

XII.—*Results of the Makerstoun Observations, No. I. On the relation of the Variations of the Horizontal Intensity of the Earth's Magnetism to the Solar and Lunar Periods. With Two Plates. By J. A. BROWN, Esq. Communicated by Sir T. M. BRISBANE, Bart.*

(Read January 5. 1846.)

1. THE following communication is intended to be the first of a series, in which I propose to consider the results of observations made at Makerstoun, near Kelso, Roxburghshire, in the Observatory of the President of this Society. These observations, and the tabular results, will be found ultimately in the volumes of Makerstoun Observations, constituting volumes of the Society's Transactions.

2. It has been found convenient to separate the observations of the varying intensity of the earth's magnetism into two parts, namely, its resolved components in the horizontal and vertical planes. I shall treat at present of the variations of the horizontal component. These variations are observed by means of the bifilar magnetometer, an instrument devised by M. GAUSS, and modified by Dr LLOYD, described in the Introduction to the Makerstoun Observations for 1842. It consists simply of a magnetic bar, suspended by two silver wires, the latter being twisted out of a vertical plane, the magnet is forced from the magnetic meridian; the variations of its position afterwards are due to two causes, namely, variations of the horizontal component of the earth's magnetic force, or of the moment of free magnetism of the bar; the former are due to changes of the total force or of its dip, the ordinary variations of the latter are due to temperature; and it is, accordingly, a point of much importance to determine the correction for temperature with accuracy, in order that the simple effect of varying intensity may be obtained. I have pointed out, in a paper read before this Society last session, the imperfections of the method usually adopted for the determination of this correction, and the method which has been adopted for the correction of the Makerstoun Observations. I shall afterwards exhibit an example of the very different results to be deduced, after correcting observations by the two methods (11).

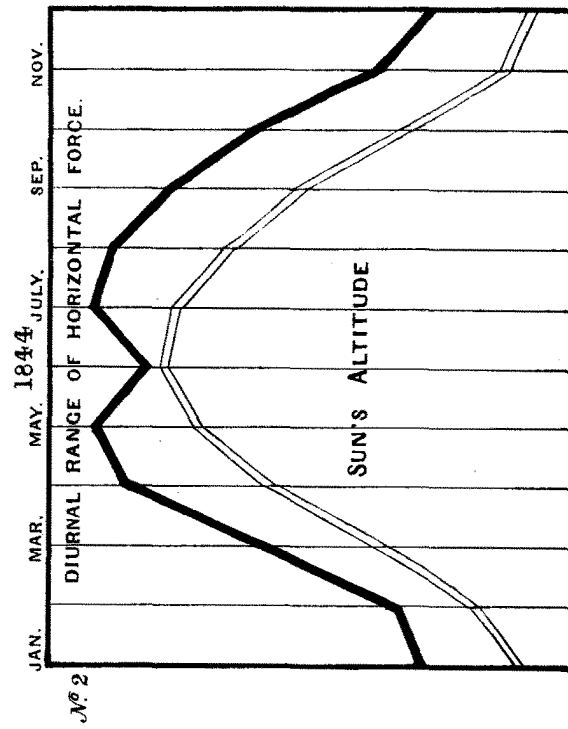
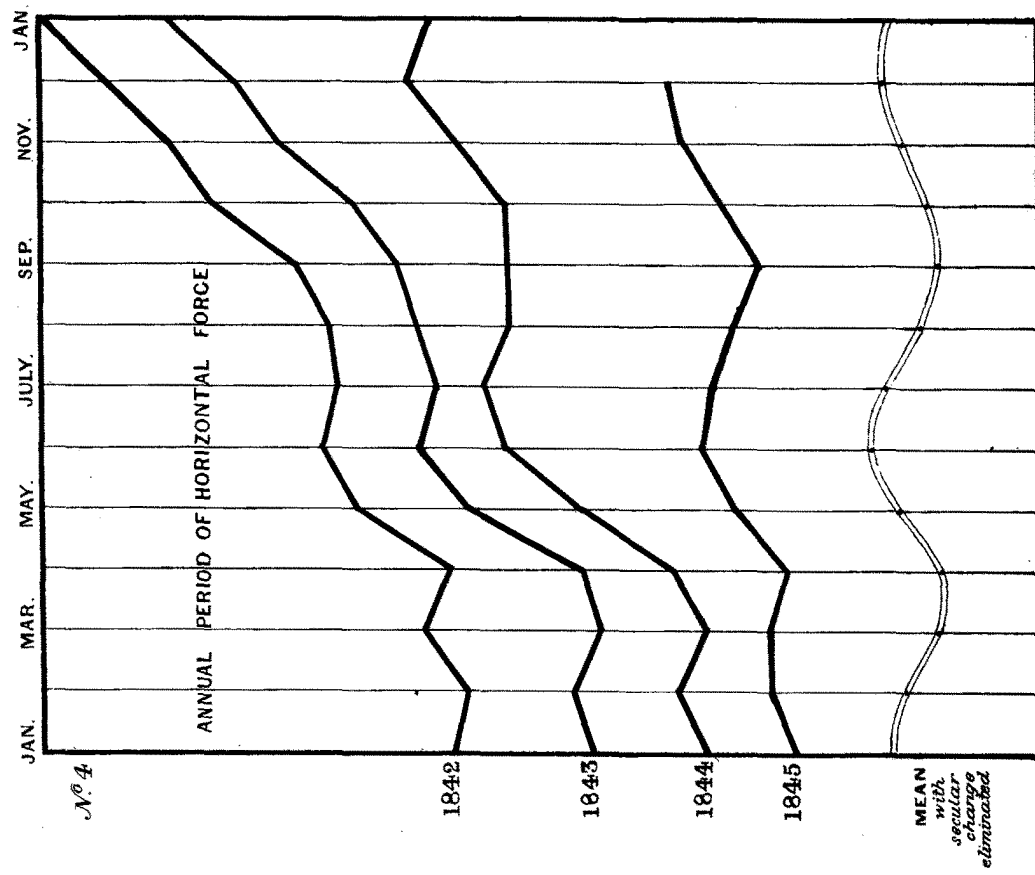
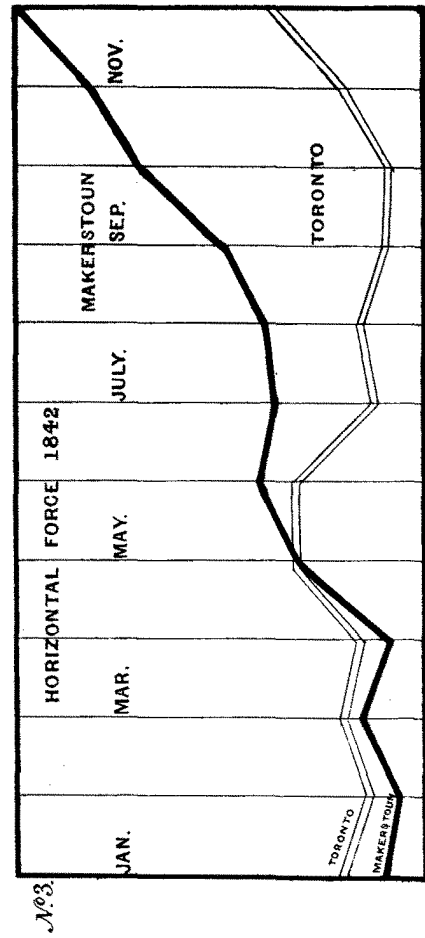
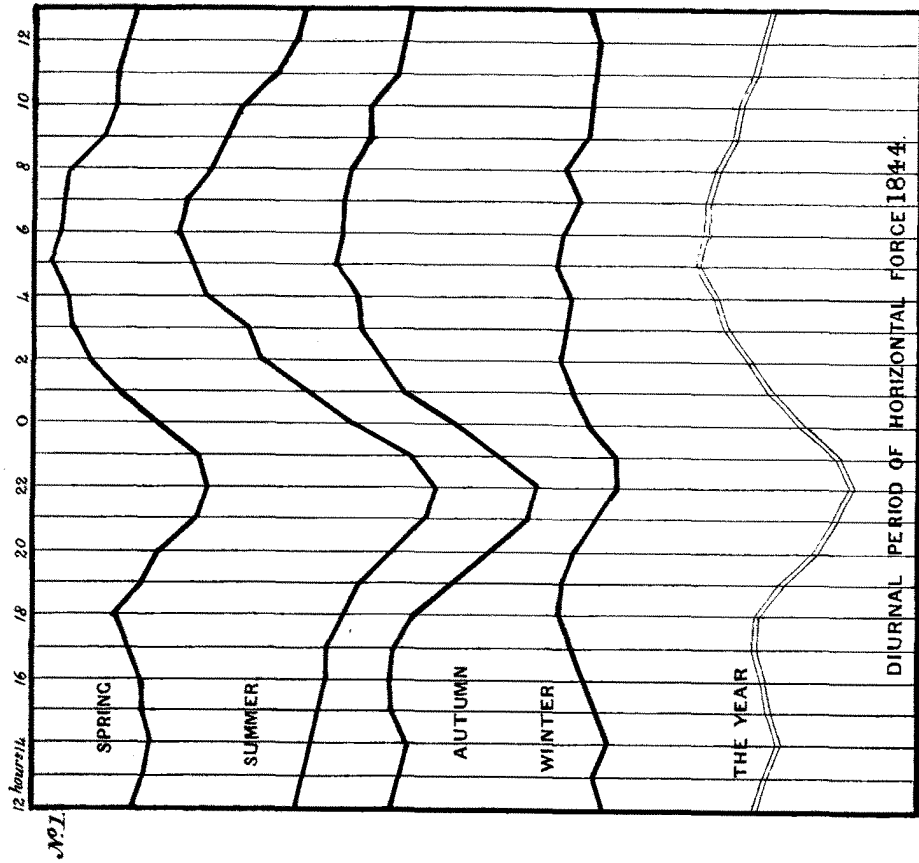
3. The horizontal force varies throughout the solar day, having, in that period, two maxima and two minima. The hours of the principal maximum and minimum were first pointed out by M. HANSTEEN, but I am not aware of the first determinations of the secondary points. In the years 1844 and 1845, observations were made at Makerstoun every hour excepting on Sundays. (See Curve, No. 1, Plate III.) From the means of the whole observations for the year 1844, the principal minimum occurs about 20^m past 10 A.M. (Makerstoun mean solar time is used throughout), or exactly when the sun is on the magnetic meridian of Makerstoun; the force then increases rapidly till between 3 and 4 P.M., when there is a slight inflexion; again it increases with its previous rapidity till about 5½ P.M., when the

maximum is attained. It now commences to decrease slowly till 8 P.M., more rapidly from 8 till 9, causing another inflexion in the curve, slowly again till 2^h 20^m A.M., when there is a minimum; the force then increases slightly till 5^h 30^m or 40^m when a maximum occurs, after which it diminishes rapidly till 10^h 20^m A.M., the period of *the* minimum. These hours differ somewhat from the periods obtained at other observatories; and while some part of these differences may be due to errors of temperature correction, I do not think that such errors will altogether account for them, but that the accurate periods of maxima and minima will be found to differ at different places. At Toronto in Canada, for example, *the* maximum occurred a little after 4 P.M. in 1842; and as the mean temperature of the magnet at the succeeding observation hour differs but little from that at 4^h, the period cannot be affected by temperature. Some observatories shew the maximum as late as 7 P.M. It does not, however, seem improbable, that the periods of maxima and minima should differ at different places, when it is known that these periods vary at the same place in the course of the year; at Makerstoun, in 1844, the afternoon maximum occurred as early as 3^h 10^m in December and January, and as late as 6^h 50^m in June; the minimum at 10^h 20^m A.M. in the winter months, and at 9^h 40^m A.M. in June; the A.M. maximum occurs at 6^h 40^m in December, and about 5^h in the summer months, while the earliest minimum occurs nearer midnight in winter than in summer. In this way the periods of the principal maximum and minimum approach to each other, and to noon in *winter*, and remove from each other, and from noon in summer. (See Curve, No. 1.) The reverse to some extent takes place with regard to the periods of the secondary maximum and minimum, which remove from each other in winter, and approach each other in *summer*, till in June the maximum and minimum seem to destroy each other.

The morning maximum is greater than the afternoon one in December;—in November, January, and February, they differ but little from each other; and in December, January, and February, the two minima are nearly equal.

4. The inflexions noted in the mean curve about 3 P.M. and 9 P.M., become minima in the winter months, so that there are then three or four maxima and minima; the smaller ones nearly compensate each other in the mean of the winter months, as they occur at different hours in each month.* I shall consider the cause of these secondary afternoon maxima and minima on another occasion. With regard to the 2 A.M. minimum and 6 A.M. maximum, these seem nearly to vanish in the summer months. In the means for the months of June and July they cannot be detected, excepting that the intensity decreases more rapidly after 6 A.M. than before it; it should not be concluded on this account that this maximum and minimum do not exist. Having projected the hourly observations made in each day of June and July, I have not found one day in ten on which the secondary maxi-

* November, December, and January, have been taken as the three winter months.



mum and minimum do not exist; the periods, however, are variable, and in this way they seem to balance each other in the mean; the maximum of one day occurring at the time of the minimum of another. (See Curve, No. 1.) It will be remarked also, with regard to the mean of the summer months, that the descending branch, after the 6 P.M. maximum, has a strongly marked concavity, indicating a tendency to a minimum which does not decidedly shew itself.

5. It has been shewn by Drs LAMONT and LLOYD, that the morning maximum seems to occur throughout the year a little before sunrise, and the afternoon maximum a little before sunset. I would mention another coincidence; the times of maximum atmospheric pressure and minimum intensity are the same throughout the year, and also the times of minimum atmospheric pressure and maximum intensity. There is also a secondary minimum of pressure occurring about the same hour as the secondary maximum of intensity, and the secondary maximum of pressure occurs about 10 P.M., the period of an inflexion in the intensity curve.

6. The diurnal range of intensity is least in January, and greatest in July, being 0.000641 in January (the whole horizontal force being unity), and 0.003396 in July, or five times greater in July than January; the mean diurnal range for the year is 0.002041, being almost exactly the mean of the ranges for January and July, and the same as the ranges for March and October. I have projected the mean range for each month in 1844, and also the mean altitude of the sun for each month. (See Curve, No. 2, Plate III.) There is a considerable similarity in the forms of the two curves; a marked inflexion occurs in June in the range curve. As June is remarkably free from irregularities, it seems to me probable that the range for June is the true diurnal range freed from those irregularities termed disturbances. If so, we may perhaps consider the deviation of the ranges for other months from the curve of altitudes as due to disturbances; in which case we might conclude that disturbances increased the diurnal range most in April and October, and least in February and June. This, however, requires other proof.

7. At Makerstoun, in 1844, each degree of the sun's altitude was equivalent to a diurnal range of about 0.00006 of the horizontal intensity.

8. The mean intensity at midnight and at 1^h P.M. are each equal to the mean of the year, and the means at these hours for each month differ very little from the mean of the month. This leads me to the monthly means, and their relation to the period of a year.

9. In June 1845, I shewed to the Physical Section of the British Association, that the Makerstoun observations of horizontal intensity for 1842 indicated well-marked double maxima and minima in the course of the year; and that as this result had been obtained from the means of only four observations in each day, I had determined the temperature correction approximately for the Toronto bifilar magnet, by the method already referred to, and applied this correction to

the monthly means, as deduced from the observations of horizontal force made at Toronto in 1842, at the following hours:—First, the hours corresponding to the Makerstoun hours; this could not be done exactly, as the observations were made at Toronto every two hours only, and for each of two of the Makerstoun hours, the mean of two of the Toronto hours had to be taken; thus, one of the observation hours at Makerstoun was 7 A.M.; the mean of the Toronto observations at 6 and 8 A.M. was taken as equivalent (see the open curve, No. 3, Plate III.) Second, from the whole two-hourly observations (see the open curve, No. 5, Plate IV.) The monthly means from both these methods, gave the same, or nearly the same, annual period as the Makerstoun observations.

10. I have since then verified this period by the monthly means of the observations made at Makerstoun in the years 1843, 1844, and 1845. I may first refer to the Makerstoun and Toronto curves for 1842, which were exhibited to the Physical Section of the British Association at Cambridge. (See Curves, No. 3, Plate III.) From these I concluded that the horizontal force of the earth's magnetism has an annual period, consisting of a maximum at each solstice, and a minimum at each equinox; both curves present a curious inflexion in March, which I then considered due to some irregularity. The monthly means for the years 1842, 3, 4, and 5, have also been projected together; (see Curves, No. 4, Plate III.) the whole speak the same language, excepting that the inflexion in March 1842 does not occur in the other years, unless it may be said to do so in 1845. All the curves shew a considerable *secular* change, the horizontal intensity increasing throughout the whole period. Whether this be really an increase of the earth's horizontal intensity, or due to some instrumental cause, cannot be positively stated; it is not at all likely to be due to an increase of the free magnetism of the bar, which is suspended at right angles to the magnetic meridian; the only apparent and possible cause is a stretch of the suspension wires; it must be a matter for consideration, whether such a cause is likely to operate for such a period, and to nearly an equal amount for two years. Whether a secular change of horizontal intensity, or due to a stretch of the wires, it is evident that it may be considered as a regular increase throughout the year. Upon this hypothesis I have eliminated this increase from the monthly means of the last three years, and projected the mean below the others. This curve shews more strikingly the annual period of solstitial maxima and equinoctial minima. The minima have nearly the same value; the summer maximum is greater than the winter maximum, but so little, that an error of a thirtieth in the amount of the temperature correction, would account for the difference. The annual range from the mean of the three years is 0.000724, or about the mean diurnal range for the three winter months.

11. M. HANSTEEN concluded from his observations, that there was a maximum of horizontal intensity in December, and a minimum in June. Colonel SABINE con-

cludes from the Toronto observations for 1842 (corrected by the usual method), that there is a maximum in June, and a minimum in December. I have projected the monthly means of the Toronto observations as corrected by the usual method. From these Colonel SABINE draws his conclusion. Under it I have projected the temperature of the magnet in a broken line, and below both, the means from the two-hourly observations, as corrected approximately by myself. (See Curves, No. 5, Plate IV.) These will shew how much depends on the accuracy of the correction in arriving at sound conclusions. I conceive that the consistency of the results at which I have arrived, independently of other considerations, will leave little doubt as to which method of obtaining the corrections should be adopted.

12. It has been already mentioned (10), that the apparent secular change consists of a considerable increase of horizontal intensity. Throughout the whole period, the rapidity of increase has been diminishing, and it is much less in 1845 than in any of the previous years. Of all the puzzling problems in terrestrial magnetism, that of connecting the secular change with some known or observed phenomenon has been the most difficult; any fact, therefore, tending to this, will have interest. One of the first questions which I proposed to myself, connected with it, was whether all hours of the day were equally affected by the secular change? In order to answer this more distinctly, the annual period was eliminated from the monthly means, or, which is nearly the same thing, the mean of each month was reduced to the straight line passing through January and December 1844.* I then found that the mean horizontal force in the first six months of the year 1844, was almost constant one hour *after* the period of the morning maximum, and also that it was almost constant for the last six months, one hour *before* the period of the evening maximum. When the diurnal curve for each month was projected, I found the curves for the first six months to pass through a space of 3-4ths of a scale division in the ordinate of 6^h 40^m A.M., with the exception of the curve for February, which is very irregular there. The curves for the last six months pass through a space of 1½ scale divisions, in the ordinate of 4^h 40^m P.M.; the increase of horizontal force from January till December was 18 scale divisions. I next eliminated all the larger disturbances from the monthly means of each hour, but this neither affected the periods of the nodes, nor the values of the ordinates in which they were contained. In this way, then, the horizontal force in its secular progress, seems to rest one foot during the first half of the year about an hour after

* The line should have been drawn through January 1844 and January 1845, but that there is an irregularity in the progress of the horizontal force from December 1844 to January 1845, compared with the previous years. I have, however, also reduced the means to the line passing through January 1844 and January 1845, and find the ordinate of the morning node slightly increased, but that for the evening node diminished.

the morning maximum, and extends the other forward at all other hours of the day, making the greatest strides at the time of the afternoon maximum. During the second half of the year, it rests the previously advancing foot about an hour before the evening maximum, and brings the lagging foot forward at the other hours, but with the greatest rapidity at the time of the principal minimum.

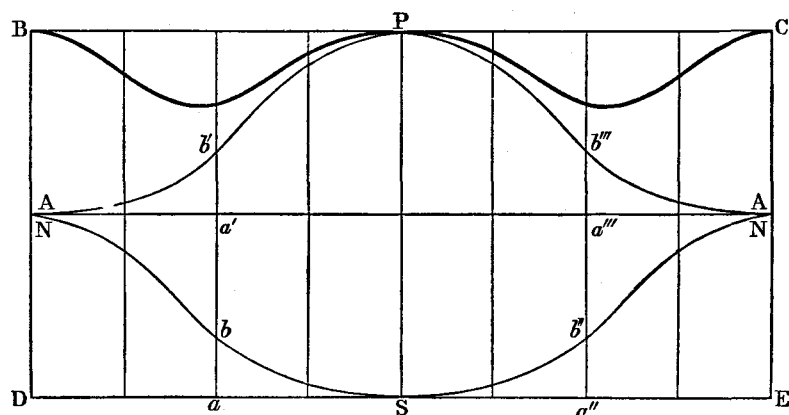
13. There is perhaps nothing more difficult in groping for the laws which regulate certain phenomena than the separation of the effects due to different causes ; but it is quite obvious that, before we can arrive at any sound conclusion as to simple laws, this must be done. In the determination of the diurnal period all the observations at each hour for a calendar month or year are summed, and the means taken ; in these summations are included several irregularities named disturbances ; if the disturbances occurred equally positive and negative at the same hour, or were equally distributed over the twenty-four hours, a large enough series of observations would serve to eliminate them ; neither of these suppositions seems to hold, and accordingly, certain hours in some months are more affected by disturbances than the same hour in other months, or than the other hours of the same month ; the diurnal curve, therefore, is complex. There are other causes, as will be seen afterwards, which render it more so.

14. In the attempt to determine whether the horizontal intensity varies with the moon's declination, the days were numbered from the day of the moon's greatest N. declination, counting that day 0 till it returned to the greatest N. declination again ; and, as 13 of the moon's revolutions, with regard to node, are equivalent to 12 lunations, and nearly to a year, the 13 revolutions, with regard to declination, were selected for summation ; as, by this means, any effect due to varying phase, or to annual period, would be eliminated. The mean intensity for each of the 13 days on which the moon had its greatest N. declination were then summed together ; the means for the 13 days numbered 1, in which the moon was moving south, and so on. For the purpose of verifying the result thus obtained, similar summations of the observations for 1845 were made ; in this case, however, only 12 revolutions with respect to declination, were obtained, so that any effect of phase will not be perfectly eliminated. No attempt has in either case been made to eliminate disturbances. The results of these summations were projected, having previously eliminated the effect of secular change. (See Curves, No. 6, Plate IV.) The curve, from the observations of 1844, indicates a maximum about 2 days after the moon has attained its greatest S. declination, and a maximum about a day after it has attained its greatest N. declination—the maxima have nearly equal values, so also have the minima. The branches ascending to and descending from the period of greatest S. declination are greatest ; so that the periods of minima are nearer, the greatest N. declination being about 5 days before and after it. The curve deduced from the observations of 1845 shews the maxima nearly at

the same periods as in 1844; but the branch ascending to and descending from the period of greatest N. declination is greatest, the periods of minima being nearer the greatest S. declination, namely, about 5 days before it and after it. The curve for 1845 is, however, more irregular after the S. declination maximum than in any part of the other curve. Besides the non-elimination of the effect connected with varying phase and disturbance, there is another possible cause of difference, namely, the varying distance of the moon; the period of perigee is about two days before the greatest S. declination in 1844, and two days after it in 1845. It should also be remembered that each point in these curves is a mean of only 12 or 13 days; as for the minor irregularities in the positions of the points, it is obvious that, as there are 27 days between the periods of the moon's greatest N. declination, if the full moon occurs on the day of greatest N. declination in one month, it will occur on the second day after the greatest N. declination on the next month, the fourth day on the next, and so on. It will be seen afterwards that this will cause a slight irregularity. It is on this account that I have projected the curves among the points, giving a preference to the mean positions of each two points.

15. The similarity of the positions of maxima and minima in these curves, having the moon's declination for abscissæ to the annual curve, or that having the sun's declination for abscissæ, is at once evident; by taking the mean of the two lunar curves, however, the cases will be identical, for then the moon's perigee will occur at the time of its greatest S. declination, and its apogee at the time of the greatest N. declination; this is the case with us for the sun. The resulting means have been projected below the other curves. By comparing the mean curves of No. 4 and No. 6, it is at once obvious that the facts are the same for both the sun and moon. I conceive, then, that I am justified in stating that the same relation exists for the moon as for the sun between the variations of the horizontal component of the earth's magnetic intensity, and the variations of declination and parallax.

16. We have, then, a law connected with two periods, namely, distance and declination. To which does it belong, or does it belong to both? It will take a few years' observations to determine this for the moon: it may be determined for the sun by observations for the annual period made in the Southern Hemisphere. Is there a maximum at the greatest N. declination, and also at the greatest S. declination; or have changes of declination no effect? and are the maxima due to the moon's or sun's distance solely? The supposition that at first sight seems most probable is, that these variations are due to both; that a maximum occurs at the time of perigee, a minimum at the apogee, a maximum at the greatest N. declination, and a minimum at the greatest S. declination. It may easily be shewn that two regular curves having these arguments, when superposed, would



produce two minima. Thus, if $A P A$ be the curve due to distance, $N S N$ that due to declination, the curve $B P C$, produced by the superposition of their ordinates will have two minima, if the sum of the ordinates $ab + a'b'$ be less than the sum $DN + AB$, and $a''b'' + a'''b'''$ be less than $EN + AC$.

The fact, that in both the solar and lunar curves the maxima are nearly equal, is against the supposition that both distance and declination are equally concerned, as it seems rather improbable that the effect of increasing distance should precisely counterbalance the effect of increasing N. declination. We have, however, much more singular cases of compensation in the motions of the heavenly bodies.

17. The range of the lunar declination curve for 1844 is 0.000455; for 1845, 0.000390; and, for the mean of both years, 0.000380.

18. I have already mentioned (14), that, by taking 13 revolutions of the moon, with respect to its declination, we eliminate any effect due to the varying phase of the moon. Similarly, if we take 12 lunations, and sum the mean intensity for the twelve days on which the moon was full, the twelve days on which it was one day old, and so on, we eliminate the effect of varying declination, and also the annual period very nearly. If, however, we may consider the intensity with respect to N. declination similar to that with respect to S. declination, it is evident that 6 lunations will be sufficient to eliminate the effect of declination. (See Curves, No. 7, Plate IV.) I have had the observations during the six summer lunations for 1844 summed by themselves, and also those during the six winter lunations; the mean intensities for both, for each day of the moon's age, have been projected, and also the mean for the year. All indicate a maximum of intensity about two days after the new moon, and a minimum perhaps two days after the full moon; the summer curve has an irregularity before full moon, and its range is only half that for the winter months. The minor irregularities may be accounted for in the same way as for the declination curve. The range for the

winter months is 0.001040. It appears to me, however, that it is exaggerated, owing to the curious fact, that the chief negative disturbances in 1844 occurred about the time of full moon.

19. It has not appeared to me necessary to verify this law by the result of another year's observations. Each of the winter months of 1844 shews the facts as completely as the mean; in the summer months, the result is not so evident. It would appear as if the effect of phase swallowed up the effect of declination in the winter, while the reverse occurred in summer. I have projected the means of the horizontal intensity for each day from January 4th till April 3d, 1844, including three synodical periods. (See Curves, No. 8, Plate IV.) In each period the curve shews the facts most completely; and the lunations in September, October, November, and December, shew them perhaps better. The periods of greatest N. and S. declination, and of the syzygies, are indicated on the curves, the open O being full moon. There are several curious facts, in connexion with the observations projected, which I cannot enter fully into at present; I may remark, however, the appearance of a weekly period. No observations being made on Sundays, breaks occur in the curve, where the intensity for these days should appear. A great disturbance spoils somewhat the form of the curve in March; the point belonging to the 29th of March would occur about $1\frac{1}{2}$ inches below the margin.

20. The law of the variation of the earth's horizontal intensity with the moon's phase, is one productive of many speculations. There is an evident connexion of the great diurnal variations of the horizontal intensity, with reference to the sun's hour angle; there is also a strongly marked connexion between the diurnal range and the sun's altitude; and we have a certain connexion between the sun's declination and the annual period. Are these connected with the heating power of the sun, its light, or its magnetism? Sir JOHN HERSCHEL has stated, that, as the sun's rays shine with their whole force on the moon's surface for a fortnight, unstopped by an atmosphere, the heat of the surface must be much more intense than that of a tropical summer; while, after the next fortnight, the cold must be more severe than that of a polar winter. M. COURNOT, the French translator of Sir JOHN HERSCHEL's *Treatise on Astronomy*, opposes this opinion, and argues that, as there is no atmosphere to prevent radiation, our knowledge of the laws of radiant heat would lead to the conclusion, that the temperature of the moon's surface would differ little at the times of new and full moon.* Supposing Sir JOHN HERSCHEL's opinion accurate, if we could conceive the moon as a magnetic body acting by induction on the earth, then, according to our knowledge of the effect of heat on magnetic bodies, its intensity would be greatest when it was coldest, and least when warmest: the period of greatest cold we should expect

* Quoted by M. FRANCOEUR; *Uranographie*, p. 97.

to be a day or two after the new moon, and of the greatest heat a day or two after full moon, in the same way as our periods of greatest cold and heat are after the winter and summer solstices. This seems to agree with the periods of maximum and minimum horizontal intensity. If M. COURNOT be right, or if Sir JOHN HERSCHEL's supposition be insufficient, then we must look to the solar emanations reflected or radiated from the moon for the causes of the variations of the earth's magnetism, and to our atmosphere for a cause of the supposed retardation of epoch.*

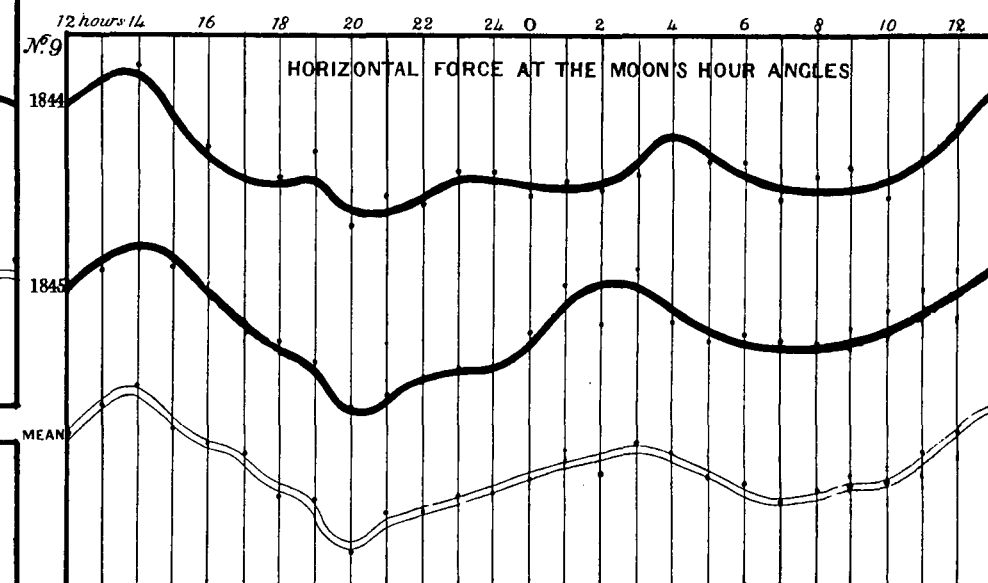
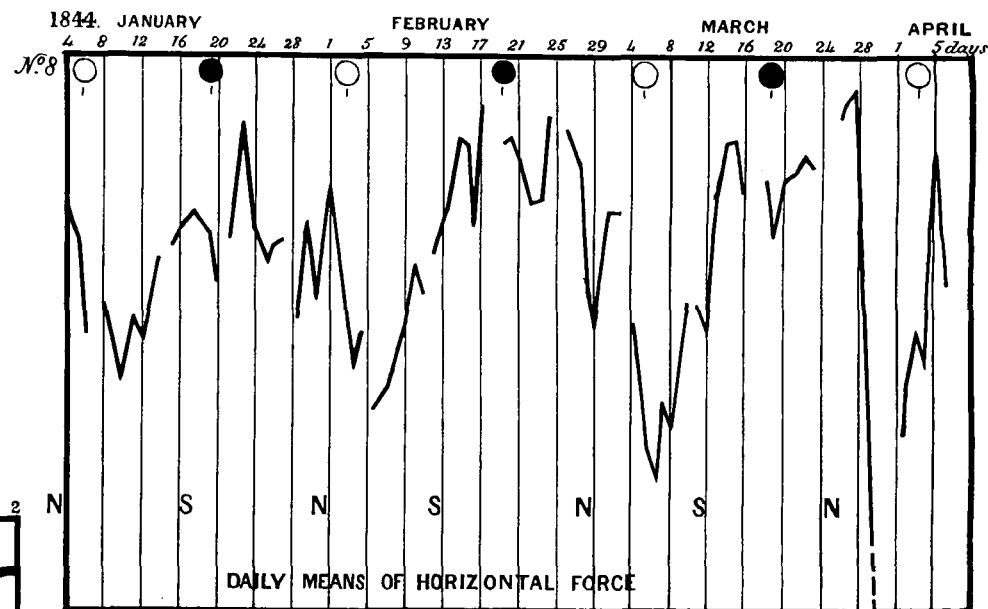
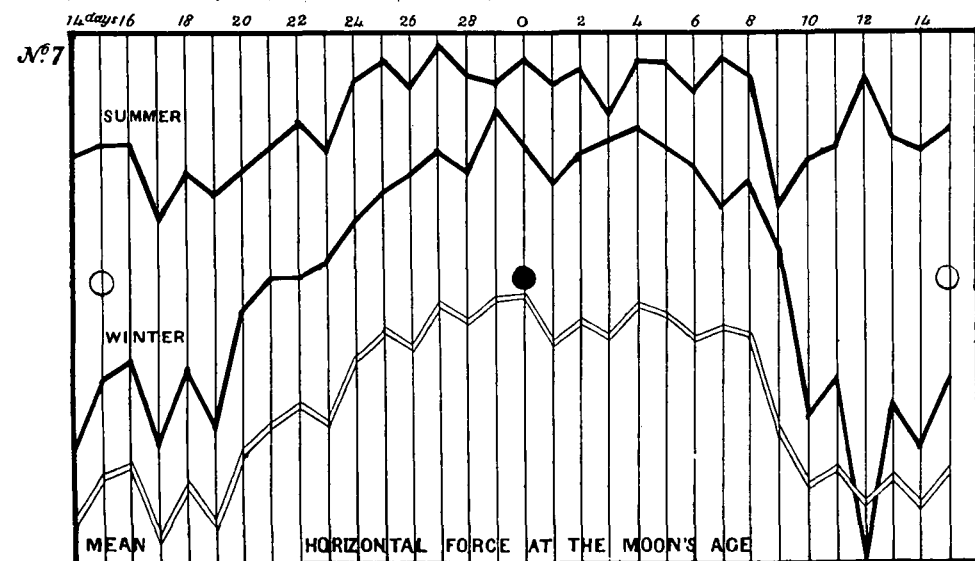
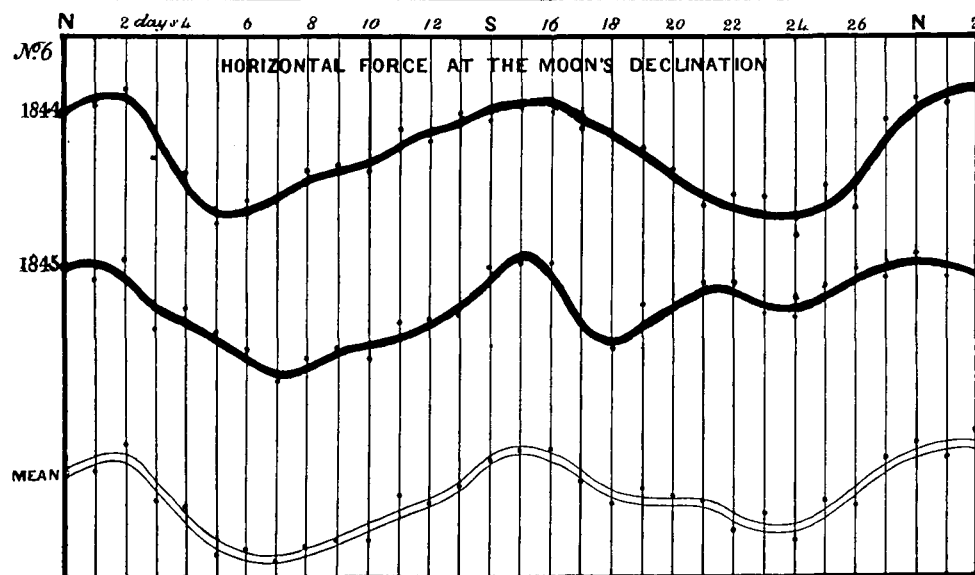
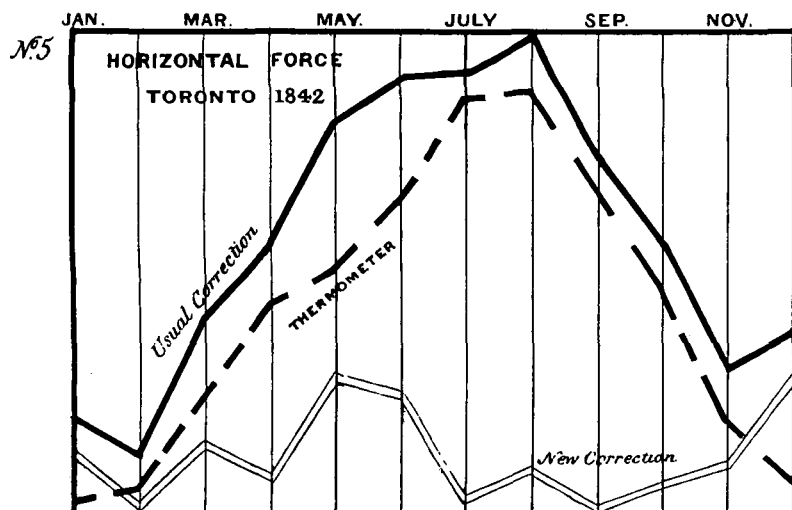
21. The connexion of lunar phase and horizontal intensity was first noticed by me in July 1845. I am not aware of any investigations on the relation of the horizontal intensity to the lunar month, excepting a paper by M. HANSTEEN, of which I have lately merely seen the title, which refers to the connexion of the horizontal intensity with the moon's ascending node.

22. Having mentioned some time ago to Professor FORBES, that I was engaged in examining the relation of the lunar periods to the variations of the earth's magnetism, I learned from him that M. KREILL of Prague had stated, in his volume of observations for 1842, that the horizontal intensity was greater at the moon's passage of the inferior meridian, than at its passage of the superior meridian. I know not whether M. KREILL has verified his statement, or to what extent his observations prove it.† I have now discussed the observations for 1844, with reference to this period, and have verified my results by a similar discussion of the observations for 1845. I shall, at present, merely state the leading facts, and leave the details to another communication.

23. The observation at the hours on which the moon was on the meridian were termed 0 hours, the observation the hour after one hour, and so up to 24; as the moon takes about 25 hours to return to the meridian again. On some occasions there were only 24 observations between the two passages; in these cases (few in number) the hour of passage was reckoned as 24 hours, and also as 0 hours of the next day. The summations for the hours were made for each month; I shall only speak of the means for the whole year in this communication; these means have been projected. The large disturbances have been eliminated from the summations for 1844 and 1845. (See Curves, No. 9, Plate IV.) Any observation in 1844 which shewed a difference from the monthly mean, for the hour at which

* It is evident that the variations of horizontal intensity may be due either to changes of the total intensity, or of its direction; any reasoning, therefore, on these facts must be necessarily incomplete, until we are certain of the actual effect.

† I have, since this was written, been favoured by Professor FORBES with a copy of M. KREILL's table for the horizontal force during the moon's hour angle. It indicates a minimum of intensity about two hours before the meridian passage, and maximum peaks at 12^h and 15^h, giving the interpolated period of maximum about 1½ hours after the inferior meridian passage; the latter period agrees completely with my own conclusion, the former differs about three hours from my result.



CURVES
to illustrate changes of
HORIZONTAL MAGNETIC FORCE
at
MAKERSTOUN

it was made, of above twenty scale division (twelve times the resulting range of horizontal force in the lunar hour angle curve), was rejected, and an interpolated quantity substituted; this elimination, however, was not found to affect the periods of maxima and minima; it reduced the range, and rendered the curve somewhat more regular. In the observations for 1845, as a farther check on these eliminations, a different test number was employed, namely, forty scale divisions, or nearly twenty-five times the resulting range of the lunar hour angle curve.

24. In both years, *the* minimum occurs at 20^h, or 5^h before the meridian passage; *the* maximum at 14^h, or about 1½^h after the inferior passage: in both years, *a* minimum occurs at 8^h; in 1844, *a* maximum occurs before 4^h; and in 1845, before 3^h. The maximum at 3^h, for 1845, differs little from the maximum at 14^h; but the maximum at 4^h, in 1844, is considerably less than the maximum at 14^h. The coincidences of these results may be considered extraordinary, when it is known that the range in 1844 is only 0·000211, and in 1845 only 0·000213, or less than the effect of one degree of Fahrenheit on the magnetism of the bifilar bar.

25. Several questions spring from this result of the connexion of the intensity with the moon's hour angle. Does the range of the lunar hour angle curve vary with the moon's declination? If so, then we do not eliminate the lunar effect from the solar day curve by a monthly or any other summation. Do the periods of maxima and minima vary throughout the lunar month in the lunar hour angle curve, as they do through the year in the solar day curve? These questions I shall endeavour to examine at another opportunity.

MAKERSTOUN, December 26. 1845.