another drawback also seems to be, that the Introduction, "comprising general remarks on the class Arachnida and its bibliography," will not appear until later; when it will, however, be specially pagged for addition to the first volume. The volume before us begins with a useful glossary of special terms used in the descriptions; to this follow (pp. 5-15) some general remarks on the characters of the order ARANEÆ, and some criticisms on the more extended works of different authors upon it; concluding with the outlines of the classification adopted in the present work. In regard to classification but little alteration is proposed from that contained in a paper, "Aranéides nouveaux ou peu connus du Midi de l'Europe, 2<sup>e</sup> mémoire," by the author,\* published (according to the title-page of its author's presentation copies) in 1873, in "Mémoires de la Société Royale des Sciences de Liège."

For the principles of M. Simon's primary divisions of the Araneidea we are referred to the second memoir above mentioned; there, after giving his reasons for dissenting from the primary divisions adopted by Dr. Thorell in his work "On European Spiders," the author divides the Araneidea into four sub-orders:—1. THERAPHOSÆ; 2. GNAPHOSÆ; 3. ARANEÆ; 4. OCULATÆ. The sequence of these is reversed in the volume before us; the name of the third is changed to *Araneæ veraæ*, and of the fourth to *Araneæ oculatæ*. The addition to the name of the third order was necessitated by the adoption of the term Araneæ

\* This paper does not, however, appear yet to have been "published" in the only true acceptance of the term; that is, offered to the public for sale; and, it is understood, will not be so published until 1875. This is in some respects a matter of importance, inasmuch as the claim of many species and some genera to the names under which they are, or will be, characterised in the present work, rests for their priority upon the date of publication of the above paper in the Mém. Liège. Similar remarks apply to the 1<sup>er</sup> Mémoire on "Aranéides du Midi de l'Europe," the presentation copies of which were issued in 1870, while the volume containing it was not published until 1873.