THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

AUGUST 1858.

IX. *On certain Results of Magnetical Observations.* *By* John Allan Broun.

To Sir David Brewster.

Trevandrum Observatory, India,
December 21, 1857.

My dear Sir,

I HAVE just had a sight of your article on Magnetism in the new edition of the *Encyclopædia Britannica* (vol. xiv. part 1), and I have observed that you notice my results with reference to the lunar influence. Most of the results which were obtained by me are so mixed up with large masses of figures in the volumes of the Makerstoun Observations, that I was requested by some scientific friends, before leaving Europe, to publish a popular *résumé* of them. This I deferred from the desire to confirm and render them more general, if possible, by observations to be made near the magnetic equator. As I have now obtained some results from my observations here, you will perhaps allow me to offer you an account of some of those obtained by me in Scotland, and here near Cape Comorin.

It is not my wish to occupy your attention with the results I had obtained with reference to the lunar magnetic influence; but I may notice, as an evidence of how little is known of the Makerstoun Observations, the statement by General Sabine, "that Toronto is the first and only station at which the numerical values at every lunar hour of the lunar-diurnal variations of the three elements have been published" (Proceedings of the Royal Society, March 5, 1857).

The numerical values at every lunar hour during each lunation in the years 1844 and 1845 for the declination, horizontal force, and vertical force, were given in the Makerstoun Observations published in 1848 and 1850. Also the numerical values for the winter groups and summer groups of lunations for the magnetic inclination and total force were given at each lunar hour in the same volume. The results for the lunations of both years for the two components of force were projected and will be found in the Edinburgh Transactions, read in January and April 1846. General Sabine's investigation, which I have not yet seen, dates, I believe, in 1856.

Besides the results for these elements, I have given in the general results (published in 1850) the projections of the diurnal curve, due to lunar influence, described by the north end of a needle freely suspended in the direction of the magnetic dip. As I have not noted in the volume referred to the results deducible from this curve, I shall state them shortly here.

1st. The amount of motion due to lunar influence of a freely suspended needle is a maximum twice in a lunar day, namely during the hours before and after the moon's transit of the inferior meridian, and at the transit of the superior meridian. It is a minimum twice; a principal minimum six hours after the superior transit, and a secondary minimum six hours after the inferior transit.

2nd. The greatest amount of motion occurs while the moon is below the horizon. This result, if confirmed, is very curious, as it is wholly opposed to all that we know of the solar action.

3rd. The direction of motion is, on the whole, the opposite to that produced by the solar influence, from one hour after the moon's passage of the superior meridian to one hour after the passage of the inferior meridian; and it is the same as that due to the sun in the remaining twelve hours.

The following are the approximate angular movements of the needle due to the moon's action for different intervals, deduced from observations at Makerstoun during twenty-five lunations, 1844, 1845.

	h m		h m		′
Moon's inferior transit	12 0	to	13 55	Angular motion	0·16
	13 55	„	15 50		0·13
	15 50	„	17 45		0·04
	17 45	„	19 40		0·04
	19 40	„	21 35		0·07
Moon's superior transit	0 0	„	2 25		0·10
	2 25	„	4 20		0·08
	4 20	„	6 15		0·01
	6 15	„	8 10		0·07
	8 10	„	10 5		0·12
	10 5	„	12 0		0·08

I shall now refer to the laws of *magnetic disturbance*.

Diurnal law.—The fact that the movements of the declination-needle were most irregular in the evening was remarked by Cassini (and probably by earlier observers); but, as far as I am aware, the first careful investigation is due to M. Kreil. This physicist concluded that the perturbations were a maximum in the evening from 8^h to 10^h P.M., and a minimum in the morning from 8^h to 10^h A.M. The law for the disturbance of force was found to have different epochs. These conclusions were verified by different persons, and by myself to a certain extent.

I found that these results were on the whole due to a combination of different and even opposite laws; my conclusion was as follows:—

"It appears from these results that the diurnal law of mean disturbance is *not constant throughout the year*, as has been supposed; in fact, the law for the summer is nearly the *reverse* of that for winter, while that for autumn is nearly intermediate between the two" (Trans. Roy. Soc. Edinb. vol. xix. part 2, p. xxviii).

In 1856 I communicated the fact to Lieut.-Colonel Sykes and others, that the law of disturbance near the equator was the inverse of the so-called mean law for Europe; that is to say, near the equator the diurnal law of disturbance throughout the year resembles on the whole the law for Europe in summer, the maximum occurring about 10 A.M. and the minimum about 10 P.M. I have therefore not only given the diurnal law of disturbance for each season in Europe, but I have also found that this law varies both with season and latitude. These results refer to the *mean* disturbance without reference to sign. When the sign or direction of movement was considered, it was found that the maximum positive disturbance coincided on the whole with the minimum negative disturbance. This is not the case near the equator; *the law for both signs is the same.* It is probable, therefore, that for intermediate latitudes the relation of the law of signs will vary. We may expect in some intermediate latitude that the mean disturbance of declination will be equal at 10 A.M. and 10 P.M., and that the hour of maximum disturbance of force will occur gradually later after noon as we leave the equator and approach the poles.

Monthly laws of disturbance.—I found that the mean departure of the declination-needle from its monthly mean positions at the corresponding hours, was greatest two or three days after the moon's opposition, and was least about the time of conjunction (Trans. Roy. Soc. Edinb. vol. xix. part 2. p. xx). I also concluded that the diurnal range of the declination-needle was greatest when the moon was near the equator (Ibid. p. xix): similar laws were found for the components of force.

Annual laws of disturbance.—The mean departure of the declination-needle from the normal position for the corresponding hour was found by me to be greatest near the equinoxes, and least at the summer and winter solstices (Ibid. p. xxii). This law was published by me in 1846. At that time some investigations had been made to determine the law. M. Kreil and General Sabine had found the epoch of maximum disturbance to be in winter, and Dr. Lloyd had ascertained it to be in summer. General Sabine's result, it is true, was derived from disturbance above a certain arbitrary limit,—a fact, however, which I showed could not have affected the general conclusion. Since then General Sabine has verified on the whole my result by his own process.

I may add here, to complete these laws, that first indicated by Dr. Lamont, namely that the diurnal movement seems to have been a maximum in 1838 and 1848, and a minimum in 1843–44, or that the amount of the magnetic variation seems to obey a decennial law; a result which General Sabine has also found to exist in his discussion for the value of disturbance.

It may be seen from the results of the Makerstoun Observations (Trans. Roy. Soc. Edinb. vol. xix. part 2. pp. xii, xxxi, xlv), that the secular change seems on the whole to obey a similar law. This fact I noticed at the time (Ibid. No. 5. p. xii.); but as the changes were probably in part due to instrumental causes, especially in the first years of the series, I could not offer any general conclusion. An examination by me of observations made in other places, however, seems to confirm the results obtained at Makerstoun, which may be stated thus:—

The secular rate of increase of horizontal force was a maximum in 1843–44, and a minimum in 1847–48.

The secular rate of diminution of the vertical force was a minimum in 1848. As 1842 was the first year for which a result has been obtained, the epoch of maximum is uncertain.

The rate of yearly change of magnetic declination seemed to attain a maximum in 1847–48.

As already mentioned, the results in the first years (1842–44) were probably affected by instrumental errors; but this could not be the case in the latter years; and this connexion with the decennial period for three instruments cannot be accidental.

These results were evidently connected with the increasing amount of disturbance; and at first sight it might appear that the variation of the secular change was due to disturbance: thus the disturbance tends to diminish the value of the horizontal force, the secular change being a yearly increase of force; the greater the amount of disturbance, the less the yearly increase should appear. This relation, however, does not appear to apply either to the vertical force or to the declination; and I am induced to conclude that the variations of the secular change are not due to disturbance, though both apparently, obeying the same law, are due to the same cause. This is the first appearance of a law affecting the secular variations.

I have endeavoured to show that the frequency of the appearances of the aurora borealis obeys the same laws as the amount of the magnetic disturbance.

The general connexion betwixt the appearance of the aurora borealis and magnetic disturbances was known in the time of Graham, Celsius, and Wargentin. The fact that the needle was even disturbed sometimes on the approach of an aurora (that is to say, when invisible at the place of the needle), is noted by Mairan as a result obtained by Wargentin (Mairan, p. 450, 2nd edit.). It is to M. Arago that we owe a distinct statement, and a considerable amount of evidence of the fact, that when there is a magnetic disturbance in Europe, there is generally an aurora borealis visible in higher latitudes, if not visible at the place where the needle is disturbed.

My own observations proved that there was always an irregularity in the movement of the needle, even when the aurora was most feeble; in some cases slight disturbances caused me to search for the aurora and discover its existence, when otherwise it would probably never have been remarked. I have noticed several times in the Makerstoun Observations a curious phænomenon occurring with faint magnetic disturbance, which suddenly covered the whole sky as with a thin milky veil, that I have designated milky aurora. I never, however, observed a magnetic disturbance at Makerstoun without finding an aurora also visible, if the state of the sky was favourable. The following are the laws of aurora deduced from my own observations:—

Diurnal law.—The maximum frequency in the winter months occurs about 9 or 10 P.M. This fact is as old as the corresponding one for magnetic disturbance.

Monthly law.—As I lost no opportunity of searching for auroræ during the years I had the direction of Sir Thomas Brisbane's observatory, I conceived, if a monthly law existed, it might be deduced from my observations. I think I have shown strong reasons to conclude that the maximum frequency of the aurora occurs near full moon. It is curious that this is the result that Mairan finds should follow from his theory (Mairan, p. 280). This is the epoch I have found for the maximum disturbance also.

Annual law of variation of frequency of the Aurora Borealis.—This law, that the maximum frequency occurs at the equinoxes, has been attributed lately to M. Hansteen (Proc. Roy. Soc. vol. vii. p. 436). It was attributed by M. Kaemtz to Mairan.

An examination of the first edition of Mairan's work induced me to refuse the credit to that philosopher. M. Secchi, who has since examined the same edition as myself (1733), has repeated Mairan's claim. A careful perusal of the second edition of Mairan's work induces me still to believe that Mairan did not announce this fact distinctly in his first edition; but in the supplement to the second edition (1754), Mairan includes the observations of Celsius (1733), Frobesius (1739), Delisle (1738) and others, all of which show this law distinctly. Mairan then refers to it in the following manner:—"On a pu remarquer en jetant les yeux sur les sommes pour les mois de la Table composée No. 77, et de la plupart de celles qui la composent, que la fréquence des apparitions de l'aurore boréale autour des équinoxes ou environ un mois avant et après, est aussi grande et quelquefois plus grande qu'autour du périhélie; ce qui paroît dit-on, infirmer les inductions que nous avons tirées de la fréquence du phénomène autour du périhélie." (De Mairan, *Traité de l'Aurore Boréale*, 2nd edit. 1754, p. 532.)

The law, therefore, is as old as the time of Mairan, whether its clear enunciation be due to that philosopher or not. The law has been verified by every moderately long series of observations since then, and latterly by M. Hansteen and by myself. It coincides with the annual law of magnetic disturbance found by me.

According to these results, I think we might perhaps be allowed to suppose that for every law of magnetic disturbance there is a corresponding law for the aurora borealis.

In this case we might conclude that the number of auroræ seen in summer in Europe is much less than it ought to be, not only because of the strong twilight, but also because the evening before midnight is the epoch of minimum disturbance, and therefore probably the epoch of minimum intensity and visibility of the aurora.

Further, the decennial law of disturbance should also have its corresponding law of aurora; and if we could prove that previously the decennial law of frequency of aurora did not exist, we might perhaps conclude that the decennial law of disturbance is not continuous. On examining the numbers of auroræ collected by Mairan from all sources, I find that we can draw no conclusion till the year 1699; before that time two or three yearly were all that had been registered. In 1699, however, we find a large number (40); 1719, again, is in the middle of a group of years, when an increase commences, which, however, continues till 1732. If we take the means of every three years to eliminate accidental variations, there is evidence of nothing but a gradual increase of frequency from *one* in 1710, to *ninety* in

1791, and diminution thence to *four* in 1746. The decennial period is certainly not marked in that part of the last century.

The mean diurnal variation of magnetic declination is, however, a maximum, both by Cassini's and Gilpin's observations, in 1787. It is perhaps worth noting the results I have been able to find on this question; they are as follow:—

1759*	10′·7	1792	9′·3	1810–11	8′·1	1829	13′·7
1771–73	7·2	1793	8·4	* *		1830	12·4
1778–80	9·2	1794	7·4	1817	8·1	1831	11·8
1784	9·6	1795	6·9	1818	8·9	* *	
1785	10·8	1796	7·1	1819	8·8	1834	8·2
1786 {	13·9	1797	7·4	1820 {	7·8	1835	10·0
	14·5	1798	7·2		10·2	1836	12·9
1787 {	15·1	1799	7·1	1821	9·1	1837	12·3
	15·0	1800	6·7	1822	8·8	1838	12·2
1788 {	13·7	1801	7·6	1823	8·2	1839	11·0
	14·4	1802	8·0	1824	8·2	1840	9·5
1789	12·0	1803	8·9	1825	9·0	1841	8·5
1790	11·9	1804	8·0	1826	9·8	1842	7·6
1791	11·8	**		1827	11·8	1843	7·6
				1828	11·5	1844	7·4

There are evidences in this list of a maximum diurnal oscillation in 1787, 1829 and 1837: 1847 or 1848 was also an epoch of maximum. The mean period is therefore about 10 years; the interval from 1829 till 1847 gives about 9 years; but the interval 1787 to 1847, or 60 years, may either give six periods of 10 years, or seven periods of 8·6 years.

It would appear that the maximum is not always exhibited; thus it does not appear at all in the period after 1787; on the contrary, there appears a tendency to a maximum in 1803. Again, the maximum in 1829 gives an interval of less than 8 years to the maximum in 1836–37; the next interval is, however, upwards of 10 years, as the following will probably be found to be. If we were to assume the interval as 885 years, the period of a revolution of the lunar apsides, we should have had the maximum in the following years, counting back from 1848, 1839, 1830–32, 1821–24, 1812–15, 1803–07, 1794–99, 1786–90. The exact period is therefore not quite certain†.

* 1759, Canton, London. 1771–73, Van Swinden, Francker. 1778–80, Cotte, Montmorency. 1784–88, Cassini, Paris. 1786–1804, Gilpin, London. 1810–11, Bowdich, Salem, U.S. 1817–20, Beaufoy, London. 1820–34, Arago, Paris. 1835–44, Gauss, Göttingen, and Lamont, Munich. These ranges are not quite comparable, as in a few cases the range is that of the mean oscillation, and they should vary slightly at the different places. The ranges from Gilpin's observations are derived from two to twelve months' observations in each year; but where the observations were not made in all the months of the year, a correction has been applied to obtain the range for the year.

† It is not a little curious, that the observations of M. Coulvier Gravier

I shall now offer you some remarks on the diurnal variation of the magnetic declination.

In a paper on this subject, read before the Royal Society of Edinburgh, May 3, 1847, while showing that the movement of the needle relative to midnight was of the same character as that relative to noon, I suggested that the apparent smallness of the night maximum might be due to the variation being a resultant of two opposite motions; one, the greatest, having its maximum near noon and its minimum near midnight; the other, the smaller, having its maximum near midnight and its minimum near noon. This supposition I illustrated by the superposition of two curves.

I have also shown in the Makerstoun Observations for 1844 (Trans. Roy. Soc. Edinb. vol. xviii. p. 354), that if we subtract the ordinates of the curve representing the diurnal movement of the needle for the northern hemisphere (employing the curve for summer at Makerstoun for this purpose) from the curve for the winter at the same place, the differences will be the ordinates of a curve representing the movement of the needle in the southern hemisphere.

The conclusion I drew from this fact was, that the reversal of movement with the sun north and south of the equator observed between the tropics was also shown in high latitudes; and that the midwinter curve at Makerstoun might be considered due to the superposition of the two opposite curves representing the movements in high north and south latitudes. M. Secchi has since then made a similar proposition with the same object, employing preferably the mean of the year as the typical movement (Monthly Notices, Roy. Ast. Soc. vol. xv. p. 27). I have little doubt still that the hypothesis of superposed opposed movements may be employed to obtain all the variations observed in different places at the same time, and in the same place at different times.

When M. Arago wrote his conclusion that there should be a line of no diurnal range for the declination-needle, the following were the facts in his possession. In the northern hemisphere, the north end of a needle moves towards the west from the morning till afternoon, and towards the east after noon till the evening, with a secondary oscillation at night.

In the southern hemisphere, if we consider the north end of the needle, its movements are in the opposite directions for the same periods of the day.

show a maximum in the number of meteors on the 9th, 10th, and 11th of August for the year 1848. The number increases from 1845, the time of commencement of his observations, and diminishes till 1854.—*Ann. de Chim.* January 1855, p. 46.

In both hemispheres the diurnal range varies with the sun's declination, being greatest in the northern hemisphere, on the whole, when the sun is furthest north, at which time it is least in the southern hemisphere; and least in the northern hemisphere when the sun is furthest south, the diurnal range being then greatest in the southern hemisphere.

The conclusion that M. Arago drew from these facts was, that there must be a line on the earth's surface where in the morning the needle moves neither to the east nor to the west, that is to say, remains stationary. This conclusion is evidently too general, and so far inaccurate. The legitimate conclusion would have been, that if there was a period of the year for which the two movements were equal in amount in two hemispheres, then there should be a line of separation where for that period the needle would rest stationary. Even this conclusion assumes not only that the amounts were equal, but that the law of the movements was exactly the same with reference to the two poles. It is not a little curious that M. Arago has cited* the "dit avoir observer" of M. d'Abbadie, "que la variation diurne de déclinaison a complétement changé à Fernambuco du moment où le soleil a passé d'un côté du zénith à l'autre," as a confirmation of his conclusion, while General Sabine has pointed out the same result from the St. Helena observations as proof of its inaccuracy. There can be no doubt that the statement of M. d'Abbadie gives no evidence of a line of no movement. The facts seem to have disproved M. Arago's supposition. The movement, it is concluded, is inverted; but it does not pass from one form to the other through a period of no movement, but rather by a sliding transfer of the epochs about the period of the equinoxes.

I think that the only conclusion that could have been deduced from the facts long known is the true one; namely, that for a given position of the sun there should be a line of no movement, if the movements in the two hemispheres follow the law of opposite directions, and equal amounts for similar positions of the needle and the sun. This proviso is not strictly true; and therefore, instead of a line of no movement for a given position of the sun, we should substitute a line of *minimum* movement. St. Helena is too far from both the terrestrial and the magnetic equators (especially from the latter) to show this result clearly; but the observations here (in $8\frac{1}{2}°$ N. lat. 2° south dip) will, I think, prove this, and render it probable that there are at least points of no diurnal movement for one position of the sun (probably near the nodes of the magnetic equator). The following are my conclusions as to the range of the diurnal oscillation.

The mean diurnal range of magnetic declination is a minimum

* *Notices Scientifiques*, vol. i. p. 491 (1854).

in high north latitudes in December; as we proceed south, it will be found, when we approach the tropics, that December is no longer the month of minimum, but the months of November and January or February; further south, near the magnetic equator, March and October are the months of least diurnal range; still further south, near the tropic of Capricorn, April or May, and September or August; and finally, June and July in high south latitudes. This curious shift of the epoch of minimum has been hitherto unknown.

The months of maximum oscillation are before and after the summer solstice in high north latitudes; before and after the winter solstice in high south latitudes; and in August and January near the magnetic equator at Trevandrum.

The amount of the daily range therefore has no relation to temperature, as indeed I have already shown from the observations at Makerstoun alone.

According, then, to the conclusion that I have shown might be drawn from the facts previously known, if all things were equal for both hemispheres, we might expect for some one position of the sun and of the needle that the sums of the positive and negative forces producing the lateral movements would be zero. If, however, from any cause (such as the unequal and unsymmetrical distribution of magnetism on the two hemispheres) the two laws should not be exactly similar, the result should at least be a line of minimum movement. The Trevandrum results will confirm this supposition. The following are the ranges of the mean diurnal variation for the months when the range is a maximum and minimum:—

	1854.	1855.	1856.	1857.
January	2·05	2·08	1·90	2·32
March	0·57	0·63	0·85	0·97
August	3·22	3·13	3·20	3·12
October	1·36	1·05	1·08	0·99

Thus the movements for August and January differ little from those of the *free* needle in Europe for the same months, while those for March and October are only about one-fourth of the movements of the free needle in Europe in the same months.

It will be remarked that the small range of the mean diurnal oscillation for March and October might be wholly due to the movement changing sign in the course of the month, without any diminution of the daily range. This, however, is not the case; the range diminishes from January to February, and from February to March. The range in some days of March, it is true, is frequently above 2′; but this is evidently due to disturbance, which is a maximum in that month. The real diminution of range, however, is shown quite distinctly when we take the means for a week: thus all the oscillations of the declination-needle in the week March 20 to 25, 1854, were comprised within limits of 2′·5; and all the oscillations during the three days, March 23, 24, 25, within the limits of 1′·3. The tendency to an extinction of the diurnal movement is therefore certain, since these limits include all the oscillations due to irregular causes: the effect of disturbance is shown when we take the mean hourly positions for a few days, as in the week March 20 to 25, for which the range of the mean variation was only 0′·49.

It is probable that the line of minimum movement for the sun near the equator is the magnetic line of no-dip, where the free needle rests nearly parallel to the axis of the earth; and, as already suggested, that the points of minimum movement on this line are those where the terrestrial and magnetic latitude are zero. It seems probable, from other considerations, that the minimum movement does not occur simultaneously over the whole line of no-dip, but that the time of minimum varies somewhat with the latitude.

Before I leave this subject I wish to draw your attention to a curious, and I think important fact, deducible from the ranges I have given. You will observe that the range in the month of March increases from 1854 till 1857; so that if March 1858 also shows a slight increase, the range for that month will be nearly doubled within four years. January also shows some increase in 1857: not so for the months of October and August; the range for these months is either constant, or appears to diminish slightly. The relation of the increase in March* to the decennial period is evident; but what shall we say of October and August? The decennial period only affects certain months at the equator *in the period* 1854–58. When the longitude of the sun was 0° and 270°, the increase was marked, and not at all when it was 180° and 90° *in the semi-period* 1854–58. I believe that other months show the increase in other periods.

I shall not at present enter into the laws of the variations of magnetic dip, nor of the force. I may say, generally, that the diurnal variation of the magnetic dip near the magnetic equator is greatest near the *equinoxes* when the variation of declination is least, and that it is as great in June and greater in December than in Europe. On the whole, therefore, the variations of dip are greater on the magnetic equator than in Europe.

The mean diurnal motion of the magnet freely suspended in the direction of the magnetic dip near the equator is as great as, or greater than, that in high latitudes in every month of the year.

* An increase in March might be produced by a variation of the epoch of minimum movement.

I would also refer to a curious fact with reference to the diurnal law of variation of total force, which I have never seen noticed, or indicated as remarkable by any physicist; on the contrary, it seems to have been so little marked, that Dr. Faraday, in his paper on atmospheric magnetism, has attempted to explain what is exactly the reverse of the fact (Phil. Trans. 1851, p. 115). Whether this is due to the doubt as to the accuracy of the temperature correction, or the effects of local influences, I cannot say; but as the diurnal variation of temperature for the instrument which indicates the variations of intensity in this observatory is less than three-tenths of a degree Fahrenheit, there can be no doubt here as to the fact that the total intensity is a maximum near 11 A.M., the time when it is a minimum in high latitudes. The fact is shown in all the intertropical observations, and was remarked many years ago by the late Mr. Caldecott in this observatory.

Of course the epoch of maximum intensity varies as we proceed northward and southward. In Scotland it occurs latest in summer and winter (6 P.M.), and earliest in autumn and spring, or when the sun is near the equator.

I shall at present only note the fact of the coincidence, in the inversion of the European mean laws of disturbance and total intensity at the equator, the maximum force and maximum disturbance of force occurring at the same times.

To enter in detail into the annual laws would occupy too much space, I shall therefore limit myself to a few brief observations.

Annual law of Magnetic Declination.—I would suggest to magneticians in Europe or elsewhere, before they attempt to determine this law, to observe the annual law of movement of an *unmagnetic* weight (or of a weight with a weak magnet attached) suspended by a silk thread like the declination-magnet. I believe they will thus find the cause of many curious differences between the results at different places, and probably why the annual law seems to depend on temperature. Cassini's observations, which have been so frequently cited, are I think worthless for this determination, as it seemed to me was proved in a paper, before referred to, read to the Royal Society of Edinburgh in 1847. I have found here that a simple weight has an annual and diurnal law of movement of its own, whether suspended by a silk thread or metallic wire, and that in a climate where the meteorological conditions are greatly more constant than in Europe.

Annual law of Force.—You are aware that I have deduced from the Makerstoun Observations, as early as 1845, that the horizontal force was a maximum near the solstices, and a minimum near the equinoxes. This law was obtained from *each* of several successive years' observations, and I confirmed it by an examination of the observations at Munich and Toronto. Another period obtained by General Sabine I showed was due to error in the temperature coefficient (Trans. Roy. Soc. Edinb. vol. xvi. p. 102).

To this result I have now to add, that the relative changes of mean horizontal force are the same over the globe, and the changes from day to day of the mean horizontal force at different places on the earth's surface are nearly equal, the unit in each case being the whole value of the horizontal force at the place. In other words, the changes of mean horizontal force, from day to day, are in the same direction over the globe, and are proportional to the horizontal forces at the places; the different effect of disturbance, due to its diurnal period, and the different directions of the secular change, being allowed for.

The preceding and other facts, which I shall shortly endeavour to prove in detailed memoirs, have led me to consider anew the different hypotheses offered to explain the magnetic variations. I had already satisfied myself, from the Makerstoun Observations, that the theories depending on temperature were wholly insufficient. I showed that the areas of the declination-curves did not follow the law of temperature, as supposed by some physicists. I also showed that such areas were not the proper elements in comparison for such a purpose. If any areas were employed, it should be the areas of the curve described by the needle freely suspended in the direction of the magnetic force.

The results I have now given prove, it appears to me, the insufficiency of any theory that attempts to relate the amount of the diurnal movement to the temperature of the place. March is with us here nearly the hottest month of the year, whereas August is about the coolest; yet the former is the month of the minimum, and the latter the month of maximum diurnal movement of the declination-magnet.

None of the facts previously known seem to have been sufficiently conclusive on this point, since Dr. Faraday has proposed a new theory which depends on the temperature of the sun, acting on our magnetic atmosphere. There can be no difference of opinion as to the great ingenuity displayed by Dr. Faraday in his attempts to make his theory explain the results of observations. No doubt many of his applications are not very clear to me; thus he explains the different motions of the dipping-needle at St. Helena and Hobarton by the statement, "that as the region is located above in the air, it is above the angle which the dip makes with the horizon at St. Helena, and therefore ought to depress the line of force and lessen the dip. At Hobarton, the region being in the tropical parts, is within the angle formed by the line of dip with the horizon, and therefore deflects the lines of force upwards and increases the dip" (Phil. Trans. p. 112, 1851).

According to Dr. Faraday's hypothesis, then, the variation of dip should be zero where the dip is zero. The law of diurnal variation ought to be opposite on opposite sides of the magnetic equator, and different on the *north* side of the magnetic equator, within the tropics, from that where the dip equals that at Hobarton (at Makerstoun, for example) and "the region is within the angle formed by the line of dip with the horizon." Now *none* of these conclusions are true.

There ought also to be some test in this place. Let us take the month of March for instance. The sun is nearly vertical; there has scarcely been a cloud seen since December; the temperature is near its maximum; the soil is red, and heated gusts of air pass as if escaped from a furnace. In October, on the other hand, the temperature is a minimum; the sky has been overcast for months, one monsoon has passed and shed its rain in deluges, and another is in full force. The lower stratum of air, a mile deep, is sheltered from the direct rays of the sun, and there is an atmospheric shell in the condition of paramagnetic polarity: ought there not to be a marked difference in the variations during these two months? and yet there is little or none.

I have now, in correspondence with his Highness the Rajah of Travancore's observatory here, another observatory on the highest and sharpest peak of our Ghats, supported also by His Highness the Rajah. You will see from the report published, of which I have sent you a copy, an account of the erection of this observatory. It is exposed for some months to the sun that is burning up the eastern side of the Peninsula, while the western side, Travancore, is covered by a sea of vapour. I have a series of observations, magnetical and meteorological, made on this peak hourly during upwards of two years. From these observations I believe I shall be able to add further evidence of the complete *insufficiency* of any temperature theory in accounting for the magnetic variations.

The following I shall offer partly in the form of questions:—

Does not the sun act as a magnet, perhaps as an electro-magnet, the currents forming it being within its atmosphere? Are not the solar spots disruptions of the current due to the positions of the planets with reference to the plane of its equator? After a magnetic disturbance there is a diminution of force shown on the earth, which remains for some days as if there had been a violent action with the result of a loss of energy. The connexion of the solar spots with the frequency of the aurora borealis (and therefore, as we now know, with the magnetic disturbance) was remarked by Mairan (*Traité*, &c. p. 264, 2nd edit.), but it appeared to him that the coincidence then remarked had not always existed.

Having found that the magnetic disturbance was greatest not only when the sun was on the equator, but also when the moon was on the equator, I was induced to compare the times when the lines of the moon's nodes and of the apsides would nearly agree with each other, and the position of the sun at the equinoxes. I found that the longitude of the moon's ascending node was 0° in 1838 and 1857, while it was 180° in 1848; and that the moon was in perigee when its longitude was 0° in the same years, which were those of maximum disturbance. The times of revolution of the nodes and perigee are well known to be about 19 and 9 years respectively.

If the coincidence of the decennial laws of frequency of the solar spots and of magnetic disturbance be not accidental, we should have a correspondence between the laws of the aurora and of the solar spots. Do M. Schwabe's observations show maxima of frequency at the equinoxes? If so, we must conclude that the earth has something to do in the formation of the solar spots, and therefore probably other planets, which would introduce other periods. Jupiter, whose revolution occupies about twelve years, was in longitudes of about 180° in the years 1826, 1838, and 1850.

It is not the case that the magnetic disturbance coexists always with the spots; but it is not improbable that during *the formation* of the spots the disturbance is produced, that is to say, at the period when the supposed *discharge* of the sun's electrical atmosphere occurs.

If the sun acts as a magnet, how does it produce the magnetic variations? It is probable that it acts directly on the suspended magnet, but that this action is *quite secondary*. The diurnal variations would be produced by a shifting of all the lines of equal declination, inclination, and force usually represented as due to the action of the terrestrial magnet. Are not the magnetic variations, then, due to the shifting of these lines, by the inducing action of the solar magnet, the direction of the shift and of the lines determining the epochs of maximum and minimum?

That the sun might act as a magnet is of course not a new idea. It had occurred to myself whilst investigating the lunar magnetic influence, twelve years ago, that some of the results obtained might be due to the sun's rotation on his axis; a similar idea occurred to M. Kreil, but an examination of the results seemed, in both cases, to prove the inaccuracy of the supposition. The results at which I have arrived lately, however, have induced me to examine the question more carefully. If the sun acts as a magnet, it is probable, from the analogy of our earth, that its magnetic poles will not coincide with the poles of rotation; perhaps even the poles may have unequal forces. In such a case, it might be expected that the fact could be determined by our magnetic observations. It happens, however, that the period of the synodal rotation of the sun (employing the results obtained by Bianchi and Laugier, and given by Sir John Herschel as the best determination, namely $25^d\ 7^h\ 48^m$*) is exactly that of a nodal revolution of the moon. It might therefore be difficult to determine whether any result obtained was not due to the moon's varying position relatively to the plane of the equator.

The result which I have now obtained from three years' observations near the magnetic equator, it appears to me, is wholly independent of the moon, and is due to the sun's rotation on its axis.

If we could suppose that the solar magnetic poles are fixed, it might then be possible to determine accurately the time of the sun's rotation by means of the movement of our magnets. If, on the other hand, the poles are in motion, as I conceive they are, we shall have to employ another period than $25^d{\cdot}325$, as obtained from the solar spots. The period to be employed will of course be found by careful examination of the observations, and by trial. Such a movement of the solar poles might explain the secular magnetic variations.

It is not improbable, after all, that *some* of the results obtained previously, as due to the moon, are really due to the sun's rotation.

Supposing that I have proved the sun's magnetic action, it has occurred to me that the fact might be applied to give some ideas for a theory of comets. Sir John Herschel, I believe, has somewhere suggested electricity as the cause which directs the tails of comets. I have looked over the different hypotheses given by M. Arago in the recent edition of his works, where he professes to omit none; but no polar hypothesis is to be found there.

Are not comets formed of magnetic gases? Is not the tail of the comet due to the directive action of the solar magnet, the curvature of the tail, sometimes seen, being due to the position of the solar magnetic poles relatively to the path of the comet? Is not the condensation of the comet, when approaching the sun, a phænomenon similar to those observed by Dr. Faraday and M. Plücker in their recent researches on the action of the poles of a magnet on certain gases or liquids? might not a like illustration be given of the varying form of the tufts in the nucleus†?

* Outlines of Astronomy, p. 232.

† Since this was written I find that Bessel considered that the vibrations of the tail of Halley's comet, observed by him, indicated the action of a polar force. Arago has made no mention of this hypothesis, apparently because he doubted the results on which it was founded (*Astronomie Populaire*, vol. ii. p. 396).

Is not the zodiacal light the magnetic æther in a luminous state, repelled by the solar magnetic poles? Does not the zodiacal light revolve round the sun? If so, what is its period of revolution? Are not the extent and intensity of the zodiacal light related to the periods of the spots, as Cassini and Mairan supposed*?

From the known action of the sun on the gases of comets, may we not infer some action of the sun on the gases forming our own atmosphere?

I shall not enter here into the reasons that I can produce, to show that the diurnal oscillations of the barometer do not depend on the diurnal variations of temperature as their chief cause; neither can it be explained by M. Dove's ingenious addition of the variation of the pressure of aqueous vapour, which is purely a local phænomenon. I think I have shown the insufficiency of the theory founded on the combination of these causes, in the Makerstoun Observations: it is insufficient both in Europe and India. We require a cause like that of the solar and lunar attraction on the mass of the ocean to produce a double tide daily. May not the facts known exhibit such a cause†? Should not the sun acting as a magnet on the magnetic gases forming our atmosphere, and by induction on the terrestrial magnet, cause the atmosphere to assume an ellipsoidal form, having the greater axis in or near the plane of the equator; thus determining the greater diurnal oscillation in the equatorial regions. If the form which the atmosphere assumed, under the influence of the terrestrial and solar magnets, were somewhat irregular (as in some of the figures assumed by magnetic liquids between the poles of a magnet, as in M. Plücker's experiments), we might explain in this way the diminished mean atmospheric pressure near the equator, and the maximum pressure 20° north and south of it. I am not aware that Dr. Faraday or M. Becquerel has determined the specific magnetism of air containing vapour in the form of cloud. Is not the specific magnetism of air diminished by containing aqueous vapour? and is not the diminution proportional to the *relative* humidity of the air, rather than to the absolute amount of watery vapour? If so, the diurnal oscillation of the barometer should diminish *ceteris paribus* with the relative humidity of the air‡.

* It seems to me that the zodiacal light is much brighter and longer this year than I have remarked it during the last five years. This impression, however, is not founded on real measures, as I had made none in the preceding years.

† Mr. Joule has found that in magnetizing an iron bar it is lengthened (without change of volume) in the direction of the magnetic axis.

‡ I have remarked, in several of the results of the magnetic observations made in Scotland, the coincidences with results for the atmospheric press-

The preceding facts and queries were addressed to you two months before this date; the letter was then shown to different persons, and among others to Lieut.-General Cullen the British resident here, with whom you are no doubt acquainted as a zealous inquirer in geology and terrestrial physics for the last half century in India. I had also communicated my results and the greater part of my guesses to that gentleman before.

I am, my dear Sir,
Yours very truly,
JOHN ALLAN BROUN.

P.S.—Since this letter was written I have seen in the *Bibliothèque Universelle de Genève*, July 1857, a translation of a memoir by M. Secchi on the periodic variations of terrestrial magnetism. This is a continuation of a former paper translated in the Philosophical Magazine for November 1854 and June 1855, and it draws from me a correction and a few remarks.

In his second paper, M. Secchi offers some laws of the movements of a needle freely suspended in the direction of dip. These movements were obtained by me first, I believe, from all the three magnetic instruments, and exhibited to the British Association in 1846; the curves produced by the movement of the north end of the needle were given for each month of the year, in the volume of Makerstoun Observations which M. Secchi has consulted; yet he says, "Mr. Broun has given analogous curves for Makerstoun, and a glance at these will show the same law, though somewhat more complicated from having grouped too many months together, and from the higher latitude and frequent disturbances." M. Secchi has examined plate 8 instead of plate 7, which contains the curves for each month (Trans. Royal Soc. Edinb. vol. xix. p. ii.). In plate 8 the figures are given for periods of 60 and 90 instead of 30 days; but this combination can scarcely be said to complicate the curves, as an examination of plate 7 will show. In plate 8, however, I have also projected the curves obtained from *undisturbed* movements, and have thus shown the effect of disturbance in displacing the curves. Various conclusions deduced from these figures are given in pages lxx–lxxiv; they are the only ones of the kind that have been projected, as far as I know, till M. Secchi has now constructed one for the whole year (*Bibliothèque de Genève*, p. 164). This curve, of course, may be considered complicated,

ure; and I have noted a resemblance between the annual law of *difference* of atmospheric pressure at Makerstoun and Greenwich, and the annual law of horizontal magnetic force. Do not the differences of the atmospheric pressures at the same two places, from year to year, show some resemblance to the decennial law, 1843–44 being an epoch of minimum? (Trans. Roy. Soc. Edinb. vol. xix. part 2. p. xci.)

since the figure deduced from the mean of 365 days cannot be said to resemble that for any day of the year.

M. Secchi deduces from his curve the following conclusions:—

"*Second Law.*—The disturbed curve is the ordinary curve entirely displaced by a certain quantity." A glance at the Makerstoun curves (plate 8) will show that this conclusion is inexact. Doubtless there is a displacement, and, as the effect of disturbance is small compared with the whole movement, the curve cannot be greatly changed in form; but the change of figure is quite as great as the amount of perturbation admits. M. Secchi also finds—

"*Third Law.*—By the effect of the disturbances, the curve tends always to become more symmetrical and equal in its two lobes."

Again, it appears to me that a glance at plate 8 will show that this conclusion is not warranted. It seems to me, on the contrary, that the effect of the disturbance is to render the figures *less* symmetrical, the curves for March and April perhaps excepted. The tendency of the disturbance is to introduce a *loop* in the easterly or night movement; and this is so evident that it seems probable, if we could obtain curves perfectly free from disturbance, the loop to the east in the night would not exist.

M. Secchi adds, "These conclusions, deduced from the observations of the three magnetic elements at Toronto, will be verified without doubt in other countries for which the discussion has not yet been performed." I refer to the Makerstoun Observations, where the discussion was published eight years ago, and where all the details will be found. With reference to his fourth law, given in a former part of this letter, namely, that "the disturbances are a maximum at the equinoxes, and a minimum at the solstices," M. Secchi remarks, "2nd. At the solstices, the poles of the sun are turned towards the earth, so that its magnetic action should be more powerful." Solstices must have been written by mistake for equinoxes, since it is near the latter periods that the poles of the sun are turned towards the earth; but the conclusion is a statement of the result obtained by me, and referred to previously under the head of "Annual law of Force," on the supposition that the north pole of the sun contains the same magnetism as the north pole of our earth.—J. A. B.

2nd P.S. I should have mentioned a fact in connexion with the history of the secular movements that I have not yet published. The magnetic declination is nearly half a degree east at Trevandrum. Before the year 1854, the movement was towards the north, that is, *westerly*. I have found that it began to turn easterly in the year 1854, and that the rate of motion yearly towards the east is becoming greater and greater.—J. A. B.

X. *Note on the Sun's Spots.* *By* Daniel Vaughan, *Esq.**

IN my article published in the May Number of the Philosophical Magazine, I endeavoured to show that the heating and illuminating action of suns, if maintained by the ætherial contents of space, can fluctuate only through the influence of surrounding worlds. But, though I offered an explanation of the periodicity of the solar spots, I deem it advisable to point out the precise manner in which they may arise from the causes to which I have ascribed them, and thus to furnish a more satisfactory basis for the tests of observation.

It appears, as a legitimate deduction from my theory on solar light, that the dark spots must be deficient in number, or entirely absent, when the sun is advancing into the realms of more dense æther; and that they must be most numerous and extensive when he is departing from these localities and entering a more rarefied ætherial fluid. In the first case, his permanent atmosphere, which takes no part in the emission of light, must be heated most intensely in its uppermost strata, and thus brought into a state of the greatest security from disturbances. But when the supply of ætherial fuel from space gradually diminishes, and heat declines near the surface of the vast ocean of incombustible gases, the elevated temperature previously transmitted to great depths would produce the most violent commotions of its aëriform matter. To such storms or swellings in the sun's non-luminous envelope we may ascribe his spots. The transparency of this gaseous appendage, together with the effects of flame in intercepting the range of vision, will account for the fact that the spots appear as hollows in the sun's disc.

If an astronomer, situated at the moon, were engaged in a telescopic survey of the earth, he could distinguish volcanic eruptions from many of our most violent tornadoes only by observing how the latter changed their position with respect to other terrestrial objects. It is only by a similar test that we can decide whether the sun's spots are to be regarded as indications of great storms in his atmosphere, or of the emission of gaseous fluids from his internal regions. The evidence which observation has hitherto afforded seems favourable to the first hypothesis. But it must be extremely difficult to form any correct ideas of the changes which occur beneath the sun's surface, from the effects of a violent heat in volatilizing his materials, while the influence of an enormous pressure is exerted in maintaining them in a solid or a liquid condition.

Cincinnati, Ohio, June 25, 1858.

* Communicated by the Author.